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REVIEWS

Emmon Bach, Eloise Jelinek, Angelika Kratzer & Barbara H. Partee (eds.), *Quantification in natural languages* (Studies in Linguistics and Philosophy, **54**). Dordrecht: Kluwer Academic Publishers, 1995. Pp. ix + 756.

Reviewed by MOLLY DIESING, Cornell University

This book is a collection of 20 papers investigating the semantics of quantification. The papers, written by semanticists and non-semanticists with expertise in a wide array of languages, provide an in-depth look at a number of theoretical issues from a variety of viewpoints as well as offering a rich source of typological data. The contributions are arranged alphabetically by name of author rather than being thematically organized; in what follows I will discuss the papers in loose thematic groupings, rather than strictly following the order of presentation in the book.

In the introduction ('Introduction', 1–11) the editors of the book establish the background for the papers that follow, by laying out the basic questions addressed and presenting a brief history of the theory of quantification. This discussion leads to consideration of the syntactic loci of quantificational elements. Indeed, one of the leading ideas in the collection is the relationship between syntactic categories and quantification. This emerges in discussion of Barwise & Cooper's (1981) universal claiming that every language has noun phrases which express quantification (as generalized quantifiers). Bach et al. point out that the analysis of quantificational adverbs in terms of 'unselective binding' (as in Lewis 1975) opens up the possibility of a rather different approach to quantification, which in turn casts doubt on the validity of the Barwise & Cooper universal. Bach et al. thus distinguish two types of quantification: 'D(eterminer)-quantification', and 'A-quantification' – the latter being cases where the quantifier is any of a number of constructional elements such as adverbs, auxiliaries, affixes and 'argument structure adjusters'. For a number of the papers, this distinction serves as a starting point for investigation, while others deal with other areas of quantification theory, such as quantificational variability, domain restriction, compositionality and the strong-weak distinction. The introduction concludes with a brief overview of the volume.

Since Barbara Partee ('Quantificational structures and compositionality', 541–601) takes a closer look at the theoretical issues raised in Chapter 1 and how they apply to a number of the languages discussed by the other contributors, this paper should perhaps be read before tackling the rest. Partee elaborates on the concepts of A- vs. D-quantification and pursues the utility of 'tripartite structures' (consisting of a quantifier, a restriction and a

scope) as a unifying generalization when dealing with issues of compositionality. That is, given Heim's (1982) analysis of D-quantifiers like *every* and *most* as analogous to A-quantifiers such as *always* and *often*, the possibility arises that the A- vs. D-quantification should both be given parallel semantic analyses. Partee's paper not only applies this idea to a number of languages, including data from American Sign Language, Salish and Warlpiri, but also considers the relevance of the tripartite structure to the topic-focus articulation.

One of the most interesting themes running through the book concerns languages in which quantification is exclusively of the adverbial type, contradicting the Barwise & Cooper universal. Several authors address the possibility of deriving the lack of inherently quantificational NPs from another major typological characteristic of many of these languages, the fact that they are PRONOMINAL ARGUMENT LANGUAGES in the sense of Jelinek (1984). In his contribution to the volume, Mark Baker ('On the absence of certain quantifiers in Mohawk', 21–58) very convincingly argues that the lack of quantificational NPs in Mohawk is due to the fact that full NPs in pronominal argument languages must (as adjuncts) be associated with the pronominal argument positions by pronominal reference, which rules out the possibility of non-referential (or quantificational) full NPs. Leonard Faltz ('Towards a typology of natural logic', 271–319), in an analysis of Navajo and Lakhota, makes a similar claim, though his explanation is couched in terms of the necessary semantics involved in the relationship between full NP adjuncts and the rest of the sentence. Eloise Jelinek ('Quantification in Straits Salish', 487–540) departs from these views and claims that the lack of D-quantification in Salish results from the fact that the function of D-quantification (which she takes to be to 'limit the scope of a quantifier to a constituent in a particular argument position', 536) is incompatible with the fact that NPs in Salish cannot appear in argument positions. Maria Damaso Vieira ('The expression of quantificational notions in Asurini do Trocará: evidence against the universality of determiner quantification', 701–720) provides additional counterexemplification to the Barwise & Cooper universal from Asurini do Trocará, a language from the Tupi-Guarani family spoken in Brazil. Vieira links the lack of determiner quantification in Asurini to its status as a pronominal argument language, following Jelinek's explanation. While there is clearly more work to be done on the relationship between the presence or lack of D-quantification and the typological characteristics of pronominal argument languages, these papers not only provide a foundation from which to proceed, but also an excellent overview of quantification in four rather different languages.

Other papers investigate the forms that both A- and D-quantification can take in different languages. Emmon Bach ('A note on quantification and blankets in Haisla', 13–20) investigates quantification in Haisla, an agglutinating language, and finds in Haisla word syntax and semantics some

striking parallels to sentential syntax and semantics in other languages. In particular, Bach notes a distinction parallel to the A-/D-quantification classification; affixes correspond to adverbial quantification, while determiner-like meanings (e.g. generalized quantifiers) are confined to roots and stems. Maria Bittner ('Quantification in Eskimo: a challenge for compositional semantics', 59–80) also takes up the task of analyzing quantification in a heavily polysynthetic language within the constraints of compositionality. In addition to outlining an approach to the compositional challenges posed by the syntax of Greenlandic Eskimo, Bittner demonstrates that Greenlandic Eskimo has both determiner and adverbial quantification, with both types of quantification displaying a variant in which a verbal suffix acts as a scope marker delineating the scope of the quantifier in question.

Other issues discussed in the volume include the representation of (in)definiteness and plurality. The paper by Maria Bittner & Ken Hale ('Remarks on definiteness in Warlpiri', 81–105) examines Warlpiri, a language with a robust noun/verb distinction, but no syntactic determiner category. Quantificational notions are instead expressed by nominals, which exhibit systematic ambiguities between weak (indefinite), strong (definite), and predicative interpretations. Interestingly, this three-way ambiguity extends even to cardinality expressions in Warlpiri, in contrast to other determinerless languages. Bittner & Hale propose an explanation in terms of constraints on the type-shifting mechanisms responsible for the ambiguity, demonstrating that a restricted set of universal principles of semantic interpretation can be productively applied to a typologically diverse range of languages. Karen Petronio ('Bare noun phrases, verbs and quantification in ASL', 603–618) examines the determinants of plurality in American Sign Language, which has no morphological manifestation of the singular/plural distinction. She suggests an analysis in which noun phrases are treated as variables that can refer to either singular or plural entities, as determined by context (the properties of the verb in the sentence) and/or pragmatic considerations.

Several papers in the volume focus on the lexical semantics of both D- and A-quantifiers. The first of these, by Nick Evans ('A-quantifiers and scope in Mayali', 207–270), provides an in-depth look at the properties of adverbial quantifiers in Mayali (spoken in Australia), revealing a wealth of interesting facts concerning lexical differences in what the various adverbial quantifiers quantify over. That is, A-quantification is not completely unselective, but is constrained by what Evans calls the 'extra semantics' of particular quantifiers, with some quantifiers being limited to quantifying over subjects, others to patients, and still others having roughly absolutive scope. Though the question of how best to represent these effects is not definitively answered (Is 'extra semantics' involved, or pragmatic implicatures of some kind?), the paper provides a good deal of food for thought. David Gil ('Universal quantifiers and distributivity', 321–362) takes a cross-linguistic look at

universal quantification, claiming that quantifiers like *every* are actually composed of a universal and a distributive component, and subsequently, that the non-distributive universal *all* is a less marked form. Data from a large number of languages is brought to bear on this claim, but the absence of full translations revealing the scopal contrasts being illustrated makes the data hard to assimilate. Martin Haspelmath ('Diachronic sources of "all" and "every"', 363–382) also investigates the differences in meaning between the D-quantifiers *all* and *every*, demonstrating that (cross-linguistically) the collective property of *all* frequently arises from its diachronic relationship to the concept of "whole". *Every*, on the other hand, quite commonly has as its historical antecedent a free-choice indefinite determiner or a distributive preposition, accounting for its distributive properties.

The strong/weak distinction in determiner quantifiers and its role in the 'definiteness effect' is addressed in papers by Ileana Comorovski ('On quantifier strength and partitive noun phrases', 145–177) and Helen de Hoop ('On the characterization of the weak-strong distinction', 421–450). Comorovski investigates a number of cases where NPs with strong determiners in English (in particular, partitives) are permitted in existential sentences and concludes that the definiteness effect is determined by both novelty with respect to the discourse and the strength of the determiner. This account is then combined with a claim that existential sentences are in fact ambiguous between a purely existential and a presentational reading to yield a full explanation of the restrictions on *there*-sentences in English. The puzzle of why partitives are sometimes allowed in existential sentences also arises in de Hoop's examination of the properties of the Dutch determiners *sommige* and *enkele*, often described as strong and weak forms of "some". De Hoop distinguishes between strong and weak determiners (attributable to purely semantic properties) and strong and weak readings for NPs (a function of the syntactic environments in which the NPs appear, mediated by Case assignment). The contrasts between English and Dutch with regard to partitive NPs in existential sentences thereby reduce to a difference in the Case assignment mechanisms available.

Gennaro Chierchia ('The variability of impersonal subjects', 107–143) takes up the issue of QUANTIFICATIONAL VARIABILITY in impersonal *si* constructions in Italian. The puzzle here is that *si* behaves like an indefinite when it restricts a quantifier, but appears more pronoun-like when it is in the scope of a quantificational structure. Chierchia provides a neat account for this mixed behavior within a modified version of discourse representation theory (indefinite NPs are treated as quantifiers which can be subject to 'disclosure' – their existential quantifiers are 'erased' to free the variable introduced by the NP) by analyzing *si* as a pronominal indefinite with a distinguished index.

Quantificational variability is also a central concern in Veneeta Dayal's contribution ('Quantification in correlatives', 179–205). Examining the

behavior of Hindi correlative constructions, Dayal questions the pure unselective binding account of quantificational variability, and suggests instead an analysis in which adverbs of quantification bind situations rather than individuals. Dayal also provides an account of negative polarity items in correlatives and draws comparisons between Hindi correlatives and English free relatives.

A final group of papers includes those with little cross-linguistic emphasis, but which nonetheless deal with issues that are essential to an understanding of natural language quantification. Pauline Jacobson ('On the quantificational force of English free relatives', 451–486) analyzes free relatives as having the syntactic structure of a *wh*-clause to which a syntactic category-changing rule applies to yield an NP. The variability in meaning seen in free relatives (definite vs. universal readings) is represented by means of a semantic type-shifting rule which transforms the predicative free relative NP to an individual-denoting expression which picks out the MAXIMAL PLURAL ENTITY composed of all the atomic individuals with the property denoted by the *wh*-expression contained in the free relative. This use of the notion of maximal plural entity yields both the definite and universal properties of free relatives.

James Higginbotham's paper ('Mass and count quantifiers', 383–419) presents a unified analysis of mass and count determiners within a Boolean algebra augmented by a notion of MEASURE, which is needed to express the meanings of mass quantifiers like *much* and *little*. The parallelism between mass and count quantification is shown to extend to the LF syntax, with the major difference between mass and count NPs being that the former must be nominalizations of the predicates they contain (this restriction also yields a source for the 'homogeneity constraint' on predicates which can appear with mass noun subjects).

Paul Portner ('Quantification, events, and gerunds', 619–659) examines the quantificational variability of gerunds, focusing on parallels to the behavior of bare plurals. Departing from earlier accounts which take a predicational approach, Portner argues for an analysis in which gerunds receive their quantificational force from the contexts in which they appear, accounting for the similarities to bare plurals. Where gerunds differ from bare plurals (i.e. in allowing only a definite interpretation in subject position), Portner explains the contrast by analyzing the gerunds as topics, which are presupposed, and thus incompatible with an existential interpretation.

Craige Roberts ('Domain restriction in dynamic semantics', 661–700) focuses on the issue of what goes into the restrictive clause and how it gets there. Roberts demonstrates (using modal subordination data) that domain restriction cannot be wholly structurally determined; certain non-linguistic aspects of the context must also come into play. She argues that the relevant notion of context is the common ground of the discourse participants, departing from more anaphoric accounts of context change.

In sum, this collection covers a wide spectrum of quantification-related topics, both theoretical and typological. While the book's size and scope may seem daunting to the reader, semanticists and non-semanticists alike will find a great deal of valuable information and useful insights in its pages.

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William H. Edmondson & Ronnie B. Wilbur (eds.), *International Review of Sign Linguistics*, vol. 1. Mahwah, NJ: Lawrence Erlbaum Associates, 1996. Pp. viii + 268.

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This volume represents the first of what is planned to be a yearly series. *The Review* is the successor to the short-lived *International Journal of Sign Linguistics* and as such, its title is slightly misleading, since it is not designed to describe the 'state-of-the-art' in sign linguistics research but rather to serve as an alternative journal. Nevertheless, the *Review* more than adequately meets the demands of its title, in particular by providing a forum for the publication of research on different sign languages. This is a much-needed complement to the large body of research on American Sign Language (ASL). Not only do the chapters provide a rare opportunity to read about several lesser-researched sign languages, but also to explore similarities and differences among sign languages, and between signed and spoken languages. Indeed, one of the major contributions of sign language research to linguistic theory is to illuminate the extent of the contribution of channel (visual or auditory modality) to linguistic structure and hence to the description of linguistic universals. The seven chapters in this volume also cover a wide range of linguistic topics and perspectives. As well as chapters on phonology (Sandler), morphology (Stavans), syntax (Wilbur, Tenny

Boster) and socio-linguistics (Fischer), two chapters (Ebbinghaus & Hessman, Johnston) directly address channel and modality issues.

Sandler ('Representing handshapes'), primarily using ASL data, proposes a model for the representation of handshapes. The model integrates feature geometry theory and dependency theory. Although these theories appear incompatible, she adopts the notion of articulator-based, hierarchically structured feature classes from feature geometry theory and that of unary phonological primitives from dependency theory. The model proposed predicts a universal markedness hierarchy which can be tested against data from languages other than ASL.

Stavans ('One, two or more: the expression of number in Israeli Sign Language') is a more exploratory study, describing the use of quantifiers, morphosyntactic agreement and lexical semantic features in a relatively young sign language. The findings, as the author herself points out, are preliminary and relatively atheoretical. There is some cause of concern, however, at the data collection technique and at some of the reported findings. Data sets were constructed by presenting Hebrew sentences to hearing bilingual informants, who translated the sentences into Israeli Sign Language. The author assumes that if any patterns generated in this way were 'agrammatical, illogical or non-existent, both participants and informers (sic) would reject, misunderstand or repair the pattern' (99). However, examples drawn from spontaneous data, or from data elicited without translation (e.g. from pictures) would have been preferable for such an initial study. In the circumstances, Stavans spends a considerable amount of time discussing such puzzling constructions as TREE TWO-WIND FALL (two winds knocked down a tree) as against WIND TWO TREE-FALL (a wind knocked down two trees).

Wilbur and Tenny Boster both contribute chapters on ASL syntax. Tenny Boster's chapter ('On the quantifier-NP split in ASL and the structure of quantified NPs') concerns numerical quantifier phrases which are split from their associated NP (BOOK I WANT THREE vs. *THREE I WANT BOOK). She argues for an analysis of the NP-NQ split in ASL as an instance of A′-movement by the NP complement of a QP, the maximal projection of a quantifier head, and suggests that this analysis can also account for Q-float and partitive constructions in English.

Wilbur's chapter ('Evidence for the function and structure of WH-clefts in ASL') rejects existing descriptions of rhetorical questions, a structure which features prominently in sign language courses in Britain as well as in the US, and which has always, somewhat oddly, been presented as forming one element of a 3-way question system, consisting of 'wh-', 'y/n-' and 'rh-' questions. Wilbur argues for a description of this structure as a focusing *wh*-cleft structure, containing two operators: one to focus on the highlighted material and the other serving to prepose non-focused material to [spec,CP]. There is considerable debate in the literature about *wh*-movement in ASL

(Lillo-Martin 1990; Aarons, Bahan, Kegl & Neidle 1992; Dubuisson, Miller & Pinsonneault 1994) and Wilbur's analyses will contribute further to this topic.

Fischer ('By the numbers: language-internal evidence for creolization') discusses numeral system data from ASL and LSF (Langue des Signes Française) its supposed ancestor, and concludes that the modern ASL number system represents a creolization of indigenous American (hearing) gesture, early American signing, and LSF, as brought to the USA in the early nineteenth century. In particular, she provides data which support Mühlhäusler's (1986) claim that the traces left by rapid creolization are distinct from those left by gradual contacts between full languages.

The two remaining chapters, Ebbinghaus & Hessman ('Signs and words: accounting for spoken language elements in German Sign Language') and Johnston ('Function and medium in the forms of linguistic expression found in a sign language') directly address issues of modality. Ebbinghaus & Hessman describe the role of mouth movements in sign language. While this topic has interested many European sign language researchers (Vogt-Svendsen 1983, Schroeder 1985, Schermer 1990), mouth movements derived from spoken words have been largely ignored by ASL researchers, or dismissed as reflecting code-mixing by bilinguals. Many of Ebbinghaus & Hessman's observations are very illuminating, in particular the linking of mouthing of words with referential expressions, and the absence of mouthing with predicative expressions. However, their conclusion, that 'spoken languages are made up of words, whereas a sign language … consists of elements of more than one type' (53), is at the least a misrepresentation of spoken language.

Johnston, describing Australian Sign Language (Auslan) takes very much the opposite view, saying that research on sign languages should re-emphasize for linguists working on spoken languages that simultaneity in establishing grammatical function is *not* unique to sign languages, and that differences may be of degree rather than of kind. He suggests that the real contrast is not in simultaneity as against sequentiality, but in the grammatical exploitation of space by sign languages. His discussion is enhanced and made highly accessible by excellent transcription and illustrations.

This highlights an issue of considerable concern. The absence of any agreed transcription system has had two unfortunate results. The first is that every chapter in this volume represents information about signs in different ways, which are often inaccessible to the non-specialist reader. Even more seriously, the use of glosses, in which signs are represented by English words, gives the reader no real indication of either surface or underlying form. As a result, even readers fluent in a given sign language may not be able to reconstruct examples from the description given. It is to be hoped that future volumes of the *Review*, and other journals publishing sign language data, will either make examples available through electronic media, or will promote a

consistent and accurate transcription system for sign language data. Even with these concerns, the integration of sign language research into the mainstream of linguistics is a development which is greatly to be welcomed.

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Lyn Frazier & Charles Clifton, Jr., *Construal*. Cambridge, MA: MIT Press, 1996. Pp. x + 230.

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In retrospect it is surprising how long the Garden-Path model of sentence processing (e.g. Frazier 1978, Frazier & Rayner 1982, and subsequent work) was able to maintain its dominant position without introducing significant changes. To be sure there were additions and modifications along the way, but the basic picture remained the same: the initial stage of sentence processing (first-pass analysis) proceeded incrementally (roughly, word-by-word structuring of the input), serially (no parallel processing of ambiguous input), and in accordance with the parsing strategies of Minimal Attachment and Late Closure. Minimal Attachment states that, given a structural ambiguity, the attachment which results in the minimal increase in the complexity of the syntactic representation is preferred. Late Closure states that the most local attachment is preferred. When conflicts arise, modulo certain length effects, Minimal Attachment has priority. Both Minimal

Attachment and Late Closure, as well as De Vincenzi's (1991) Minimal Chain Principle, are argued to follow from more general principles of information processing, such as the need to reduce the burden on short-term memory resources by efficiently structuring the input. As Frazier & Clifton state, '… all three principles may be seen as consequences of a deeper principle …' (9). This principle is 'Choose the first available analysis' (8). The minimal, or more-local, analysis will generally be the one which is computed most efficiently.

The Garden-Path model is a modular theory of sentence processing in that structure-based strategies have priority over (potentially-disambiguating) semantic and pragmatic information. Given prior work within the Garden-Path model, perhaps the most surprising experimental results reported by Frazier & Clifton involve the failure to find initial structure-based attachment preferences for a number of different types of ambiguous structures.

The basic goal of *Construal* is to argue that these ambiguity types constitute a theoretically-interesting class (called 'nonprimary phrases and relations') which, once distinguished, permit the structure-based strategies to be retained as important properties of the human sentence processing mechanism (although their domain of application is reduced). For example, the traditional interpretation of Late Closure predicts that in a sentence such as (1), the preferred attachment of the relative clause should be to *the colonel*, rather than to *the daughter*.

(1) John saw the daughter of the colonel who was laughing.

This is because at the point in the parse when the relative clause is processed, the NP *the colonel* is the more local attachment site (Minimal Attachment is inapplicable, given the lack of a complexity difference). However, using Spanish stimuli, Cuetos & Mitchell (1988) report a preference for attachment to the less-local NP. In English, as Frazier & Clifton (Chapter 2) discuss, there is no evidence for a purely structure-based preference. They note that, 'The assumption that relative clauses are parsed according to the predictions of general structural parsing strategies (e.g. Late Closure) does not account for the behavior of relative clauses with complex heads' (31). In response to this, Frazier & Clifton introduce the Construal Hypothesis, given in (2).

(2) (a) *Associate* a relative clause to the *current thematic processing domain* – the (extended) maximal projection of the last theta assigner.
(b) *Interpret* the relative clause with any grammatically permissible material in the associated domain using structural and semantic/pragmatic information.

To ASSOCIATE a phrase means to connect it to a REGION of the phrase marker, rather than to uniquely attach it to a specific node. The region of association is the Current Thematic Processing Domain, defined in (2a). The concept of an extended maximal projection is due to Grimshaw (1993). Roughly,

extended projections are functional projections for specific lexical categories. For example, DP is an extended maximal projection of NP, and CP and IP are extended maximal projections of VP. Simplifying somewhat, verbs are theta assigners, so, at most, the Current Thematic Processing Domain will be the current clause (CP or IP). A noun or preposition may or may not be a theta assigner. For example, in (1) only the verb *see* is considered a theta assigner, so the Current Thematic Processing Domain is the entire clause (the extended verbal projection), and neither NP is predicted to be (structurally) preferred over the other as a potential head of the relative clause. But non-structural factors (e.g. type of modification, properties of the phrase being modified, the availability of unambiguous alternative structures, etc.) will invariably be present to affect interpretive preferences within the structurally-defined Current Thematic Processing Domain.

In contrast to *of* in (1), the preposition *with* in (3) is a theta assigner and here the Current Thematic Processing Domain for the relative clause is more local, the PP. This predicts that the NP within this PP will be the preferred head of the relative clause. Chapter 3 presents experimental results consistent with these predictions.

(3) John saw the man with the colonel who was laughing.

But the definition of the Current Thematic Processing Domain incorrectly predicts that there should be no structure-based preference for which NP the relative clause modifies in (4). That is, the Current Thematic Processing Domain is the entire clause as the verb is the only theta assigner.

(4) The boy saw the girl who was laughing.

Frazier & Clifton (Chapter 5) attribute the preference for object modification to the Minimal Chain Principle (De Vincenzi 1991), i.e., given Frazier & Clifton's syntactic assumptions, the extraposed relative reading requires a two-member chain (connecting the relative clause to its pre-extraposition site) whereas the preferred reading requires only a singleton chain. But it is unclear why the processing of relative clauses should be subject to the structure-based Minimal Chain Principle but not the structure-based principle of Late Closure, especially as both are argued to stem from the common need to reduce short-term memory burden. For example, Inoue & Fodor (1995: 35) unify Minimal Attachment, Late Closure and Minimal Chain Principle effects under the 'general least effort principle' Minimal Everything. A more-detailed discussion of this issue by Frazier & Clifton would have helped the reader sort out why the Minimal Chain Principle, but not Late Closure, applies to relative-clause ambiguities (and how it interacts with non-structural information in sentences such as *The man gave an inspiring speech who was running for reelection*).

Frazier & Clifton do present a general characterization of which types of ambiguities are resolved by Minimal Attachment and Late Closure, and which are resolved by association and construal. The Construal Principle,

which generalizes the approach to relative clauses in (2), is given in (5). New input is either a primary phrase (subject to attachment) or a nonprimary phrase (subject to association). Primary relations are defined in (6).

(5) *Construal Principle*
Associate a phrase XP that cannot be analyzed as instantiating a primary relation into the Current Thematic Processing Domain.

(6) Primary phrases and relations include:
(a) the subject and main predicate of a clause;
(b) complements and obligatory constituents of primary phrases.

Thus NPs and PPs are subject to attachment, not association, because they can be analyzed as instantiating a primary relation (although it may turn out that they do not). Relative clauses, adverbials, and secondary predicates are examples of nonprimary relations. Note that the Construal Principle in (5) assigns the same processing domain to all nonprimary phrases. One implication of this is that the Current Thematic Processing Domain for a particular item may not contain an appropriate host. Consider the sentences in (7).

(7) (a) John hit the friend of the girl quickly.
(b) John hit the friend with the girl quickly.

In (7) Frazier & Clifton assume that *friend* is a theta-role assigner so the Current Thematic Processing Domain for the adverbial in (7a) is the NP headed by *friend*. Here the VP is outside the Current Thematic Processing Domain. In (7b) both *friend* and *with* are theta assigners, so the VP is TWO Current Thematic Processing Domains removed from the adverbial. Frazier & Clifton mention (166) an eye-movement study suggesting that VP attachment of *quickly* is more difficult in (7b) than (7a). They account for the comparative ease of VP association in (7a) by appeal to a distinction between DEPENDENT and INDEPENDENT Current Thematic Processing Domains. One Current Thematic Processing Domain is dependent on another if the theta-assigning element is itself the recipient of a theta role. For example, in (7a) *friend* establishes a Current Thematic Processing Domain but also receives a theta-role from the verb. Therefore, the nominal Current Thematic Processing Domain is dependent on the verbal Current Thematic Processing Domain. Given this 'association of *quickly* to the VP should not be very problematic' (166). One issue that remains to be investigated is whether or not there is increased processing load for an association outside the Current Thematic Processing Domain, even if it is thematically dependent upon the required association site. For example, how does VP association of the adverbial in (8), where the only Current Thematic Processing Domain is the entire clause, compare to VP association in (7a) and (7b)?

(8) John hit the daughter of the colonel quickly.

A related question concerns what role the Current Thematic Processing

Domain plays in the processing of primary phrases, such as the locative PP in (9a). That is, when *into the box* is processed, the Current Thematic Processing Domain is the PP headed by *with*. If this domain is restricted to nonprimary phrases, then the primary locative phrase can be directly (minimally) attached to the VP (recall that Minimal Attachment has priority over Late Closure). Further, if the Current Thematic Processing Domain is not relevant to the processing of primary phrases, then Frazier & Clifton avoid the counter-intuitive prediction that, once any independent processing differences between the complex NPs is factored out, associating *into the box* with *put* is easier in (9b) than (9a).

(9) (a) John put the jars with mushrooms into the box.
(b) John put the jars of mushrooms into the box.

However, it appears that Frazier & Clifton intend the Current Thematic Processing Domain to apply to primary phrases as well. This is clearly seen in their discussion of length effects (Chapter 8, Section 4.3). Consider the processing of the locative phrase *in the library* in (10).

(10) Though Susan put the book that John was reading in the library …

Frazier & Clifton argue that the Current Thematic Processing Domain can explain the intuition that *in the library* is initially connected to the *reading* clause rather than attached as an argument of *put*. They state that

> Though in principle *in the library* may be analyzed as a legitimate primary phrase (an argument of *put*), by hypothesis *put* is not visible to the syntactic processor because it lies outside the current thematic processing domain (the extended projection of *read*). The processor may initially attempt to analyze *in the library* as an argument of *read*, but that will fail since *read* does not take a locative argument. At this point the syntactic processor has no visible alternative but to analyze *in the library* as instantiating a nonprimary relation, an adjunct of *read*. (167)

Although not required by the definitions given above, this passage interprets the Current Thematic Processing Domain as applying to primary, in addition to nonprimary, phrases. This is the explanation for the initial processing of *in the library* as part of the more-recent clause. The fact that there is a stronger locality effect (defined in terms of Current Thematic Processing Domains) in (10) than (7a) is due to the independent status of the verbal Current Thematic Processing Domain in (10). That is, because verbs are never theta-role recipients, clauses will always be independent Current Thematic Processing Domains.

But thematic prepositions too will always establish independent Current Thematic Processing Domains, so the *with* PP in (9a) presumably creates as strong (i.e. impermeable) a domain as the clause in (10). Intuitively this is not the case and further work is required to determine the various factors which contribute to the differential strength of locality effects with respect to theta-

theoretic domains (e.g. a complement clause assigned a theta role must still function as an independent processing domain).

It should be clear that these specific issues stem from the strength of the Construal Framework. It is a well-argued proposal which expands the range of the Garden-Path model to include a number of syntactic structures and ambiguities never before experimentally tested or subjected to theoretical investigation. Although the focus in this short review has been on relative clauses and adverbial phrases, Frazier & Clifton also present experimental results involving adverbial clauses, adjunct extraction structures, and adjunct predicates. As research proceeds, there is little doubt that specific problems for the initial hypothesis will give rise to new research questions. But, as the discussion above demonstrates, it is the theoretical proposals themselves which draw attention to the problems. Frazier & Clifton have opened up an important new area of research in human sentence processing, one which will be of interest to both linguists and psychologists – or anyone who recognizes the importance of exploring the relation between knowledge of language and the process of sentence comprehension.

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Hubert Haider, Susan Olsen & Sten Vikner (eds.), *Studies in comparative Germanic syntax*. Dordrecht: Kluwer Academic Publishers, 1995. Pp. v + 344.

Reviewed by CHARLOTTE REINHOLTZ, University of Manitoba, Winnipeg

The volume under review comprises 13 papers that were prepared for the Seventh Workshop on Comparative Germanic Syntax held in November

1991 at the University of Stuttgart. The emphasis is on comparative syntax, which the editors take to mean that at least two languages should be analyzed, and at least one of the languages should be Germanic. Though not a formal requirement, all papers in the volume are written in the framework of Principles and Parameters Theory. NP licensing and (phrasal) movement are important themes in the volume but there are also papers which touch on the structure of IP and DP.

The editors, Hubert Haider, Susan Olsen and Sten Vikner, provide an introduction (1–45) which offers their own overview of the field of comparative Germanic syntax (1–31) followed by a survey of individual papers (32–45). Adopting the editors' organization, individual papers in the volume are reviewed by topic area: 1) Clause structure, 2) NP licensing, 3) movement and 4) DP-structure.

Two contributions in the volume touch on clause structure. In a short paper/squib entitled 'Agreement and verb morphology in three varieties of English', Richard Kayne discusses number as a functional category in three varieties of English. Kayne argues that English has inflection for number but not for person and adopts the notion of a functional category Num(ber) whose role in three varieties of English is considered. The paper, which is very condensed, offers an interesting discussion of contentful Num marking and expletive number agreement in Newfoundland English that develops an earlier suggestion by Paddock (1990). Also worth highlighting is Kayne's analysis of number agreement with *wh*-moved phrases in non-standard (American) English which involves *wh*-movement through a NumP-adjoined position. Kayne supports the analysis with evidence from floated quantifiers and suggests that the absence of this phenomenon in standard English is due to the presence of a zero plural suffix and a requirement that Num always be spelled out in standard English.

Manuela Schönenberger and Zvi Penner's paper 'Cross-dialectal variation in Swiss German' is not concerned with clause structure per se, but rather with structures which allow syntactic movement yet disallow LF movement. At issue is the restricted distribution of scope-bearing elements in Verb Projection Raising (VPR) constructions with Doubling Verbs (DVs), which the authors take to reflect a failure of LF movement. In Bernese, any constituent can undergo syntactic scrambling out of a Doubling Verb Phrase (DVP) before raising applies to create a VPR construction. Scope-bearing elements (which must raise at LF) occur freely in DVPs with *cho* 'come' and *aafe* 'begin', but DVPs with *ga* 'go' disallow scope-bearing elements except those which are universally quantified. The authors seek to explain this specialized restriction in terms of feature enrichment at LF. DVs have the potential to erect a minimality barrier in the sense of Baker (1988). This has no effect in the syntax because non-distinctness of the two verbs in a DVP eliminates barrierhood. The authors propose that the non-finite verb in a DVP with *ga* is enriched by a feature [+Scope] in LF, causing the two verbs

in a DVP with *ga* to be distinct at LF. This gives rise to barrierhood and a resultant failure of scope-bearing elements to undergo grammatical LF movement out of a DVP with *ga*. This proposal is motivated by entailment effects which differentiate the DV *ga* from other DVs. To explain why universally quantified NPs can circumvent the LF barrierhood of a DVP with *ga*, the authors show that these can move out of DVP via adjunction and propose that they can also do so at LF. The paper finishes with a discussion of St. Galler German where cross-doubling verbs present a problem for further research.

Five papers in the volume address topics in NP licensing. In 'Structural Case, specifier-head relations, and the Case of predicate NPs', Joan Maling and Rex Sprouse argue that predicate NPs in Germanic languages are licensed under structural case assignment. Germanic languages can be divided into two groups according to how this requirement is satisfied: 1) predicate NPs may be licensed under (accusative) case government by the copula (in English, Frisian, Danish and Norwegian), 2) other languages (Icelandic, Swedish and German), allow case features from a higher (typically nominative) case assigner to penetrate into the VP containing the predicate NP. The argumentation for structural case licensing of predicate NPs builds narrowly on the need to account for variations in the morphological case marking of predicate NPs. A case-less treatment of predicate NPs fails to explain such variation for obvious reasons. A default case treatment of predicate NPs cannot explain why Icelandic predicate NPs show systematic variation in morphological case-marking. Finally, the authors argue that a subject-predicate agreement treatment cannot derive the full range of morphological case variation shown by predicate NPs in Icelandic. The structural case assignment account which the authors develop for Icelandic predicate NPs adopts earlier proposals by Sigurðsson (1989/1991) and Lee (1992) that Icelandic predicate NPs receive case from I^0 via a 'Structural Case Path' as long as these are not contained within a more immediate case path. Aux is not a case assigner in Icelandic, and this means that the structural case path of I^0 includes the predicate NP in VP. This explains why predicate NPs in Icelandic regularly turn up with nominative case instead of the accusative case pattern which predominates in e.g. Danish and English predicate NPs. The account does not explain the occurrence of accusative predicate NPs embedded under ECM verbs, but the authors present convincing evidence that this is a result of an alternative mechanism of lexical case marking.

Case is also a central topic in Tarald Taraldsen's paper 'On agreement and nominative objects in Icelandic', which breaks new ground by proposing a distinction between Case licensing and Case identification. The paper discusses Icelandic constructions where the Case of the subject is oblique, while the object is nominative. In these constructions, the finite verb agrees in number with the nominative object but invariably displays third person

inflection. Taraldsen proposes that the feature content of Agr_S, the subject Agreement Phrase, is divided between two autonomous functional heads, Agr_P (person agreement), and Agr_N (number agreement). He goes on to propose that Case-licensing may be viewed as having two different components, one relating to the licensing of a Case-feature per se, the other to the identification of the specific value a Case-feature may assume. The distinction calls to mind earlier proposals to a similar effect in the Pro-drop literature and certain versions of the ECP. In addition to providing an account of partial agreement with nominative objects, Taraldsen explores a range of consequences, looking among other things at oblique subject raising and (very interestingly) the licensing of PRO.

In 'To have to be dative', Teun Hoekstra proposes that the range of double object constructions found cross-linguistically can be traced back to a limited variation with regard to the (sometimes empty) preposition which governs the indirect object in a double object construction. Hoekstra argues for a distinction between HAVE-type and BE-type possessive constructions which is extended to double object constructions. Hoekstra posits a (possibly empty) preposition in double object constructions, and proposes two parameters affecting the preposition (P) which determine the range of 'possessor' constructions where at least four different interpretations need to be distinguished: recipient, result benefactive, affective benefactive and inalienable possession. The parameters proposed for P are: 1) whether P is overt or empty, 2) whether an empty P assigns its own Case or is merely a case transmitter. The second parameter determines how passivization of the verb affects indirect objects. In English, empty P is a case transmitter, and therefore has no case to transmit when the verb is passivized. Hence the need for indirect object promotion to the Nominative case position when the verb in a double object construction is passivized. In German, empty P assigns its own Dative case, and this is seen to explain why indirect objects cannot be passivized in German double object constructions. The paper, which is very condensed, touches on many important questions relating, directly and indirectly, to the analysis of double object constructions.

Ad Neeleman's paper, 'Complex predicates in Dutch and English', is not so much concerned with Case as with the analysis of constructions like *paint the door green*. Neeleman argues, contra the standardly assumed small clause (SC) analysis, that such constructions should be analyzed as having a complex predicate (CPr). CPrs arise from 'base-generated adjunction of a predicate to a verb', yielding a complex verb (*paint green*) whose theta-grid is derived by theta-role percolation. In arguing for a CPr analysis, Neeleman points to Dutch cases of nominalization, preposition stranding and topicalization which are more readily handled under a CPr analysis. He also points to differences between resultative and *consider* type CPrs which are explained under the analysis he proposes. Finally, Neeleman argues that English constructions like *paint the door green* can also be analyzed as CPrs

in which the non-verbal predicate has been extraposed for reasons to do with Case.

In 'Pronouns, anaphors and Case', Eric Reuland & Tanya Reinhart discuss the anaphoric systems in four West-Germanic languages (English, Dutch, Frisian and German), each of which poses problems to the standard Binding Theory. Elaborating the theory of reflexive binding proposed in Reinhart & Reuland (1993), the authors provide an account of the four different anaphoric systems which distinguishes binding effects from chain effects. The authors make a three-way distinction between SELF-anaphors (e.g. Dutch *zichzelf*), SE-anaphors (e.g. Dutch *zich*), and pronouns. SE-anaphors pattern with SELF-anaphors in being referentially dependent, but they pattern with pronouns in having no reflexivizing function. The distribution of the three elements is determined by two conditions on reflexivization of predicates. Condition A states that a syntactic predicate which is *reflexive-marked* (i.e., lexically reflexive or having a SELF-anaphor as one of its arguments) is *reflexive*, meaning that two of its arguments must be coindexed. This condition allows the occurrence of logophoric SELF-anaphors in non-argument position of a syntactic predicate while still ruling out logophoric SELF-anaphors in argument position. Condition B states that a reflexive semantic predicate (one with two coindexed arguments) is reflexive-marked. This condition rules out core cases of what are known as Principle B violations in the standard binding theory, but allows a pronoun to be bound in its own clause when the pronoun is not itself a syntactic argument of the verb (e.g. a pronoun in a locative PP). In Dutch, a SE-anaphor in the argument position of a reflexive predicate is ruled out by Condition B for the same reason as a pronoun (neither has a reflexivizing function) unless the verb is lexically reflexive. To explain why a SELF-anaphor is not permitted as the object of a verb which is (unambiguously) a lexical reflexive, e.g. Dutch *gedragen* 'behave', the authors suggest that double-marking (lexical reflexivity plus a SELF-anaphor) is precluded for reasons of economy. In addition to this framework, the authors propose a chain condition whereby only the head of a chain can be fully specified for grammatical features, including Case. With this condition in hand, the distribution of bound pronouns in the four West-Germanic languages is shown to follow from differences in their Case systems.

Five papers in the volume deal with movement. Helen de Hoop & Wim Kosmeijer's paper, 'Case and scrambling: D-Structure versus S-structure', presents an account of Dutch scrambling which appeals to Case-licensing. In Dutch, NPs with strong readings can scramble, but NPs with weak readings cannot. The authors offer evidence that scrambling involves A-movement and argue for a distinction between A- versus A-bar movement in terms of S-structure licensing. The landing site for A-movement is licensed at S-structure, and the landing-site for A-bar movement is not. Citing the well known strong/weak readings of Turkish D-objects (and similar effects in

other languages), the authors hypothesize that NPs receive strong readings if they occupy an S-structure position in which strong Case is licensed. NPs bearing strong Case can move, because movement takes them to a position licensed at S-structure. Weak NPs, which are not licensed at S-structure, cannot undergo scrambling, because movement takes them to a position where they are not licensed at S-structure, giving rise to additional barriers which cause subjacency violations and interfere with antecedent government.

Ian Roberts' paper, 'Object movement and verb movement in Early Modern English' addresses pronominal object shift. The point of take-off is pronominal object shift in Mainland Scandinavian which is shown, encapsulating earlier works by Holmberg and by Vikner, to involve Case-motivated A-movement triggered by movement of the main verb. Roberts shows that Early Modern English possessed the same type of movement, and that the failure of modern English pronouns to show the same type of object shift is directly related to the failure, in modern English, of main verbs to move out of VP. Roberts proposes an account of pronominal object shift where pronouns in North Germanic are required to check for phi-features with AgrO, where AgrO's strong features are induced by verb-movement. Pronouns in Early Modern English and Modern English are subject to the same licensing condition, but modern English main verbs (almost) never raise to AgrO and do not trigger object shift.

In his paper, 'On the origin of sentential arguments in German and Bengali', Joseph Bayer argues for an argument shift analysis of sentential arguments appearing on the right side of the verb in OV languages like German and Bengali. Bayer shows that German extraposed (post-verbal) clauses are not A-bar-adjoined, and proposes that extraposed clauses are adjoined to IP. The finite verb raises to I^0, which governs and licenses the IP-adjoined position as an A-position. Finally, Bayer presents evidence concerning LF-derived scope in Bengali which supports an extension of his analysis to this language.

Gereon Müller's paper, 'Crossover effects, chain formation, and unambiguous binding', addresses strong crossover and improper movement. Müller shows that both strong crossover effects and cases of improper movement arise with non-Case-driven movement. This similarity is seen to call for a unified treatment. Rizzi's (1986) explanation of strong crossover, which involves a local binding constraint on chain formation, is rejected on the grounds that it is too strong (it rules out Case-driven movement across a coindexed item). Furthermore Rizzi's treatment does not generalize to cover improper movement. Müller instead argues for a unified treatment which appeals to Müller & Sternefeld's (1993) Principle of Unambiguous Binding, which was originally proposed as a constraint against improper movement.

In 'Preposition stranding and resumptivity in West Germanic', Jarich Hoekstra presents evidence for preposition stranding by an empty resumptive

pronoun strategy in Frisian and German. The paper begins with a discussion of Frisian constructions which appear to show DP extraction from PP. Assuming that prepositions are not proper head-governors, this type of extraction is unexpected in a version of the ECP which incorporates head-government, and it is in fact prohibited in other West-Germanic languages (Dutch and German) which only allow so called R-pronouns to be extracted from PP. Hoekstra offers evidence showing that the problematic Frisian cases do not involve preposition stranding, but rather an (empty) resumptive pronoun strategy. The idea that Frisian allows preposition stranding by a resumptive pronoun strategy as well as by movement is exploited to explain differences between Frisian and Dutch, the latter having no resumptive pronoun strategy to facilitate stranding. German is also claimed to have a resumptive pronoun strategy, but differs from Frisian with regard to preposition stranding. Hoekstra proposes that this is for reasons to do with an independently motivated subparametrization of resumptivity.

The single paper in the volume which addresses DP-structure is Giuliana Giusti's paper 'A unified structural representation of (abstract) Case and article', which boldly argues that the article is a syntactic means of expressing case. Giusti implements that proposal by analyzing the functional projection containing NPs, not as a Determiner Phrase (DP), but rather as a Functional Projection (FP), whose head F^0 is reserved for nominal case and articles. Quantifiers appear above FP in QP and select for an FP, whose head may either be empty or incorporated into Q. Germanic possessives are claimed to have adjectival status (like Italian possessives), and Giusti proposes that their determiner-like behaviour derives from raising to Spec-FP. Why adjectives do not behave like determiners is not clear, however. Finally demonstratives appear in Spec-FP. The author proposes a historical development whereby Germanic languages originally (i.e. in Proto-Indo-European) had case morphology in F^0 which subsequently became reinterpreted as autonomous articles, while case morphology on the noun was reanalyzed as agreement and either retained (in German) or dropped (e.g. in English). The FP analysis is also exploited to provide an account of strong and weak adjectival agreement depending on whether or not the FP projection is morphologically realized.

The volume contains a list of contributors (329–330), a language index (331–332), a name index (333–336), and a subject index (337–344). Endnotes and a bibliography are provided after each paper.

The volume contributes significantly to the field of comparative Germanic syntax with original, often controversial papers addressing a diverse range of topics. A highly condensed exposition makes for strenuous reading in a few papers, but the contents make it well worth the effort. As a source of theoretical inspiration as well as detailed information on many aspects of Germanic syntax, the volume is commended to all who take an interest in the field of comparative Germanic syntax.

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Peter Harder, *Functional semantics: a theory of meaning, structure and tense in English*. Berlin & New York: Mouton de Gruyter, 1995. Pp. ix + 586.

Reviewed by Alex Klinge, The Copenhagen Business School

The pivotal role of the verb in clause meaning and the intricacies of the grammatical categories of the verb phrase continue to inspire and tantalize linguists. Within one year, we saw the publication of two substantial and comprehensive works on tense by Binnick (1991) and Declerck (1991), and now a third heavy volume by Harder joins the fray. In terms of pages Harder even outnumbers his otherwise substantial predecessors.

Harder takes his reader through two 'foundational discussions', of linguistic meaning and of linguistic structure, both from a functional perspective. This paves the way for a merger of the two in the ensuing description of tense. He explicitly recognises two major sources of influence, Simon Dik and Ronald W. Langacker. In addition to modified versions of the functionalism and cognitivism of those two important scholars, Harder's frame of reference is cast in what may safely be called a 'European neo-structuralist' mould. The book is organised in three parts: 'Part One: Meaning', 'Part Two: Structure', and 'Part Three: Tense'.

In 'Part One', Harder traces the history of the philosophy of meaning from classical philosophy, where the meaning of language reflects the ontology of the world, to the development through rationalist and empiricist thought, where the meaning of language more or less is what the world is like, and to cognitivism, where the meaning of language is a reflection of how

we mentally organize what the world is like. At the beginning of 'Part One' Harder makes the opening moves for a book-long quarrel with the approach to meaning that grew out of the descriptive practices associated with unitary science. The seemingly powerful tool of mathematical formulae representing 'true science' had a natural home in philosophical logic as a mediating link between the world and the language of description, avoiding the messiness of natural language. The formulae, however, also sneaked into linguistic thinking in the form of frameworks for semantic representations in logical form. The availability of the formulae of logic led to an aspiration of precision where internally consistent systems of representation of meaning could be formulated. As a consequence, Harder argues, theories of meaning became self-contained, addressing model worlds, and became impervious to the communicative contexts in which natural language has its natural habitat.

Harder rightly points out that we can still detect the consequences of logical philosophy turned linguistics in the form of trench lines between syntax, semantics and pragmatics in a somewhat marred landscape of linguistics. But perhaps more importantly, Harder brings home the point that we should not mistake language for logic, and that we should not try to impose on language a perfect match with a conceived reality. Later in the book Harder returns to a case in point, the analysis of tense as against an ontology of time.

In 'Part One' Harder also considers the role of the mind in the equation of language and meaning. He argues against two extreme manifestations of post-war cognitivism: the misguided metaphor of linking mental processes with the computational processes of machines; and, at the other extreme, where the world is all cognition, what he refers to as 'pan-cognitivism'. The computer-mind metaphor adds a dimension to the static representations of logic, namely a set of rule-governed computations specifying the relation between input in the form of symbols and output in the form of symbols. This of course offers an alluring parallel: the brain takes some language input, exposes it to some computational operations, and produces output in the form of meaning. Harder points to generative grammar as the most important offspring based on such intellectual foundations. The computer view of cognition lends itself to a formal pattern of description, and according to Harder this is also its fundamental limitation. The formal patterns are without content in the sense that they do not stand for anything outside themselves, and in essence we lose from view the 'aboutness'-relation and the participants that are crucial elements in understanding natural language.

The other extreme version of cognitivism Harder opposes is 'pan-cognitivism', where non-cognitive entities are eliminated. Harder points to the obvious problem that 'if all you can talk about is what is inside your own head, all sciences are theories of mental content: astronomy is not about

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stars, and economy is not about the wealth of nations – they only deal with the way the scientist conceives of these things' (57). Such a position again leaves us with a severed 'aboutness'-relation.

Harder's own preferred framework is that of a functional approach, where meaning is not merely a static relationship between a linguistic expression and some representation of it, but where meaning is an aspect of communicative interaction, and where linguistic expressions owe their raison d'etre to the (literally) meaningful function they fulfil in the ongoing interaction between the sum of individuals in a speech community. According to Harder, the meaning of an expression is to be understood as its potential contribution to the communicative function of utterances (101). In some instances this contribution is hardly describable from the perspective of cognition, but calls for a functional explanation. Harder mentions the case of greetings such as 'Hello!', which are better explained in terms of function, but he also points to the declarative sentence mood of English, which does not represent conceptual meaning, but which presupposes conceptual meaning: 'the declarative, as a paradigmatic alternative to the interrogative indicates that the content of the sentence is to be understood as describing what is the case' (106).

This leads us to what I see as Harder's most central leitmotif in 'Part One'. Much linguistic writing assigning a central role to cognition ends up equating linguistic meaning and mental models. This misses the point that linguistic meaning actually works by triggering the building of mental models rather than representing them. We need the dynamics of a procedural semantics where language codes process input in the form of instructions to the addressee enabling her to produce representations. Harder's semantics is an instructional semantics; linguistic meaning constitutes instructions to an addressee about how to make sense.

In 'Part Two' Harder discusses linguistic structure from a functional perspective. A discussion of linguistic structure can hardly avoid addressing the foundations of transformational syntax. Being of functional persuasion, Harder dismisses as misguided a programme based on the notion of autonomous syntax. The perceived autonomy, he argues, is to be found in the formal metalanguage, not in the object of description, natural language. Harder blames both Hjelmslev's glossematics and Chomsky's syntax for 'confusing the autonomy of the calculus with the autonomy of the structure that was inherent in the object' (191). Another feature of transformational syntax which is of doubtful descriptive status is the notion of underlying structures. How is it possible to constrain 'underlyingness' by any means other than descriptive convenience?

In his structuralist views Harder comes down on the side of Saussure in a slightly modified version. Saussure's position on linguistic arbitrariness and autonomy was too strong, and as a consequence it missed the point that language is a product of the context in which it serves interactional purposes

and that it should be studied as such. Linguistic structure is motivated. Harder also invokes a range of other Saussurean basics. First of all, it is valid to distinguish by way of abstraction between langue and parole to find 'the pattern that must exist if we are not to understand linguistic communication as totally random and spontaneous activity' (165). To the extent that communication is an ordered activity it will also impose order on its medium, language. Since communication is concerned with conveying messages, the functional pressures are on the meaning potential of language, so what needs to become ordered is the meaning content. The structure of linguistic form simply serves to structure meaning content. Harder's line of argumentation naturally leads to Saussure's dyadic linguistic sign as manifested in grammatical structure: we need to distinguish 'expression syntax' from 'content syntax' (193). Harder thus coins a new collocation, 'content syntax', which forms one of the most important constructs of his theoretical foundation.

One of the prime tasks of the two interdependent syntactic structures is to order scope relations in the meaning content. Syntactic structure orders the layered structure of clauses in terms of scope relations between operands and operators, thus providing the compositionality input to the addressee's sense-making operation. This should be seen in conjunction with Harder's instructional semantics, where compositionality then turns out to be 'in the process rather than in the received message' (222).

At the end of 'Part Two', 310 pages into the book, the reader has been taken through the foundational discussions of meaning and structure. Harder built his own theoretical scaffolding out of conclusions reached in the many quarrels he picked on the way. At least to this reviewer Harder's procedural, instructional semantics holds promising perspectives, so it is unfortunate that the thrust of the first two parts lies more in polemics than in careful crafting of Harder's own theory. This may also help to explain why anyone looking for empirical underpinning of Harder's position on meaning and structure is in for a painstaking search. Depending a bit on the way you choose to count, up to page 310 only some 50 illustrative examples stand out from the running text. In other words the reader has to keep track of a descriptive apparatus unfolding over 300 pages with an average of more than six pages between data worthy of being singled out. I hasten to add that the dearth of data does not necessarily reflect on the quality of the argument, but it does tax the reader.

The stage is now set for 'Part Three', the analysis of tense. Harder's argument develops from the meanings of the tenses themselves as constituted by monosemantic instructions, to the compositional meaning of tenses and their clause environments. He begins by exposing two pre-theoretical fallacies of traditional grammar. The first fallacy is the assumption that the categories of Latin grammar are the primitives in any tense system. The second fallacy is the assumption that tense corresponds to a mental

organisation of the ontological nature of time as a line divided by a present point in time into past time and future time. The caveats are then followed by a brief review of some of the most influential treatments of tense by Jespersen, Reichenbach, Bull, Allen and Comrie. He ends this general introduction by noting that there is disagreement as to whether the category of tense has two or three primary members, the future tense being the odd one out. The rift tends to follow the dividing line between formally oriented, defending the two-primary-tenses case, and functionally oriented linguists, defending the three-primary-tenses case.

Being functionally oriented himself, Harder takes a broad view of tense in English. Following an established typology, Harder distinguishes between primary, deictic tenses, viz. past and present, and secondary, relational tenses, viz. future and perfect, which depend for their semantic livelihood on the primary tenses. In a traditional analysis the possible combinations of primary and secondary tenses result in a paradigm of eight oppositions in the tense category, with for instance the past tense being in an equally-ranking opposition to the future perfect, the past perfect and five other tenses. This is where the descriptive framework developed in 'Part One' and 'Part Two' begins to make a real descriptive contribution.

The result of assuming a broad category of tense is a paradigm of eight oppositions, which is not very satisfactory, because clearly the opposition between the past and the present is somehow more pertinent than the opposition between the future and the past perfect. If instead of the traditional eight static representations we adopt a scope-ordering content syntax and expression syntax together with a procedural, instructional semantics, we can neatly account for both the meaning and the form of the eight tenses while retaining sets of binary oppositions. The entire tense paradigm begins with a fundamental deictic opposition between the past and the present tense, creating two sub-paradigms in which the semantic instruction conveyed by the past and present becomes an operator in whose scope we may choose the semantic instruction of the future, arriving at a composite instruction, and finally in the scope of the previous choices we may choose the semantic instruction of the perfect, creating an even more complex composite instruction. Each choice of additional semantic content is matched by an increasingly complex expression syntax. In terms of content syntax the present/past opposition has the widest scope, and it is correspondingly marked first in the expression; second in the scope hierarchy of content and expression is the future, and third is the perfect. The full structure turns out as (1), which has three sub-paradigms of binary oppositions, yielding the eight traditional tense forms:

(1) 'past'/'present' (+/−'future' (+/−'perfect' (state-of-affairs)))

This structure is describable in terms of its content syntax. If we let SoA (State of Affairs) stand for a propositional nucleus, S (speech time) stand for

meaning of the present tense, P (past time) for the meaning of the past tense, F (future time) for the meaning of the future, and A (anterior time) for the meaning of the perfect, the content syntax, ie. the scope relations, for the eight possible forms works out as:

(2)

simple present:	S(SoA)
simple past:	P(SoA)
present future:	S(F(SoA))
past future:	P(F(SoA))
present perfect:	S(A(Soa))
past perfect:	P(A(SoA))
present future perfect:	S(F(A(SoA)))
past future perfect:	P(F(A(SoA)))

On the principles of instructional semantics, the tense marking of the clause 'He will have played', with the structure S(F(A(played(he)))), would be analysed as follows (see 397): the present tense is an operator instructing the addressee to apply the meaning content to the time of speech, the operand is F(A(played(he))), where F is an operator instructing the addressee to look ahead from the application time already established and apply the operand A(played(he)), which has the operator A instructing the addressee to apply the operand (played(he)) to a time anterior to the time ahead of speech time. In other words, the present future perfect tells the addressee that relative to the time of speech there is a time ahead relative to which 'he' engaged in the activity of 'playing'. From the perspective of the interpreter, compositionality lies in the process of carrying out the semantic instructions.

It is impossible here to examine the many aspects of Harder's tense semantics, but one point seems particularly worthy of note. As it appears from (2) above, in Harder's tense category, there are only two deictic tenses; both the future and the perfect are relational tenses which depend on the present and the past tense for basis times. The past future has always been part of the stock-in-trade of a traditional grammarian. Harder goes to considerable lengths to argue that the past tense version of the future has its counterpart in the present tense version, so that present versus past is retained as the fundamental opposition, resulting in a past future and a present future. In so doing Harder adopts an untenable position between those who take 'will' to be just another modal, such as Lyons (1977) and those who take (one) 'will' to be a fully-fledged deictic tense marker, such as Davidsen-Nielsen (1988), who is frequently invoked in this context. In the heat of the argument, Harder seems to lose sight of some structural regularities which favour the former position, for instance in the lengthy discussion, so often found in works on tense in English, as to why temporal 'will' seems to be excluded from conditional sub-clauses. In works on modality this is simply accounted for by noting that subjective epistemic modality is simply not available as an interpretation in that position – in this

context the so-called future tense 'will' behaves just like any other epistemically used modal. Nevertheless, there is little doubt that anyone contemplating an argument in favour of a future tense in English is ill-advised to overlook Harder's discussion of the deictic status of the future.

In the process of reading and reviewing this book I found myself agreeing with Harder in many places of 'foundational' import. However, I also agree with him when in the Introduction (3) he writes 'in terms of the current division of scholarly labour I have tried to cover too much.' In the first two parts Harder simply casts his net too wide. The result is that at least to my taste the first half of the project appears rather polemic and speculative, and it offers little support by way of data. The situation improves significantly as we move into 'Part Three', the tense analysis.

In assessing the contribution this book can make to an already crowded space of scholarly thinking, I reach a dual conclusion: 'not a lot' and 'quite a lot'. As Harder himself admits (499), his contribution does not make new empirical observations. However, readers who stick it out to the tense analysis and beyond will be rewarded with a new, smoother and much more plausible analysis of a broad tense category in English. Harder's structural approach offers new ways of taking the categories of the verb into constituent parts for further detailed analysis.

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Peter Lasersohn, *Plurality, conjunction and events*. Dordrecht: Kluwer Academic Publishers, 1995. Pp. xi + 304.
Barry Schein, *Plurals and events*. Cambridge, MA: MIT Press, 1993. Pp. xiii + 384.

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Both of these books are concerned with the proper semantic treatment of plurals and both strongly advocate the use of (Neo-)Davidsonian event semantics. Both authors reject the individual view of plurals (i.e. that plural

objects exist and are of the same general sorts as singular individuals, see Link 1983). However, despite these similarities, their concerns and theoretical solutions differ greatly. Lasersohn is concerned principally with the provision of a unified account of sentential and noun phrase conjunction that adequately handles collective predication. He takes plural (and conjoined) noun phrases to denote groups. Schein, on the other hand, is concerned with quantification and its analysis within a particular version of event semantics involving the 'essential separation' of thematic roles from predicates. For him, plurals are predicates and plurality involves second order quantification over these predicates.

Lasersohn develops his theoretical position over the whole length of the book, but begins with a discussion of two methodological principles, Strong Compositionality and the Weak LF Hypothesis, which crucially inform the succeeding arguments. As he points out, any strongly compositional semantics (where the semantic value of a complex expression is determined as a function of the semantic values of its component parts) is vitiated where there are no, or only very weak, constraints on the relationship between surface syntax and any level of logical form (LF). A serious commitment to strong compositionality thus leads directly the assumption that LFs must be minimal, that is close or identical to surface syntactic structures. Hence, he adopts as his principal methodological strategy that preference will be given to an analysis 'which is compatible with a more restrictive theory of how LF can differ from surface representation over an analysis which is compatible only with a less restrictive theory' (9). Although this position may seem a natural one to take by a linguistic semanticist, it is not by any means always adopted (Schein, for example, provides an extreme case, see (3) below) and for Lasersohn it provides a crucial motivation for the rejection of sentential accounts of noun phrase conjunction.

The first part of Lasersohn's book consists of a discussion of the semantics of conjoined noun phrases that has an interesting and helpful historiographical dimension. Four different ways of approaching the problem are presented which he names the NP/S, Relational S, S/S and NP/NP analyses. Apart from the Relational S analysis (which Lasersohn admits has no obvious proposers, although he does refer to authors whose analyses are 'reminiscent' of it), each of these approaches has, or had, adherents, linguistic or philosophical, and Lasersohn takes great pains to present them in as fair a way as possible, while at the same time indicating their shortcomings. The first, which he ascribes in essence to Aristotle (in my opinion, somewhat dubiously), proposes a distinction in analysis between distributive and collective conjoined NPs. The logical forms of the former are identical to those of the truth-conditionally equivalent conjoined sentences (so *John and Mary are asleep* has the same LF as *John is asleep and Mary is asleep*), while the latter are given analyses in which the conjoined noun phrase is represented as a single constituent and interpreted as denoting a group.

Lasersohn quickly passes over the Relational S analysis (in which group denoting noun phrases are rejected in favour of predicates having valencies that are greater than their associated syntactic structures seem to allow) to provide a lengthy discussion of the S/S approach. This takes all phrasal conjunction to be reducible to sentential conjunction and is traced back to Sanctius in the sixteenth century, through John Horne Tooke in the eighteenth and numerous philosophers in the nineteenth centuries, to Gleitman and Schein in the late twentieth. He provides some reasonable criticism of Gleitman's analysis involving reciprocal relations (e.g. *John and Mary met = John met Mary and Mary met John*) but his complaints against Schein's use of essential separation (see below) are not particularly compelling and are, in any case, mostly dealt with in Schein's book (see particularly chapter 6 on event mereology). Ultimately, Lasersohn's rejection of this approach rests on the adoption of his LF Preference Strategy, noted above.

Unsurprisingly then, Lasersohn favours the NP/NP analysis of conjunction, where conjoined noun phrases are analysed semantically as always involving the conjunction of noun phrases. However, he begins his discussion critically by rejecting analyses (like Montague's and the generalised conjunction strategy of Partee & Rooth 1983) that (superficially) provide the means for representing phrasal conjunction, because of their interpretation in terms of sentential conjunction and their consequent failure to account for collective readings of conjoined NPs. The argument proceeds with a discussion of the latter property, which is followed by a long consideration of ambiguity involving distributional and collective readings of the same sentence (*John and Mary lifted the piano*). Following Link (1983, etc.), he concludes that the locus of the ambiguity is in the predicate and not the conjoined noun phrase. Thus, collective and distributional sentences can be given minimally different LFs, with the latter incorporating a distributive operator over VP (symbolised as ^{D}, below) which ensures that the predicate distributes over the group denoted by the conjoined NP. Thus, (1) gives the LF for the collective reading of *John and Mary lifted a piano*, while (2) is its distributional counterpart.

(1) $[_{S}[_{NP}[_{NP}$ John] and $[_{NP}$ Mary]] $[_{VP}$ lifted a piano]].
(2) $[_{S}[_{NP}[_{NP}$ John] and $[_{NP}$ Mary]] $[_{VP}\ ^{D}[_{VP}$ lifted a piano]].

Lasersohn's arguments here are cogent, complete and hard to refute. Indeed, this chapter (7) would make useful instructive reading for graduate semanticists on how good argumentation should proceed, i.e., carefully, clearly and with due regard for the data.

The first part of the book ends with a sketch of a theory of noun phrase conjunction based on (set-theoretically defined) groups, Lasersohn argues that the latter provide sufficient structure to account for the freedom which is apparent in the interpretation of the relationship between the referents of

conjoined noun phrases and the event they participate in (e.g. whether all such referents have the same thematic relation to the event). (Schein deals with this problem by the use of essential separation.)

This theory of conjunction, however, entails distinct analyses for NP conjunction as against VP and S conjunction, an undesirable result that Lasersohn attempts to put right in the second part of the book. This deals with event semantics and the discussion begins with a lengthy look at the semantics of *together*. Lasersohn notes that the theory of conjunction in part one has unfortunate consequences. For example, in that theory *John and Mary are sleeping* entails *John and Mary are sleeping together* (non-idiomatic reading) and *John and Mary* (*each*) *lift a piano* entails *John and Mary lift a piano together*. Since neither of these are true entailments, something more is required and that is reference to events and the subevents that make them up. In essence, his analysis of *together* treats it as holding true of an event if (and only if) there are no subevents of that event of the same sort where only a subset of the participants in the main event are involved in the subevent. Thus, *John and Mary lift a piano together* is true only if there is no other lifting of the relevant piano within the event referred to where just John or just Mary is involved. This approach, taken from his earlier work, is then developed to solve problems noted by himself and other scholars. Ultimately (chapter 12), he arrives at an analysis that generalises nicely over collective, temporal and locational uses of the adverb, while maintaining the insight of his earlier analysis.

In order to extend the parallelism between conjunction and plurality into the domain of events, Lasersohn presents a discussion of pluractional markers found in some native North American languages (Klamath being the language of illustration). These markers apparently indicate multiple actions named by the verb that may involve multiple participants, times or locations which are given a semantic analysis that is similar to that given to *together*. While the discussion is interesting in many respects, the argument is weakened by the rather sketchy presentation of the data which makes it hard to judge the validity of his analysis. Overall the discussion here does not particularly take the argument further and could have been omitted.

Finally, we return to conjunction and the solution to the problems noted with the theory of part one. Essentially, Lasersohn treats conjoined (and pluractional) verb phrases as denoting groups of events, just as conjoined (and plural) noun phrases denote groups of individuals. He provides a generalised (and rather complex) definition of the interpretation of conjoined functors that appears to have the desired effect.

Unlike Lasersohn, Schein presents his principal hypotheses at the very beginning of the book, the rest of which is thus taken up with the specification of the theory and its justification. For him, there are two principal properties of plurals: that they are predicates and that events and their associated arguments are 'essentially separate'. The notion that plurals

are predicates is developed in chapter two which contains a rejection of the plural object analysis based on Russell's paradox. Unfortunately, his argument here is weakened by a rather cavalier approach to exemplification (a problem that is apparent through the whole book). Schein's argument is based on the assertion that sentences like *There exists an elm* entails *The elms exist*. This, of course, is not a valid entailment and the assertion in a footnote that the fact that plurals are taken to denote more than one element is irrelevant for the argument and that 'analogues can be found for all the inferences [he] discuss[es]' (fn. 6, 323) using circumlocutions like 'more than one elm' or 'the one or more elms' is not shown anywhere. Even if it were, plurals (in English and apart from morphological accidents like *scissors* and *trousers*) do not mean 'one or more' but 'more than one'. Thus, while *There exists an elm* does entail *The one or more elms exist*, it does not entail *More than one elm exists*. Since this appears to be crucial for his argument, it is far from clear that the case against the theory of plural objects is proven.

The discussion moves on to Schein's next major hypothesis: the essential separation of thematic roles from the argument structure of a predicate. This variant of Davidsonian event semantics views all verbs as being one-place predicates over events and participants in those events as being, not arguments of the verb, but linked to the semantics of the verb via an event variable. As an example which illustrates Schein's rather idiosyncratic representation system and his radical departure from Lasersohn's LF Preference strategy, a sentence like *Three video games taught every quarterback two new plays* is given the LF in (3) (= Schein's example 2, p. 57) and not (4) (= Schein's example 3, p. 57) where INFL, TO and OF correspond to thematic roles, equivalent in this case to AGENT, GOAL and THEME.

(3) $\exists e[\exists X : 3(X) \,\&\, \forall x(Xx \rightarrow Gx)]\forall x(\mathrm{INFL}(x,e) \leftrightarrow Xx) \,\&$
$[\text{every } y : Qy][\exists e' : e' \leqslant e](\mathrm{teach}(e') \,\&\, \forall z(\mathrm{TO}(e',z) \leftrightarrow z = y) \,\&$
$[\exists Z : 2(Z) \,\&\, \forall z(Zz \rightarrow Pz)]\forall z(\mathrm{OF}(e',z) \leftrightarrow Zz)))$

(4) $\exists e[\exists X : 3(X) \,\&\, \forall x(Xx \rightarrow Gx)][\text{every } y : Qy][\exists Z : 2(Z) \,\&\, \forall z(Zz \rightarrow Pz)]$
$(\mathrm{teach}(e',X,y,Z,e))$

The complexity of the representations implied by the theory of essential separation is matched by the complexity of Schein's arguments for it, which take up chapter 4. The argumentation involves an examination of sentences containing three or more quantified noun phrases and an exploration of the contexts that satisfy or fail to satisfy the different representations indicated above. It would be impossible for me to review these here, but suffice it to say that he concludes that essential separation is necessary to properly account for all these contexts because it allows reference to parts of the event described by the verb. In other words, essential separation allows the relation between plural noun phrases in a sentence to be vague enough to allow for the observed variation in interpretation of such sentences. (This is a feature

of Schein's theory that is criticised by Lasersohn, referring to an earlier unpublished manuscript, but it seems to be adequately defended here.)

Having provided a semantics for his basic theory of plurality and quantification in chapter 6, Schein devotes the rest of the book to extending the theoretical coverage to account for semidistributive and cumulative quantification. The discussion of the latter takes up almost half of the book (chapters 9 to 12) and is not particularly easy to follow. This is partly due to the difficulty of constructing natural sounding examples with sufficient quantificational force to illustrate the points being made (a problem for all researchers into natural language quantification) and partly due to the idiosyncratic notation, illustrated above. The discussion is, however, fairly exhaustive, providing discussions of increasing and decreasing quantifiers, nonmaximal reference of anaphors, dependent quantifiers and certain interactions between quantifiers and adverbials. On the way, there is an extremely interesting discussion (in chapter 10) of the importance of perspective to account for the felicity (or otherwise) of definite and pronominal reference which for me is one of the high points of the book and would be of interest for anyone interested in the role of context in formal semantics of natural languages.

In summary, I would recommend both books to those researching into plurality. Both books provide interesting arguments for their different theoretical positions and both cover a range of issues involved with plural terms. However, while I would happily recommend the Lasersohn to graduate students (who would find it to be a very helpful introduction to nominal and verbal semantics in general), I would be more circumspect about the Schein, which is difficult and does not always reward the effort required to understand the argumentation.

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Beth Levin & Malka Rappaport Hovav, *Unaccusativity: at the syntax-lexical interface*. Cambridge, MA: MIT Press, 1995. Pp. xii + 336.

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The most ambitious goal of work of this kind would be a linguistic theory in which different verb argument deployments would be fully predictable from verb meanings by universal rule. While Levin & Hovav do not emphasize this goal, their book is a remarkable sorting out of mainly English verbs which goes a long way towards substantiating it.

It is not obvious that the goal is achievable. There are well-known instances of 'minimal pairs': *wonder* vs. *ask*, where the second but not the first takes an NP complement (*ask*/**wonder the time*); *try* vs. *attempt*, where the first is optionally transitive and the second not (*I tried*, **I attempted*). Nor is it clear how to distinguish these cases from the large number of cases for which Levin & Hovav show that subcategorization (by which I mean details of argument deployment, however implemented) is predictable. But Levine & Hovav's findings certainly make a universal mapping a plausible research objective.

Levin & Hovav's modus operandi is to supply a sufficient set of semantic distinctions that the verbs of English can be sorted into classes with uniform syntactic behavior. They show us the utility of the distinctions that they draw, but the finding of relevant distinctions still remains an art, of which they might be the best practitioners.

The tighter the correlation between meaning and behaviour the better the understanding of lexical learning. Presumably the child does not need to learn what distinctions will be useful; the full map from semantics to syntactic behavior is given in advance. Full predictability does not establish epistemic priority: we may think 'if the subcategorization is fully predictable from the meaning, then it need not be learned' but in fact, it may be that the meaning of a verb is partly deduced, or at least first approximated, from the subcategorization, it being the more directly observable property.

What is certainly not universal, but language particular, is the realization of semantic categories themselves. Levin & Hovav accept Talmy's (1985) finding, for example, that French and English differ systematically as to whether manner-of-motion verbs (*float*) can be used as verbs of directed motion; in fact, they extend his observation to verbs of sound emission (*roar*: *John roared down the road*). But the systematicities of lexical inventorying which structure the lexicons of particular languages lie outside the scope of their book. Much of what is considered language-particular syntax may lie in fact in the theory of lexical inventory; but the discovery of the system of lexical inventory can hardly begin without the sort of semantic distinctions put forward by Levin & Hovav.

The main contribution of Levin & Hovav's work is in fact the distinctions

it draws, and the striking regularities in the English lexicon that these enable. The fear one has when making distinctions is that in the end, one will wind up with a separate story about each case; but Levin & Hovav's well motivated distinctions do not condemn us to this explanatory atomism. Although I will question details of the analysis, I am sure that the distinctions are all correct.

Levin & Hovav usefully warn against language-to-language comparisons that do not take into account these semantic distinctions. A given verb token in one language may participate in a certain number of distinct classes, and there is no reason to expect any token in another language to be identical in this respect, especially since the inventories of classes will in general be different. They question a number of conclusions that have been drawn from such erroneous comparisons; see the discussion of *melt* (100) for a particularly clear case, and the discussion of *blush* below.

Levin & Hovav's main target of analysis is the unaccusative-causative alternation, (*break* (intransitive) and *break* (transitive)). Previous analysis has turned on the question of whether the axis of the alternation was semantic or (purely) syntactic, and whether the alternation was fully confined to the lexicon, or was linked to sentence-level semantics (such as aspect). Levin & Hovav provide new evidence to show that the syntactic difference is determined by a purely lexical semantic difference which they embed in a theory of how arguments are linked to syntactic positions.

The main obstacle to a satisfying analysis of the alternation, if Levin & Hovav are correct, is that the true cases of it lie amidst a horde of pretenders. The authors substantiate a number of properties of the alternation: the intransitive does not take cognate objects; both transitive and intransitive can take result clause modification under 'Simpson's generalization'; and the transitive has no agency restriction on the subject; and these are theoretically rationalized and then used to weed out the false cases.

Simpson's generalization asserts that resultatives can only modify objects; hence, *John painted the fence* (*red*/**tired*). But unaccusatives are the interesting exceptions: *the fence rusted orange*. But if unaccusatives are underlyingly transitive, with no subject, then Simpson's generalization is extended to these as well.

In the first chapter Levin & Hovav plausibly suggest that Simpson's generalization is to be accounted for in syntactic/semantic terms, along the following lines: the resultative modifies the change-of-state, and verbs instantiate the thing whose state is changed only in the direct object position (the 'Change-of-State' or 'Directed Change' linking rule, p. 51). But as in other cases, the rule given ('An NP that refers to the entity that undergoes the change of state … must be the direct object of the verb …') is not helpful in a number of particular cases; for example, in *John memorized the answers* it would seem to be John that underwent the change of state, rather than the answers (but **John memorized the answers well-briefed*).

A class of causative agentive verbs in English falls outside the class of verbs picked out by the above properties. The verb *march* (112) appears to enter the causative-unaccusative alternation, as in (1) but does not admit nonagentive subjects, as in (2).

(1) (a) The soldiers marched.
    (b) The generals marched the soldiers to war.
(2) *The downpour marched the soldiers to the tents.

So *march* is set aside; it turns out verbs of 'internal causation' like *march* do not enter into the unaccusative alternation; only verbs of 'external causation' do. One and the same verb can participate in both classes, but with different meanings (151):

(3) (a) The doorbell buzzed.
    (b) The postman buzzed the doorbell.
    (c) The bee buzzed.
    (d) *The postman buzzed the bees.

A bee buzzes thanks to 'internal causation', but a doorbell requires external causation; so only in the second use does the unaccusative-causative alternation obtain. Likewise, verbs of existence and appearance (*appear*, *live*, etc.) are excluded; their basic use is unaccusative, but they lack the causative half of the alternation.

In the last chapter, Levin & Hovav convincingly argue that locative inversion is not diagnostic of unaccusativity, thereby clearing away further irrelevant cases.

Beside the distinctions in meaning, Levin & Hovav make manifold distinctions among the ways that predicates can be related to one another in the lexicon; but the number of these – at least five – is unsettling. Levin & Hovav stipulate or assume 5 distinct mechanisms, but without an overarching theory of these mechanisms and how various phenomena are to be sorted out amongst them, these can only be taken as suggestions.

First, there is the rule of detransitivization (and presumably other rules of its sort) which, according to Levin & Hovav, is responsible for the binding of the external argument in the derivation of unaccusatives from causatives (I will discuss shortly the merits of this proposal), which is presumably a universal rule, not one triggered by language particular morphology; or so one would conclude from Levin & Hovav's discussion of the Athabaskan alternation (115) which does not use the causative suffix available in those languages. Hence, causative suffixation, and argument-changing affixation in general, constitutes a second distinct sort of lexical relation. Third, there is the assignment of one and the same predicate to two different semantic categories. This operation is due at least in some cases to systematic but

language particular 'lexical rules' not necessarily tied to morphology; for example, the existence of parallel 'manner of motion' (swim around) and 'directed manner of motion' (*swim to the other side*) predicates in English. Fourth, there are cases where one and the same verbal 'token' is compatible with 'two or more semantic templates'; for example, *buzz* can be construed as either 'externally caused' or 'internally caused' (211). And finally, of course, there is accidental homonymy.

Are there too many possibilities? Could the unaccusative alternation be assigned to mechanism 3 (one and the same predicate assigned to two different semantic categories)? This draws us immediately into questions about the correctness of Levin & Hovav's analysis of the alternation, which I defer briefly. But if the 'parallel categories' analysis is not the correct one, why not? Does the universality of the phenomena decide the issue? Not on strictly logical grounds; either mechanism could allow of language variation, or not. Is there any difference to be made apart from universality? Another possibility is to derive the causative from the unaccusative by 'θ' suffixation, presumably by a universal suffix with fixed meaning (causative); this would be mechanism 2, subcase 'null affix':

(4) *break*(intrans) + θ → *break*(trans)

Why is this not the correct analysis, as in fact a number of researchers have suggested?

My greatest reservation concerns Levin & Hovav's analysis of the unaccusative alternation. In their view, the unaccusative (superficially intransitive) *break* is derived from the transitive by lexically binding the agent argument of the transitive. But this assigns to the unaccusative the same structure as the passive. Levin & Hovav show that the unaccusative is syntactically distinguished from the passive in various ways having to do with the syntactic availability of the agent argument as controller; for example, '**the ship sank to collect the insurance*' vs. '*the ship was sunk to collect the insurance*' (from Roeper 1987), and they attribute this to the difference between binding the external argument BEFORE argument structure, in the case of the unaccusative, and AT argument structure in the case of the passive. But in both cases, the agent should be 'cognitively' present, and so no difference in meaning should be felt between *the rock was broken* and *the rock broke*. But it is felt, and can be brought out in various ways:

(5) (a) The rock spontaneously broke.
(b) The rock was spontaneously broken.

Levin & Hovav insist that unaccusatives are 'externally caused', but 'spontaneously' should be incompatible with that. Do all events have causes? What is the external cause of *the proton disintegrated*, a statement not incompatible with current understanding of quantum theory?

Furthermore, the notion of internal and external causation is difficult to apply in a good number of cases; for example, *blush* and *tremble* are classified as 'internally caused' events (90) even though external causes are easily imagined; the classification is correct as far as the syntactic behavior goes, but the rationale is mysterious, though the discussion of the difference between *shudder* and *shake* (100) is illuminating.

At the same time, I think Levin & Hovav are correct in their characterization of the difference between *break* (which participates in the causative-unaccusative alternation) and *cut* (which doesn't: **the paper cut*): *break* makes no specification about what the causative process is, whereas *cut* does (102 ff.). But this only confirms the view that intransitive *break* does not encode or presume the causative process in any way (or lack of it, for that matter); and so it does not have argument positions (bound or not) that correspond to the causative agent.

One of the more subtle and intriguing of the distinctions drawn by Levin & Hovav is among 'internally caused' intransitives between those which are also 'directed change' verbs and those which are not; often, as in English, there is no overt mark of the distinction, but it nonetheless determines that the former are unaccusative, and the latter not. In English, some types of verbs are systematically ambiguous on this dimension; for example, the verbs of posture: *stand* means to be standing (unergative), or to stand up (unaccusative), as witness the following telling demonstration, which uses the incompatibility of passive and unaccusative (164):

(6) (a) This platform has been stood on by an ex-president.
(b) *This platform has been stood up on by an ex-president.

In a similar vein, Levin & Hovav show, for example, that *blush* is systematically ambiguous across languages, sometimes meaning 'to go into a blush' (directed motion), and sometimes 'to be in a blush'; only the former is unaccusative (160). Such contrasts show at the same time how various languages are, and how uniform: while a given verb token in different languages can belong to different categories, the categories themselves seem to be in a regular language-independent relation to argument structure.

In Chapter 3 Levin & Hovav draw from the distinction just mentioned (between *stood* and *stood up* and similar cases) an important theoretical conclusion about the nature of the argument linking process. In their proposal, the internally caused verbs of 'directed change' are subject to the 'Immediate Cause' linking rule, which links the causing argument to the subject position; but they are also subject to the already mentioned 'Directed Change' linking rule, which would link the thing changed to the object position. But for these verbs, the causer and the thing changed are the same argument, so rule ordering is invoked to ensure that the first of the two rules takes precedence. Thus, for example, *stood up*, as a verb of directed change, will be mapped into the unaccusative class; but *stood* in its stative meaning

will be marked unergative. The observations are compatible with several alternative mechanisms; for example, one could imagine a solution in terms of ordered constraints, as in Optimality Theory. But Levin & Hovav's remarks do seem to establish that some sort of ordering is implicated in the linking mechanism.

In grounding the linking rules in notions like 'immediate cause' and 'directed change', Levin & Hovav put aside various proposals that unaccusativity is keyed to aspect; but it is hard to see how the notion of 'directed change' by itself could draw the following distinction (170):

(7) (a) *The skirt lengthened.
(b) In the 70s, skirts lengthened.

The problem raised by this contrast in fact goes beyond the role of aspect in the system; it challenges the notion that unaccusativity is a purely lexical categorization, one not determined at least in part in compositional syntax.

Levin & Hovav is a real advance in understanding the semantic classification of verbs and its link to syntactic argument deployment, and deepens our understanding of variation in this domain. It achieves this through sufficiently refined and of course correct semantic distinctions. Their analysis of the unaccusative-causative alternation is compatible with their findings in verb semantics, but on several points falls short of full conviction.

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Jack Windsor Lewis (ed.), *Studies in general and English phonetics: essays in honour of Professor J. D. O'Connor*. London: Routledge, 1995. Pp. xxii + 473.

Reviewed by LEIGH LISKER, Haskins Laboratories

This volume honoring J. D. O'Connor for his contributions to phonetics offers a rich variety of studies that deal largely with topics to which 'Doc' has

himself devoted much of his published work. The thirty-eight studies it includes are preceded by a short foreword (by Sir Randolph Quirk), a very brief account of Doc's pre-retirement career, a listing of his publications, and a helpful overview of the book's contents. The scholarly contributions are in four categories: general phonetics and phonological theory; pitch, intonation and rhythm; the phonetics of mother-tongue English; the phonetics of non-mother-tongue English.

The quality of the essays presented is of the high quality we expect from scholars trained in the London school of phonetics. Within each area covered a range of interests is considered, from the theoretical appeal of A. Fox's 'Principles of intonational typology' to 'narrower' matters of description ('The low vowels of Vancouver English' of H. J. Warkentype & J. H. Esling) and advice on teaching English as a foreign language ('Segmental errors in the pronunciation of Danish speakers of English: some pedagogical strategies' by I. Livbjerg & I. M. Mees). Even more 'practical' concerns are addressed in an account of the present state of the art of speaker identification in Germany (H. Künzel) and an enumeration of factors that should govern the selection of 'Voice types in automated telecommunications applications' (J. Laver). In the interest of brevity comment will be limited to a subset of the papers that I find especially evocative.

1. Basis of articulation

Phoneticians often restrict description to properties believed to identify a speech signal as one particular word sequence and not others in the language, thereby performing a service function for linguists, who on principle neglect 'linguistically irrelevant' phonetic properties. Three papers remind us that phonetic description need not be so narrowly 'functional.' In 'Postura, clear and dark consonants, etcetera' L. van Buuren invites us, from the perspective of a Dutch student of English, to consider a property said to characterize, not segments or other elements, but a LANGUAGE tout simple, i.e. its 'articulatory setting'. He supposes that for any language 'speakers maintain a certain constant equilibrium in the tongue body and lip muscles, which is best described as a vowel "positioning" and indeed should be described before any other tongue and lip activity' (139). In 'Approaches to articulatory setting in foreign-language teaching' B. Collins & I. M. Mees assert that Danish learners of American English can profit from awareness of the 'uvularisation combined with alveolarisation' of AE (419) as against the 'palatalisation plus laryngo-pharyngealisation' of Danish (417). In similar vein J. Kelly ('Consonant-associated resonance in three varieties of English') draws attention to the occasionally reported cross-language differences in the 'resonances' of certain consonants.

That the three just-mentioned papers involve the free expression of subjective judgment is of course in itself no reason to reject their arguments,

for while phonetic intuitions, however widely shared, are not ipso facto true, neither can they be lightly discounted. Nevertheless, this somewhat dubious reader finds these papers more provoking than compelling, and wonders whether it is accidental that they suggest few tests of the propositions suggested (those they do are hopelessly gedankenexperimental in nature) and that no supporting physical data are supplied. Can there not, for instance, be made available ANY physical evidence for the assertion (Collins & Mees, 417) that stop aspiration is explained by an articulatory setting characterized by 'lack of tension' in oral closure, a claim that should unsettle anyone educated to believe (with possibly no more reason) that aspiration is rather a symptom of 'fortis' articulation?

2. Issues of prosody

Of the ten or so papers on intonation two should attract wide interest for the two general questions raised: 1) Is it feasible to construct a typology of intonation, defined narrowly as exclusively pitch or Fo, that can accommodate both its language-specific and its language-universal aspects? (A. Fox), and 2) Can criteria be devised whereby utterances can be unambiguously segmented into 'intonation units'? (P. Tench). Fox assumes that cross-language Fo differences are partly determined by other non-segmental features, and reaches the tentative conclusion that 'intonational features which are not dependent on the rest of prosodic structure are not significant enough to justify a meaningful typology of intonation' (206). Tench, in worrying the question of how to segment a speech stretch into 'intonation units', compares O'Connor's views with certain others, and finds for the former, arguing that a segmentation based on the metrical foot as defined by Abercrombie and adopted by Halliday too often leads to counterintuitive breaks within syntactically and semantically close-knit word groups. O'Connor's treatment is preferred because it avoids the need to entertain such uncomfortable divisions by allowing rhythm units in which a stressed syllable may be preceded as well as followed by unstressed syllables. It seems unfortunate that at the outset of this illuminating account Tench makes the question-begging assertion that 'if an utterance consists of a single intonation unit, then the boundaries are preceded and followed by silence' (270), and that he nowhere makes explicit why intonation units need always be unambiguously non-overlapping.

Two papers on prosody that report experimental data deal with speech of a special kind, somewhere between the 'citation' forms of the laboratory and spontaneous speech. In 'Spelling aloud: a preliminary study of idiomatic intonation' P. D. S. & M. G. Ashby report some regularities in the 'chunking' of letter names by subjects' oral spellings of certain familiar words: words of more than five letters are broken up into groups averaging about 2.7 letters each, a number that is independent of the number of letters

in the word. They propose a simple computer algorithm for inserting prosodic boundaries in synthesized spellings that yields 'realistic results' (152). And while they initially alert us to the possibility of some role for syllable/morpheme structures, neither element figures in the algorithm, so we are left to suppose that their effect on chunking behavior is negligible. The 56-word corpus sampled does however include a few items, e.g. *timetable*, where the automatic parsing routine should generate syllabically/morphologically counterintuitive groupings. Thus it seems unlikely that *timetable* would be divided into *tim–eta–ble* by a human speller familiar with the form.

In 'Intonational stereotypes: a reanalysis', F. Nolan wants to account for the generally observed tendency for fundamental frequency to fall within a sequence of stressed followed by unstressed syllables, and at the same time to 'decline' over successive stressed syllables: Is this falling saw-toothed Fo profile the product of an interaction between the two falling contours of different scope, or is it rather the outcome of a rule in which the Fo of an accented syllable is related solely to that of an immediately preceding one. On the basis of readings of several orderings of the musical expressions *B# C# D# E# G#* under various 'emphasis' instructions, Nolan finds for the interaction model, since the emphasis that raises the Fo on one item has no effect on the subsequent Fo contour. Surprisingly enough, as Nolan points out, emphasis has no apparent effect on timing. The reader may wonder why his interesting body of data is not accompanied by any statistical evaluations.

3. On syllable boundary placement

Three interesting contributions appeal to syllable boundary placement in treating certain kinds of phonetic variation in British English. Thus J. Baldwin explains the unexpected presence of stop aspiration in the /k/ of *back up* and its absence in the /t/ of *fifteen* as effects of consonant 'capture' by a following syllable, so these words are [bæ.kʰʌp] and [fɪ.ftin], despite the unorthodox onset and codas that result. He argues for [fɪ.ftin] because /t/ as [t] instead of [tʰ] is otherwise inexplicable. But there are conceivable alternatives: the form might be [fɪf.tin] and not [fɪf.tʰin] because of lack of stress (about which the reader is uninformed), or it might be treated phonemically as /fɪfdin/, the /d/ devoiced following /f/. (In American English {teen} and {ty} certainly occur sometimes as /din/ and /di/.)

J. C. Wells proposes a 'coda-maximizing syllabification' rule to account for the emergence in RP of the syllabic consonants [m̩ n̩ r̩ l̩] (which are of course 'old hat' in American English), so that *Italy* as [ɪtl̩ i] derives from an underlying [ɪt.ə.li] via [ɪt.əl.i]. Rather oddly, neither Baldwin nor Wells mentions the possibility of sometimes taking a segment (or segments) as ambisyllabic. This option is explored by J. Local, who uses a speech synthesis system based on a 'co-production' model of speech ('YorkTalk') to compare

the acoustic consequences of alternative syllable divisions. His conclusion, argued on the basis of spectrograms in which the reader may not readily see everything that the author does, is that a syllable division which maximizes ambisyllabicity yields the most realistic output.

4. Segments: alternation and neutralization

Three writers have things to say in this general area. The first (T. Akamatsu) draws attention to the assimilation, in allegro RP, of word-final alveolar oral and nasal stops to an immediately following labial or velar stop or nasal, so that e.g. /t/ > /p/ in *that pen* and /n/ > /ŋ/ in *thin girl*. The author makes two points: 1) that 'as the rate of speech increases, coordination in executing the various articulations necessary in the production of a sound deteriorates simply because the speech organs find it increasingly difficult, in the progressively less time available, to maintain reasonably clear-cut implementation of them' (3), and 2) that these assimilations should not be understood to involve phoneme replacement, but rather to be cases of alveolar-labial and the alveolar-dorsal neutralisation. If the absence of any reference to such conceivable assimilations as /g/ > /b/ in *big boy* or /p/ > /t/ in *lap dog* means that they do not occur, then it would seem that an ease-of-articulation explanation, plausible as it might be, can only be seriously entertained if we suppose that sequences such as [gb] and [pd] involve less 'articulatory effort' than do [tp] and [dg]. The second point has arguably more merit, and is supported by the fact that while underlying /t d n/ may be realized as [p b m] before labials and as [k g ŋ] before velars, they also occur as dentals before [θ ð], as retroflexes before [ɹ], perhaps even as labiodentals before [f v], i.e. as segments that nowhere in English have independent phonological status. It is however not so easy to follow Akamatsu when he asserts that phonetic identity, as of e.g. *ran* and *rang* before *quickly*, implies phonological identity, and that because there are neutralizations in allegro English, therefore this variety of the language possesses a larger number of 'distinctive units' (8).

In 'Assimilations of alveolar stops and nasals in connected speech', W. J. Hardcastle examines assimilation instrumentally. Two questions are addressed: 1) How consistently do English speakers assimilate word-final alveolars to following velars? and 2) Are stops more susceptible to assimilation than nasals? The answers that emerge from electropalatographic data are 1) not very, for there is variation, both within and across speakers, from zero to complete assimilation of the 'underlying' alveolar closure, and 2) there is 'a clear preference for assimilating the nasals' (66). The apparent contradiction between the Akamatsu and Hardcastle findings seems easily resolved. The EPG data come from 'connected speech', but given that this speech was recorded under laboratory conditions of a fairly

invasive type, we may reasonably conclude that it was far from anything like the casual allegro speech discussed by Akamatsu.

A. Butcher ('The phonetics of neutralisation: the case of Australian coronals') uses direct palatography to examine coronal consonants in several Australian languages, and asks whether neutralization products are 1) phonetically identical with one member of an elsewhere contrasting pair of sounds, 2) 'in-between' sounds, or 3) both sounds used nondistinctively. Butcher finds neutralization products to be in most cases 'in-between' sounds. One question arising from his description of the complex relations among coronals across the languages examined is whether Australian neutralization is true neutralization, like that of final stop voicing in German, or rather the pseudoneutralization of the English post-/s/ stops, in which position there is no underlying voicing contrast to be neutralized.

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Alice G. B. ter Meulen, *Representing time in natural language: the dynamic interpretation of tense and aspect*. Cambridge, MA: MIT Press, 1995. Pp. xii + 139.

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The author's intention is, in her own words, '…[to] present a systematic investigation of how we use the temporal information in texts or discourse to reason in time about the flow of time' (ix). Ter Meulen identifies dynamic interpretation, partial information, context and situated inference as the starting points of her investigation. Her emphasis is on the reasoning we do with temporal information – motivated, she says, by 'the existing wealth of descriptive semantic insights on tense and aspect, contrasting with the relatively poor understanding of valid reasoning patterns' (x).

Ter Meulen begins by presenting several short narrative discourses in the simple past and considering what inferences may be drawn from them regarding the temporal relations between the events described. These reveal the importance of tense, aspectual class, temporal adverbials and causal connections to the determination of temporal relations. The author argues, however, that causality should not form part of a theory of temporal reasoning – causal connections belong to the domain of AI and Cognitive Science, and she aims to develop her theory in a way that makes clear 'what

options are left open by the information given' to be selected upon by further knowledge we may have about possible causal connections. It is not clear to me what she intends to happen in cases where knowledge about causality may arguably 'overrule' information given by tense, aspects and so on. It would be helpful here if ter Meulen were to give an explicit comparison with other systems that try to do something similar. Notable among these is Lascarides & Asher (1993), a system which determines temporal relations in discourse on the basis of various kinds of information, including world knowledge about causal connections. As Lascarides & Asher's enterprise is similar in many respects to ter Meulen's, it is puzzling to find no mention of their work in ter Meulen's book. Arguably, there is no space in a book such as this for detailed comparisons, but at least mention should be made of related work.

In 1.2, ter Meulen discusses the contribution of aspect and aspectual class to the determination of temporal relations, and presents informally the classification upon which her reasoning system is based. The classification is similar to Vendler's, but with activities, accomplishments and achievements renamed as 'holes', 'filters' and 'plugs'. The idea is that if an event description is interpreted as a hole, the next event to be described is interpreted as temporally included within the previous one. A plug, by contrast, causes the next-described event to be placed after the previous one, and a filter allows either possibility (this is refined slightly later in the book). Thus, an important feature of the approach is that activities are treated as a separate class, in contrast to the standard DRT treatment (Kamp & Reyle 1993) and Lascarides & Asher (1993), which make a distinction only between 'events' and 'states'.

In 1.3, the author discusses the notion of situated reasoning about time. She points out that in natural contexts, human reasoning is based on partial and often very limited information. A text is not 'an unstructured lump of information' but a structured object in which the information available from a discourse is determined by the 'temporal vantage point' from which it is surveyed. Thus, a particular conclusion may be valid at one point but not at another. She discusses examples with the simple past, past perfect, progressive, *before*-, *after*- and *when*-clauses, showing how, for example, perfect and progressive aspect interact with event order and with aspectual verbs like *start*, *continue*, *end* and *finish* to determine what inferences may be drawn at particular points in the text.

In Chapter 2, ter Meulen analyses the logical properties of the aspectual verbs, including *start*, *begin*, *continue*, *end* and *complete*. She investigates the interaction of these verbs with negation, and shows how their interaction with the various aspectual classes affects the flow of information. In order to capture the fact that, for example, *starting to read* describes the action that turns a state of non-reading into a state of reading, she designs a representation system that encodes how information about temporal

relations is extracted in the process of interpreting a text. This representational system forms the heart of her analysis, and is worth examining in some detail.

She represents events as infons – situation-theoretic objects with internal structure, consisting of a relation, one or more arguments and a polarity (positive or negative). I find her wording confusing at this point. She begins by saying '…[events] consist of relations, objects related by them, and…[a] polarity…' (25) – thus apparently equating events with infons. But later in the same paragraph, she appears to consider infons as types, with types being used to classify events. That is, she now seems to be saying that events are not infons but are situations classified by infons (which is, in fact, more standard in the recent situation semantics literature – see, for example, Cooper 1995). As her explanation proceeds, it becomes clear that infons correspond to event types, but it never becomes clear to me exactly what status situations have in her system, and precisely how they are related to events. Nor is it clear what role 'times' play in infons and events. The infon in example 2.18b (25) has an argument called 'representing time' which is not clearly explained. No infons appearing later contain arguments that are identified as 'temporal'. This leads to potential confusion when ter Meulen makes statements like 'When someone is not reading in a situation, nowhere in that situation is she reading.' If relations can have temporal arguments, then presumably a situation can support the infon,

(1) $\ll$ read, mary, t, $-$ $\gg$

while also supporting,

(2) $\ll$ read, mary, t′, $+$ $\gg$

where $t \neq t'$. That is, Mary can be reading at time t′ in a situation s, while she is not reading at a different time t in s. Perhaps ter Meulen intends

(3) $s \models \ll$ mary, read, $+$ $\gg$

to correspond to the fact that Mary is reading throughout the temporal duration of s, but this is not made clear.

This may be a problem, since infons/types are the building blocks of the Dynamic Aspect Trees which form the basis of ter Meulen's system for reasoning about events and times in the rest of the book.

In Chapter 3, ter Meulen develops the notion of a Dynamic Aspect Tree (DAT), which is a directed graph whose nodes are labelled by types/infons encoding descriptive information about the events described in the text. These nodes may either be OPEN (corresponding to HOLES), CLOSED (corresponding to PLUGS) or either (corresponding to FILTERS), and this

property controls the temporal relations between successive events in the text. State descriptions are not represented as nodes in their own right, but as 'stickers' introduced to label already-existing nodes. This is an interesting treatment of stickers which, as it is clearly related to other proposals in the literature for treating states as predicates of times (such as Galton (1984) and Sandström (1993)) would merit further discussion here.

A point about presentation: ter Meulen takes several pages to give a detailed explanation of what DATs are and how they are constructed (39–42). Not until (43) are we presented with an example of a DAT and can actually see what they look like. Such a diagram a few pages earlier would help the reader considerably.

The nodes of a DAT are connected by downward arrows which represent temporal inclusion between events. The left-right order of downward paths of connected nodes reflects temporal precedence. Each DAT has a single ROOT NODE, representing the entire episode described by the discourse. Each DAT has a rightmost terminal node called the SOURCE NODE, representing the communication event. There is also a CURRENT NODE, the last node to be constructed in the interpretation process. The open/closed status of the current node determines how the given DAT must be updated when further information is supplied. If it is an open node (a hole), the DAT grows an arrow downwards to a new dependent mode. If it is closed (a plug), the DAT grows a new node from one of the present nodes dominating the current node. If there is a choice of possible dominating nodes here, the lowest dominating node that is 'compatible with the new information' is chosen as the parent node. New nodes are introduced only by simple past tense clauses describing events. Clauses describing states (which include progressives, perfects, conditionals and generics) introduce stickers on existing nodes. If the current node is a plug, the sticker is appended to the label of that node. If the current node is a hole, the sticker is appended to the next node. This is given simply as a rule, with no discussion of the motivation. Although it appears to give the desired results for ter Meulen's examples, it would have been helpful to include some discussion and justification for the rule.

The processing of a piece of discourse begins with a syntactic analysis of the sentence under consideration. This gives rise to an infon, with the NPs represented as parameters paired with appropriate restrictions. DATs are built up sentence by sentence, according to the rules outlined above. The author gives a number of examples of short texts and the DATs derived from them by this method, showing how they give rise to the required inferences concerning temporal ordering of events.

Although the system seems to work well for the examples given, there are, in my opinion, some important omissions.Firstly, no explicit comparison is made with the predictions made by DRT (Kamp & Reyle 1993). Secondly, the contribution made by temporal adverbials is not considered. Ter Meulen admits that these are not treated, but given their importance to temporal

relations in discourse, it is not clear that they can be so easily ignored. Temporal adverbials not only select among possible temporal orderings – they frequently override the temporal orderings that would be derived in their absence. Because of this, it is not easy to see how the system could be extended to incorporate their contribution. Thirdly, I do not see how the system could be extended at all readily to deal with anaphoric reference to times and events. Infons do not have temporal constituents, so no referents appear to be introduced corresponding to times. DATs contain no explicit reference to situations/events, so it is not clear how anaphoric reference to events could be captured.

In Chapter 4, ter Meulen gives more detail on the portability conditions associated with the various kinds of states. The 'downward persistence' of stative information is captured by the fact that a sticker can be copied freely to any node that it dominates. She also considers the more difficult question of when stickers can be transmitted upwards and horizontally. Perfect states are assumed to begin when the corresponding event is completed, and to continue forever after, and the portability rules for perfect stickers reflect this. She attempts to formulate portability constraints for generic states which say that generic stickers can be freely imported upwards in a DAT provided that their restrictor is supported, which, as she admits, does not address the thorny problem of how to determine what material goes into the restrictor of a generic statement.

Two notions of perspective emerge in this chapter: (1) PERSPECTIVAL SHIFT – which is involved in the updating of a DAT by, for example, the plug rule (which involves backing up to a higher node); (2) PERSPECTIVAL REFINEMENT – which is what happens in a narrative flashback, where the text directs us to add new information to a node that was previously closed off. Introducing perspectival refinement allows the author to revise her earlier definition of filters as allowing a choice between holes and plugs. Instead, a filter is always represented as a plug, but perspectival refinement may unplug it and turn it into a hole if the new information meets certain conditions.

The operation of the plug rule is discussed in more detail, including constraints involving compatibility of information. Ter Meulen explains how, normally, the plug rule simply introduces a right sister to the current node, and sets it to be the new current node. But if the new information is incompatible with some information contained in the current path, the interpretation backs up to the nearest hole that is labelled with compatible information. This hole is plugged up, and it is selected as the current node, and the plug rule applied to it. The discussion here is rather difficult to follow, and, once again, more diagrams and an example would be helpful. Since so much depends on checking for compatibility of information, and since the author claims to be developing a system that could be used as a basis for building a reasoning system, some discussion of complexity issues would also be welcome.

In Chapter 6, ter Meulen presents a useful fragment with syntactic rules, rules governing the mapping of syntactic structures to types, DAT rules and specification of the semantics of DATs in terms of embeddings into 'event structures'.

In summary, I think ter Meulen presents an interesting proposal from a somewhat novel standpoint, which has relevance to temporal, aspectual and discourse semantics as well as to the development of temporal reasoning systems. The book is short and the explanations, while usually clear and precise, are sometimes rather terse and read like summaries. More time spent on explanation, more diagrams, and some discussion of other relevant work would make this a more useful book, especially to students.

As mentioned above, there are also some worrying omissions from the system, and I am not at all clear how it could be extended to account for temporal adverbials and anaphoric reference to times and events.

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James L. Morgan & Katherine Demuth (eds.), *Signal to syntax: bootstrapping from speech to grammar in early acquisition*. Mahwah, NJ: Lawrence Erlbaum and Associates, 1996. Pp. xi + 487.

Reviewed by JOHN ARCHIBALD, University of Calgary

The contents of this volume are based on the proceedings of a conference held at Brown University in 1993. The goal of the conference was to bring together researchers from a variety of backgrounds to investigate the question of how children's processing of the speech stream bears upon the acquisition of syntax. Linguists, computer scientists, acousticians,

psychologists and cognitive scientists were present, and as a result, there is something here to make almost anyone feel inadequate. The book, running to 487 pages, contains 25 chapters divided into five sections: (1) The nature, perception, and representation of input speech, (2) Speech and the acquisition of words, (3) Speech and the acquisition of grammatical morphology and form classes, (4) Speech and the acquisition of phrase structure, and (5) Speech and the acquisition of language.

Before getting into a little more detail, let us broadly consider the difficult task that faces the child. The linguistic input comes at the learner in the form of a continuous sound wave. There are no obvious pauses between every word, and yet the child must learn how to segment the speech stream into a series of recognizable words. These words must be stored in the mental lexicon. Acquisition, of syntax, however, is more than learning how to string a sequence of words together. The child must acquire non-linear (or hierarchical) representations for which there is no obvious trigger in the input.

The Phonological Bootstrapping Hypothesis (Gleitman & Wanner, 1982) maintains that children are sensitive to phonological cues that provide information about syntactic structure. Such things as intonation contours, pausing, and vowel lengthening have been argued to signal the end of a syntactic constituent. For example, a pause is more likely to occur between constituents than within a constituent, as can be seen in the following sentence:

(1) The man (pause) who I saw on the corner (pause) is my father.

Similarly, the pitch of an utterance is more likely to fall at a constituent boundary. So, if a learner is sensitive to such phonological cues as pause length and pitch contour, then the learner may have an input trigger that would signal the setting up of a new syntactic constituent. This sets the stage for the detailed investigations of each of the chapters.

Following an impressively clear-sighted introduction by Morgan & Demuth ('Signal to syntax: an overview'), Section 1 ('The nature, perception, and representation of input speech') includes chapters that deal with many of the foundations of this enterprise. Eimas ('The perception and representation of speech by infants') summarizes the vast literature on infant perceptual abilities. Dresher ('Introduction to metrical and prosodic phonology') describes the intricacies of the adult phonological representation to illustrate the complexities of what phonological cues may be present for the child. Lieberman ('Some biological constraints on the analysis of prosody') addresses the question of modularity and how to tease apart linguistic from non-linguistic perception which leads to a discussion of the question of species-specificity of linguistic abilities. Price & Ostendorf ('Combining linguistic with statistical methods in modelling prosody') introduce the complexities of the nature of the learning theory we must adopt

to account for how children interacting with a linguistic environment come to set up a complex hierarchical representation.

Section 2 ('Speech and the acquisition of words') looks more closely at how children process the incoming speech stream and identify word-sized chunks that can be stored in the lexicon. Cutler ('Prosody and the word boundary problem') discusses much of her own vast research programme and points out that while adults have the advantage of possessing a lexicon which aids in segmentation of the input, children are both setting up a lexicon and learning to segment the input. She looks at how prosodic cues can help children in the explicit segmentation of the input stream into words. Mehler et al. ('Coping with linguistic diversity: the infant's viewpoint') summarizes the results of a variety of experiments that suggest that children are sensitive to the rhythmic type of the language they are exposed to (e.g. stress-, syllable-, or mora-timed). This may be an ability that aids the child in separating multilingual input and to avoid mixing systems. Aslin et al. ('Models of word segmentation in fluent maternal speech to infants') presents an analysis of the acoustic properties of infant-directed speech that concludes that word boundaries are not enhanced and that difficult-to-segment words are not avoided. Often, though, target words were presented in utterance-final position. Bernstein Ratner ('From "signal to syntax": but what is the nature of the signal?') calls for further research into the precise acoustic properties of child-directed speech as the signal can be untidy or noisy at times. Echols ('A role for stress in early speech segmentation') demonstrates how children are sensitive to syllables which are stressed and that they segment stressed syllables first. She also raises the issue of a trochaic template that has been used to account for the fact that children from a variety of langauges tend to delete unstressed syllables that occur before stressed syllables more often than those that occur after stressed syllables. The existence of a trochaic template in Universal Grammar is a contentious one, though, as Archibald (1996) points out. Demuth ('The prosodic structure of early words') shows that while children's early productions may be ill-formed from a segmental, syllabic or morphological point of view, they are prosodically well-formed minimal words. As such, the form of the output is deemed to result from constellations of constraints on output forms. Demuth's research programme is consistent with much current work in Optimality Theory, and I think we can look for much more work within this framework in the near future.

Section 3 ('Speech and the acquisition of grammatical morphology and form classes') looks at how children acquire grammars that include grammatical categories. Selkirk ('The prosodic structure of function words') provides an outline of the prosodic hierarchy that illustrates some of the differences between the behaviour of functional categories as opposed to lexical categories. This is crucial because current versions of syntactic structure admit a wide range of non-lexical categories. Little attention has been paid to the kinds of cues (either semantic or phonological) that would

tell the child to set up these categories. Peters & Strömqvist ('The role of prosody in the acquisition of grammatical morphemes') propose the 'Spotlight Hypothesis' that suggests that prosodic patterns (pitch contours, duration, etc.) can serve as spotlights on the phonological forms that are associated with these patterns. This may help to account for the cross-linguistic variation in the acquisition of grammatical morphemes. Leonard & Eyer ('Deficits of grammatical morphology in children with specific language impairment and their implications for notions of bootstrapping') focus on the problems that children with specific language impairment have with grammatical morphology. They suggest that the children's inability to process morphemes of short duration (which in English are often function words) causes problems in allowing the children to recognize boundaries between syntactic constitutents. Kelly ('The role of phonology in grammatical category assignments') explores the notion that there may be reliable phonological cues to grammatical categories within a language. For example, in English nouns have more syllables than verbs, and nouns are more likely than verbs to have nasal consonants. These patterns, he claims, may provide the child with the impetus to set up different grammatical categories. Morgan et al. ('Perceptual bases of rudimentary grammatical categories: toward a broader conceptualization of bootstrapping') argue that there is a set of cues (statistical, phonological, and acoustic) which, while they are not highly valid individually, may act together to provide the learner with evidence that the ambient language has two distinct categories of function and content words.

Section 4 ('Speech and the acquisition of phrase structure') investigates the acquisition of syntactic structure above the word level. Venditti et al. ('Prosodic cues to syntactic and other linguistic structures in Japanese, Korean and English') discuss the mappings of phonological groupings and syntactic ones. They argue that cross-linguistically the complexity of these mappings make them unreliable cues to setting up syntactic structure. They prefer to view phonological grouping as highlighting information structure. Mazuka ('Can a grammatical parameter be set before the first word? Prosodic contributions to early setting of a grammatical parameter') investigates the setting of the head-direction parameter. She argues that children are able to set the syntactic parameter of branching direction prior to the onset of the one-word stage using suprasegmental cues. Once this has been set then the value of the head direction parameter falls out deductively. Steedman ('Phrasal intonation and the acquisition of syntax') points out that the assumption that phonological phrasing provides accurate clues to syntactic structure needs to be made with care. Intonational structure, in fact, may be orthogonal to syntactic structure. Intonational structures allow freer bracketing of the string than syntactic structures do which may present a problem for the notion of phonological bootstrapping. Fisher & Tokura ('Prosody in speech to infants: direct and indirect acoustic cues to syntactic structure') analyse infant-directed speech in English and Japanese and point

out that the short sentences of motherese combined with ellipsis in Japanese and pronominal subjects in English make the prosodic cues to syntactic structure of lesser utility to the child. They suggest that the child's sensitivity to distributional patterns at other linguistic levels may help to overcome this. Fernald & McRoberts ('Prosodic bootstrapping: a critical analysis of the argument and the evidence') maintain that researchers arguing for the phonological bootstrapping hypothesis have been oversimplifying the nature of the input data. They remind us that much of the input directed at children consists of sub-clausal fragments so that the child's task is really to listen to short utterances separated by long pauses. How this leads to the induction of hierarchical syntactic structure remains unclear, according to these authors. Jusczyk & Kemler Nelson ('Syntactic units, prosody, and psychological reality during infancy') summarize much of the perception literature (a great deal of it done by Jusczyk and his colleagues) and note that infants are sensitive to a wide range of prosodic phenomena. Furthermore, they argue that this prosodic information may well be a useful guide in helping the child to segment the speech stream and thus discover syntactic structure. However, they suggest that these prosodic markers are only one source of information that the child makes use of.

Section 5 ('Speech and the acquisition of language') consists of three more general papers. Gerken ('Phonological and distributional information in syntax acquisition') addresses how children (1) segment the speech stream, (2) label grammatical categories, and (3) set up hierarchical structure. She argues that the child makes use of prosodic cues in the first task, identifying function morphemes and their distributional properties in the second task, and cross-sentence comparisons to accomplish the third task. She argues, then, that learners incorporate phonological and distributional information to arrive at adult syntactic representations. Werker et al. ('Putting the baby in the bootstraps: toward a more complete understanding of the role of the input in infant speech processing') outline their own broad research programme on the nature of infant perception and how it changes over time based on interaction with the ambient language. They also call for more investigation into domain-general abilities of children. Much of linguistic inquiry sides with a modular approach to the mind, but surely we will add depth to our understanding as we come to know more about how the modules interact. In the final paper Hirsh-Pasek et al. ('Dynamic systems theory: reinterpreting ''prosodic bootstrapping'' and its role in language acquisition') look at the acquisition literature in light of the dynamic systems of Thelen & Smith (1994). The theory attempts to account for how information from the syntax, semantics, morphology, phonology, and social context are all utilized. While I admit I am unfamiliar with the seminal Thelen & Smith book, this paper is reminiscent of current work in phonology (e.g. Goldsmith 1993; Prince & Smolensky 1993), and the current acquisitionist concerns of emergentist perspectives.

In conclusion, this is clearly an impressive collection that deals with one of the greatest linguistic puzzles of our time. Given the status of the syntax/LF and syntax/PF interfaces in recent minimalist syntax, it seems unlikely that questions related to the integration of semantic and phonological cues to syntactic structure will lessen in import. The editors deserve congratulations for producing such a fine work.

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William C. Ritchie & Tej K. Bhatia (eds.), *Handbook of second language acquisition*. San Diego: Academic Press, 1996. Pp. xxv + 758.

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The 19 contributions in Ritchie & Bhatia's *Handbook of second language acquisition* are intended to represent the state of the art in second-language (L2) research. The tome's 758 pages are hardly the size of the prototypical handbook, so either the field is incredibly vibrant, or the volume has some fat that might be trimmed. In fact, I think both options are true: This volume does reflect a vibrant research area, but it could also lose a bit of weight. To cover this material, I first provide a paper-by-paper sketch (cum praise or critique, as needed) and then present the good news and the bad about the volume as a whole.

The volume opens with Ritchie & Bhatia's introduction, a full-length contribution presenting the major trends and issues of the field along with an historical overview. In their view (and that of many others), L2 research has changed fundamentally over its relatively short history, from a praxis-bound auxiliary of the language-teaching profession to an area of basic research into the language potential. Ritchie & Bhatia also provide the expected in-

troductory overview of the papers in the volume, which they organize into seven (sometimes dubious) sections, each containing one or more contributions.

The Ritchie & Bhatia introduction is a competent and valuable overview of issues, but the first must-read chapter – the only paper in section one – belongs to Gregg, who considers the logical and developmental problems of L2 knowledge. For example, Gregg notes that a theory of L2 knowledge, whatever form it may ultimately take, cannot be judged on the basis of Pinker's Learnability Condition, requiring across-the-board success. As Gregg points out, the consequences of this difference are omnipresent; because of it, one consistently finds appeals to critical periods, crude psychological concepts like intelligence and self-image, primitive social-psychological notions like acculturation, and so forth. He then goes on to offer a number of criteria for the development of a theory of L2, many interrelated in that particular choices on some mandate choices on others. The verdict: Gregg is required reading for newcomers; it wouldn't hurt some old-hands to read it, too.

The next six chapters comprise section two of the 'issues of maturation and modularity'. The first of these chapters joins Gregg's contribution as a must-read: White's overview of L2 research in the principles and parameters (P&P) tradition. After a cursory illustration of P&P-style grammars, White presents the three broad positions on 'access' to Universal Grammar (UG): full access (UG fully available to both first-language [L1] and L2 learners; no L1–L2 competence differences); no access (L1-L2 competence differences abound because UG is not available to adult learners); and UG mediated (L1 and L2 grammars may differ, but UG underlies both). White then presents a fair and open review of research findings both for and against these three positions. Where White covers more recent work is in the latter half of the chapter, where she reviews studies on L1-L2 and child-adult competence differences as well as on the roles of maturation and functional projections.

If White's overview is more balanced, Flynn and Schachter argue in their chapters for particular positions, Flynn for her full-access stance and Schachter for maturational decline. Alas, neither is entirely convincing. Flynn's presentation is marred by the same lapses noted by the several respondents to her more in-depth work (with Martohardjono & Epstein, in press). Schachter's proposal is better in this regard, yet her underlying linguistic assumption – that individual principles of UG are instantiated in particular languages only if triggered by exposure (e.g., *wh*-word displacement in exposure triggers Subjacency) – is not without problem, especially in light of Minimalism. Schachter's assumptions about critical periods are also suspect, if only because they are so vague.

The authors of chapters six, seven and eight do not assume the involvement of an innate Universal Grammar; all three represent, for better or for worse,

little more than footnotes in today's research community. (So much for 'issues of maturation and modularity'.) In his contribution, for instance, Eckman recounts his earlier work on implicational universals, especially his Markedness Differential Hypothesis and his Structural Conformity Hypothesis. (He calls this a 'functional-typological' approach, though he doesn't get around to explaining what is functional about it.) By contrast, McLaughlin & Heredia suggest that L2 acquisition and use are best characterized in terms of information processing: short-term and long-term memory; learning and automaticity; restructuring; and practice, repetition, and time on task. Alas, they don't get around explaining anything about L2 acquisition. (Both Eckman and McLaughlin & Heredia are thankfully brief.) Finally, Preston discusses variationist models and L2 acquisition. Here we hear a great deal about L2 acquisition, most of it exacting VARBRUL-aided descriptive statements on variation in language use. Toward the end of his paper, Preston also examines why variationist thought has not penetrated L2 research to any great extent. The one cause he seems to overlook is that, for all of its laudable descriptive advances, variationist work has produced little in terms of explanation.

Section three is to include work on L2 'speech' and L1 influence. The 'speech' paper is Leather & James' overview of a huge literature (and a 16-page bibliography with nearly 300 entries). Most of their review involves articulatory and acoustic phonetic research on what they call the 'mastery' of L2 'pronunciation' and its various determinants (L1, but also context, motivation, register, etc.). More explanatory approaches to the matter are considered only very briefly at the end of their paper. The other contribution in the section is Gass's contribution on L1 influence – another must-read in the volume. She examines L2 research more generally and L1 influence more specifically in a historical context. After a short discussion of contrastive analysis (1950s, 1960s) and creative construction (1970s), Gass provides more depth on the various and more recent cognitive approaches to the transfer phenomenon, including especially the role of transfer in UG-based approaches and in approaches that assume the Bates & MacWhinney Competition Model.

Section four, with two contributions, is on 'research methodology and its applications'. Bets are that placing contributions under this label is not going to elicit much itchy-fingered motivation among readers. And in the case of Nunan's contribution, they wouldn't be missing that terribly much. To be sure, Nunan does provide brief, though competent coverage of quantitative versus qualitative research, elicited versus naturalistic data, and so forth. But suggesting, as Nunan does, that outdated notions like 'creative construction' constitute 'substantive issues in L2 acquisition research' is not awe inspiring. Not so with the second contribution in the section, that of Sorace, who produces another must-read for the volume. Sorace provides a thorough and thought-provoking discussion of perhaps the most common source of data

in L2 research, the grammaticality judgment. After showing that interpreting such data is not nearly as straightforward as one might like, she suggests the use of magnitude estimation, a technique borrowed from psychophysics.

The two papers in section five are said to deal with 'modality and the linguistic environment'. Since the two concepts are almost assuredly related theoretically, one might expect work that addresses the relation. Not so. What we get instead is one contribution on each of the topics. (The problem is grouping into dubious sections, not the quality of the individual contributions.) In his contribution – another must-read – Long provides a thorough discussion of the role of input in L2 development, especially the notions of comprehensibility, attention and awareness as well as the standard classifications of input as positive, negative and explicit data. In the end, Long proposes an updated and provisional Interaction Hypothesis: Negotiations for meaning that result in interactional adjustments facilitate acquisition by connecting input, internal learner capacities and output. The modality contribution is Berent's work on near-deaf learners of (spoken) English – hence not a switch in sensory modality (as with deaf learners of sign), but a severe deprivation in a modality. After a review of older, descriptive work on the linguistic difficulties of these learners, Berent turns to more theoretical analyses. In particular, he follows Radford's well-known proposal on L1 acquisition to suggest that the less advanced near-deaf learners lack functional projections. He also describes work on the Subset Principle, suggesting that where mature English displays more marked values on various parameters, the less advanced near-deaf learners employ less marked values.

Like the first section, section six, the 'neuropsychology' of L2 acquisition and use, has only one paper: Obler & Hannigan on neurolinguistic research. At 14 pages, this is the shortest contribution in the volume, and Obler & Hannigan spend a good part of it dealing with older work on cerebral dominance, attrition and recovery from aphasia. They do bring up the critical period phenomenon but their treatment remains largely at the level of behavioral effects and psychological profiles of more successful L2 learners. They make only passing mention of work in the neurobiology of L2 learning; they do not discuss research on the neurobiology of critical periods.

The final section includes four contributions dealing with 'language contact and its consequences'. Andersen & Shirai's contribution on primacy of aspect – yet another must-read – is the first in this section, and it only briefly addresses language contact (pidgins and creoles) per se. After reviewing the controversy surrounding the notion and providing definitions for tense/aspect and (Vendlerean) aspectual classifications, Andersen & Shirai review extensive research on L1 and L2 development, where they find support for the notion. The final parts are devoted to explanation: Andersen's own input-distribution account, and an account based on prototype theory. The Andersen & Shirai contribution is a good example of careful reasoning. It's

also dense – in stark contrast to the next contribution in the section, Romaine's discussion of 'bilingualism', which could just as well have appeared as a lengthy newspaper exposition. In it, Romaine covers, breezily, areas like the composition of bilingual communities, diglossia, measuring bilingualism, maintenance and bilingual education.

The final pair of contributions in the section are Seliger on primary-language attrition in bilingual contexts and Bhatia & Ritchie on switching/mixing research. Seliger paints a picture of the progress of attrition in which the speaker-external contexts that define the uses of the speaker's different languages become 'confused and finally unified into one system'. When this happens, the speaker's languages become a single unit, and, according to Seliger, the speaker is then free to 'prune' those linguistic elements that are redundant. While readers may at times be misled by Seliger's discussion (his references to the logical problem and to under-determination are rather opaque, for example), his thesis is interesting, if perhaps less than perfectly supported. The final must-read in the volume is Bhatia & Ritchie, which presents a concise overview of research from early ideas on switching – that it is random and unconstrained – to the more recent and theoretically more interesting work showing that constraints derived from UG may be involved. Bhatia & Ritchie do not ignore the somewhat softer constraints on switching imposed by, for example, discourse or psychosocial attitudes in their work, however. For the development of switching in the course of L2 acquisition, they also present an interesting model that may well have ramifications for L2 research conducted outside of the switching framework.

In the final analysis, there is good news and bad news about the Ritchie & Bhatia volume. First and foremost, the Ritchie & Bhatia volume includes a number of quite good review papers, though, as the discussion above might suggest, certain other of the contributions add only very little. Other positives include the extensive subject index, the chapter outlines included in the table of contents and at the beginnings of (most) articles, and a glossary of terms. In addition, the editors also include extensive cross-referencing to other contributions in the volume. Further, with the exception of certain bibliographical problems (Flynn; Bhatia & Ritchie), the volume is also fairly free of local editorial and typographical errors. I also think it important to point out that the volume is reasonably priced (in today's inflated market) at just under US $70.00 for a well-made hardcover. The editors' organization of contributions into sections is sometimes a bit dubious, but if there is a more serious shortcoming in the volume, it is this: In a research arena in which the critical period phenomenon plays such an important role – nearly every author brings it up, if only in passing – the volume includes no contribution that tackles the phenomenon head-on.

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Nikolaus Ritt, *Quantity adjustment: vowel lengthening and shortening in Early Middle English*. (Cambridge Studies in Linguistics: supplementary volume). Cambridge: Cambridge University Press, 1994. Pp. x + 206.

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It will soon be a century since the Viennese philologist Karl Luick published his suggestion that the major vowel length changes in late Old and early Middle English conspired to standardize the quantity of stressed syllables according to their prosodic environment (Luick 1898). Luick's insight continues to haunt English historical linguistics (see e.g. Lass 1992: 70–76), but translating it into a descriptively adequate account has proved a difficult task. Ritt's volume (appropriately enough, a revision of his 1990 Viennese doctoral dissertation) presents itself as an attempt to validate Luick's insight within a theoretical framework unfettered by the stringencies of the Neogrammarian concept of Lautgesetz. The book seeks to fulfil the following tasks:

(i) to collect a theory-neutral body of data on vowel shortenings and lengthenings in early Middle English;
(ii) to identify and describe the factors at work behind such vowel length changes with the aid of modern non-linear phonological theory;
(iii) to show that Luick was right in claiming that the changes were part of a single major process of quantitative adjustment.

Ritt's work is based on a statistical analysis of four corpora, offered to the reader in a conveniently annotated form as appendices. Following the practice of Minkova (1982), the corpora consist of Present-day English wordforms compared with their etyma. There is a risk, in such long-term comparisons, that historic dialect mixture may introduce a bias in the data, but Ritt devotes most of chapter 1 to arguing that such risk is minimal. Ritt also claims that Luick's insight has failed to be successfully developed partly because of the straitjacket of the four Neogrammarian Lautgesetze posited

to describe the Middle English quantity adjustment: Homorganic Lengthening (HOL), Shortening before Consonant Clusters, Trisyllabic Shortening (TRISH) and Open Syllable Lengthening (OSL). According to Ritt, these Lautgesetze have supplanted the first-hand data they seek to describe (4–5). It is therefore rather contradictory that Ritt should build up his four corpora entirely from words whose etyma fulfil the structural descriptions of these four Neogrammarian sound laws. The quantitative behaviour of vowels in other potentially relevant environments fails to be represented in the database, which consequently does not allow a number of hypotheses to be tested. This flaw is a serious one: Ritt's description of the Middle English quantity adjustment implies some (largely unacknowledged) predictions concerning the probability of lengthening in stressed monosyllables with a VC rhyme, but his database provides no evidence for the historical behaviour of this phonological class.

There are also grounds for concern in the statistical handling of the data. According to Ritt (29), his corpus of OSL candidates (appendix I) consists of 428 items, largely drawn from Minkova (1982), and enriched with Anglo-Norman loans from Bliss (1952/3) plus a few of Ritt's own additions. By our own count, however, Appendix I contains 432 entries, apparently comprising most of the 326 items listed in Minkova (1982: 33–40), plus Minkova's own choice of 107 Anglo-Norman tokens from Bliss (Minkova 1982: 54). The discrepancy is only partly explained by the fact that words with vacillant vowel length in Present-day English (e.g. *azure* and *besom*) are given two entries in the appendix. Throughout chapter 2, some tables support the total of 428 (e.g. table 2.1, 30), whilst others imply 432 (e.g. table 2.4, 33). In table 2.9 (36), which considers the behaviour of OSL candidates according to their etymological source, the total amounts surprisingly to only 422 or 423; there is no explicit indication of the fact that those items whose etymology is uncertain have been excluded.

A comparison of Ritt's figures for HOL with those of Minkova & Stockwell (1992) reveals further shortcomings in his handling of corpus material. Minkova & Stockwell claim that only 77% of surviving Old English words show up in Present-day English with lengthened *u* before *nd*: the unlengthened 23% consists of *sunder* and *wonder*, where the cluster *ndr* present in related forms may have had a blocking effect (1992: 198, 204). In contrast, Ritt's table 6.3 (87) claims that 100% (9 out of 9) of Present-day English reflexes show lengthening of *u* before *nd*, even though no less than six unlengthened reflexes appear in appendix II: *bundle*, *hundred*, *sunder*, *trundle*, *under* and *wonder*. This discrepancy arises from a briefly mentioned and ill-justified decision to exclude words with historically stable final syllables from the reckoning (confusingly, 'stable' on page 87, line 15, is a misprint for 'unstable'). Traditionally, lengthening in forms such as *bundle* or *hundred* has been seen as blocked by the third consonant immediately following the homorganic cluster. If true, this would mean that the lengthening of the

vowel was inhibited by the composition of the ONSET of the next syllable. Ritt's treatment of the evidence leaves this quirk of HOL unacknowledged and unexplained.

In spite of these statistical problems, Ritt's fresh look at the first-hand data produces some important results. Most notably, he observes that the traditional case for positing TRISH as a Lautgesetz rests almost entirely on Present-day English forms such as *chicken* (< *cīcen*) or *bosom* (< *bōsm*), where the short vowel is assumed, without independent justification, to have been analogically levelled from trisyllabic inflected forms: e.g. *cicenu* and *bosome*. Given the extreme scarcity of uninflected trisyllables in early Middle English, Ritt describes TRISH as a 'sound change without inputs' (104); see further Minkova & Stockwell (1996).

The non-linear phonological theory used in the book is dated. The notion of the foot with which Ritt works is, for all intents and purposes, that of Abercrombie (1965): a string consisting of a stressed syllable followed by any number of unstressed syllables up to, but not including, the next stress. Similarly, his approach to syllable quantity ignores Moraic Theory (Hayes 1989): the term 'mora' is used as mere short-hand for rhymal segmental position, 'mainly to avoid the use of more clumsy phrasings', and without claiming 'any theoretical significance' (60). As a consequence, non-linear representations often serve as little more than visual aids (see e.g. 70).

This ad hoc approach to formalism results sometimes in glaring idiosyncrasies. The assumption of ambisyllabicity, for example, allows Ritt to speak of syllables as weighing '$2\frac{1}{2}$ moras'. In chapter 4 it is suggested that in the word *resten* the whole *st* cluster is ambisyllabic. This is represented by a tree where *s* associates to the coda of the first syllable and the onset of the next, whilst *t* does the same (59). An accompanying endnote contains the following cavalier injunction: 'Never mind the crossing branches. The purpose of the employed notation is just to make clear that each of the elements in the intermediate cluster ''belongs'' both to the first and the second syllables' (180). The inverted commas around *belongs* suggest perhaps that standard notions of constituency do not apply here. Ritt's insouciance is surprising, since the irregular behaviour of vowel length before *s* + stop clusters is meant to be explained by the alleged ambisyllabicity of the latter, and to provide Ritt's main argument against maximal onsets.

In general, the book's overall view of prosodic structure is anchored in the mid to late eighties (see e.g. a list of works on suprasegmental phonology in endnote 5, page 178, where the latest references are to two 1988 publications). Thus, the author ignores fundamental theoretical developments which have a bearing on his subject: notably, Ritt seems unaware of the analysis of TRISH as a form of Trochaic Shortening made possible by Hayes' asymmetric foot typology (Prince 1990).

Ritt claims that all the major late Old and early Middle English vowel length changes were manifestations of a single historic process of Middle

English Quantity Adjustment (MEQA), controlled by a 'probabilistic law' stated informally as follows (75):

(1) The probability of vowel lengthening was PROPORTIONAL to
  (a) the (degree of) stress on it
  (b) its backness
  (c) coda sonority
  and INVERSELY PROPORTIONAL to
  (d) its height
  (e) syllable weight
  (f) the overall weight of the weak syllables in the foot.

This law is not regarded as effecting either shortenings or lengthenings, but rather as regulating the likelihood of their occurrence (97). Ritt describes it as constituting a combination of universal 'tendencies' grounded in human physiology and psychology. In the algebraic formulation of the law, the relative 'weight' of each factor is expressed by a numerical constant, whose value Ritt leaves provisionally indeterminate (75). Such a combination of numerically weighted universal tendencies in a single probabilistic generalization is put forward as the main theoretical contribution of the volume. It is, however, fraught with theoretical and empirical difficulties.

To begin with, the use of probabilistic laws to describe sound changes is unhelpful in that it obscures the relationship between synchrony and diachrony. There appears to be no compelling evidence that the synchronic grammars internalized by native speakers incorporate any sort of probabilistic device for computation. The bearing of Ritt's diachronic generalization on speakers' competence remains, in this sense, unclear. Synchronically, moreover, the use of numerical weighting as a formal mechanism to resolve tendency clashes would undoubtedly result in an explosion of possible grammars, and hence in insurmountable problems for learnability theory.

By definition, Ritt's probabilistic law does not predict regular change in the Neogrammarian sense. This, however, does not mean that MEQA conforms with standard theories of lexical diffusion. If, for example, a probabilistic law assigns a 60% probability of occurrence to lengthening in a certain environment, is one to assume that lengthening will propagate through the lexicon in a familiar S-curve tailing off towards 60%, rather than 100%? Ritt fails to address this problem of implementation.

Vagueness is the most essential disadvantage of MEQA. As a probabilistic law, it cannot be falsified by individual wordforms. Yet statistical corroboration is also impossible, except in the most general terms, because the numerical weighting of parameters has been left indeterminate. Under such conditions, one cannot ascertain whether, as Ritt assumes, each of the tendencies he invokes exerted the same degree of influence upon all the changes. In fact, such vagueness lends itself to equivocation. Ritt is aware

that clause (d) of MEQA clashes with the fact that, according to Present-day English data, only high vowels appear to have lengthened before *nd* (ch. 6). To salvage the unity of the process, he invokes the interference of factors specific to nasal clusters, which would lie beyond the purview of MEQA (91–92). This gambit illustrates the loss of content which grammars incur by resorting to statements of tendency; as Prince & Smolensky point out, 'Linguistic theory cannot be built on "laws" of this sort, because they are too slippery, because they contend obscurely with partly contradictory counter-"laws", because the consequences of violating them cannot be assessed with any degree of precision' (1993: 197–198).

Such vagueness aside, parameters *e* and *f* suggest an essential continuity between Ritt's proposals and those of Luick (1898). In this sense, the absence of lengthening in stressed monosyllables with VC rhymes, for which Luick could not provide a convincing treatment (see Luick 1898: 337), continues to plague Ritt. There is no consistent evidence to suggest that words such as *pic* 'pitch', *pæð* 'path', *God* or *man* experienced any pressure to undergo lengthening in Middle English. Given Ritt's assumption of ambisyllabicity, however, MEQA would appear to suggest that lengthening in this environment was even more likely than in [*ta*[*l*]*u*] 'tale' or [*na*[*m*]*a*] 'name'.

In other respects, Ritt's emphasis on the unity of the late Old and early Middle English vowel length changes out-Luicks Luick. Luick viewed the English *Quantitätsveränderungen* as conspiring to achieve a single goal, but was reconciled to the fact that their application was not historically simultaneous: 'Dass die einzelnen akte nicht gleichzeitig eingetreten sind, kann kein argument gegen diese zusammenfassung ergeben. Es ist nicht verwunderlich, dass sich eine solche uniformierungstendenz in mehreren absätzen bahn bricht' [The fact that the individual processes did not occur simultaneously can provide no argument against this unification. It is not surprising that such a tendency towards uniformity should make its way ahead in several stages] (Luick (1898: 338); see also Lass (1992: 70–71)). Ritt, in contrast, regards MEQA as a monolithic entity, and accordingly finds handbook chronology (particularly the dating of HOL in the ninth century) something of an embarrassment (82–83, 92–93). The issue remains open, particularly in the light of Hogg's postulation of an Old English phonetic forerunner of Middle English OSL (Hogg 1996). Ritt also assumes that the quantitative changes under consideration ceased to be productive when, during the Middle English period, the isomorphic relationship between feet and wordforms became disrupted: wordforms, he claims, 'ceased to be a domain for the prosody-based constraints on Quantity Adjustment' (122). This assumption is chronologically dubious, since the initial-stress pattern remained productive throughout Middle English.

Although the volume under review focuses on an area of considerable topical interest, probabilistic laws such as Ritt envisages are not viable tools for historical linguistics. They do not advance our understanding of

optimization in sound change. It is consequently ironic that this book should have seen the light shortly after Prince & Smolensky's (1993) theoretical breakthrough.

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Izchak M. Schlesinger, *Cognitive space and linguistic case: semantic and syntactic categories in English*. Cambridge: Cambridge University Press, 1995. Pp. xiv + 239.

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This book concerns cases (or thematic roles) and their relationship with cognition, mainly in English simple clauses. Linguistic generalizations, which are here largely theory-neutral, are claimed to be motivated by psycho-linguistic evidence, from first language acquisition in particular. The main

contribution of this work should, therefore, stem from its interdisciplinary orientation and thus be of interest to both linguists and psycholinguists. The problems that lie at the intersection of these two disciplines belong to some of the most exciting areas of current research into the nature of human cognition. Unfortunately, this work has a number of shortcomings that greatly diminish its overall value.

The first chapter explores the nature of cognitive categories and sets the stage for developing the concept of 'case' in Chapter 2, in which the Agent case and its relation to the syntactic category 'subject' is discussed. In Chapter 3 the proposed approach is applied to the Instrument and Comitative. Chapter 4 concerns the Instrument case in subject position. A new case category, 'Attributee', is introduced in Chapter 6. This case is used in Chapter 7 in the analysis of psychological predicates. Chapter 8 focuses on the direct object. Chapter 9 explores the prototypical structure of linguistic categories. In the final chapter, the results of the proposed analyses are reviewed and some areas of further research addressed.

The focus of this review is on the first two chapters, 'Cognitive space' and 'Agent and subject', where the theoretical background and the main arguments are laid out. As a working hypothesis, Schlesinger proposes that cases should be defined 'in such a way that syntactic categories turn out to be maximally homogeneous in terms of the case categories they express' (30). While the subject, for example, turns out 'to be much more homogeneous than is usually assumed' (2), '[t]he notions expressed by direct objects are so variegated that they have foiled our attempt to find a set of features that characterize at least the most typical members of this category' (179). Although Schlesinger is aware that the homogeneity hypothesis is not defensible in its strongest form, he attempts to defend at least the claim that a theory that is closer to the ideal of a one-to-one mapping between semantic and syntactic categories is to be preferred, because it allows for a simpler and more plausible explanation of first language acquisition (1–2).

Schlesinger distinguishes three levels of linguistic description: a cognitive (or conceptual), semantic and a linguistic expression level. He uses the term 'cases' for linguistic constructs on the semantic level that mediate between cognitive and linguistic (syntactic) categories (30–1). Similarly to Cruse (1973), for example, Schlesinger characterizes cases as cluster concepts with a graded structure that comprises cognitively anchored primitive features (30, 58, and elsewhere). This conception of cases is opposed to one which regards cases as cognitive or conceptual categories and assumes a single cognitive-semantic level that is mapped (directly or indirectly) into a syntactic level (compare Fillmore 1968; Jackendoff 1983, for example).

The point of departure for the chapter 'Agent and subject' is the subject selection of converse verbs (e.g. *buy-sell*) and symmetric verbs (e.g. *collide*). Such verbs pose difficulties for any linking theory, because they have more than one argument that seems to lay the same claim to subjecthood. When

faced with such data 'the current conception of cases' (29) is, according to Schlesinger, doomed to failure, because it adheres to the following 'rule of case grammar' (30): '[w]hen two sentences differing in syntactic structure have the same truth value (…), the corresponding noun phrases are necessarily assigned the same case' (29). Schlesinger also argues that subject selection in terms of thematic hierarchies (28–29) and the relative saliency of the entities involved, proposed by Fillmore (1977) and Dixon (1991), for example, is inadequate to account for converse and symmetric verbs (55). Moreover, 'the current conception of cases rests on the implicit assumption that it is possible to categorize [semantic] relations that are "out there"' (29), and therefore it arrives at classifications into cases that are merely naive and common-sense ontologies.

In order to overcome such problems, Schlesinger stipulates that the assignment of case features and cases is determined by the verb and its lexical entry (43). Contrary to '[t]he assumption made in previous writings on case theory that there must be a case for every noun phrase' (54), '[a] conceptual distinction is to be admitted as a case if and only if it subserves the statement of some linguistic regularity' (25) in a given linguistic expression (see the Principle of Linguistic Relevance, 26, 29–30). This leads Schlesinger to the conclusion that 'cases are language-specific' (24–5), and not cognitive or conceptual, and hence universal, concepts. Finally, if two or more arguments have the same case feature(s), and hence compete for assignment to one and the same case role, the conflict is resolved by three factors: '(i) the relative strength of features; (ii) their number; (iii) their differential weights' (45).

There are many problems with Schlesinger's account. I will only address four of them. First, a view of cases as cognitive or conceptual categories does not necessarily entail that they also must be viewed as universal categories, contrary to Schlesinger. For example, recent case theories assume a multilayered conceptual structure in which information about changes, causal relations, motion, volitionality, and the like are specified in terms of conceptual primitives; linking rules refer to arguments (or sets of arguments) in specific conceptual sub-structures. Both language particular and cross-linguistic linking generalizations can be stated in terms of such conceptual sub-structures.

The second main problem has to do with the non-standard and inconsistent use of the proposed case features. Schlesinger suggests that a prototypical Agent is a cluster concept characterized by the features CAUSE, CONTROL, and CHANGE, and any of these features 'is sufficient for making a noun phrase a candidate for the A-case [agentive case]' (35). Take the feature CONTROL, for instance: 'the test for CONTROL may be failed by a noun phrase that obviously does have this feature. Thus, one can normally avoid owning something – one can give it away, etc. – and yet (12b) [??Don't own this house] sounds strange' (34). The obvious mistake here has to do with the confusion between entailments of certain verbs, on the one hand, and general

world knowledge we have independently of our knowledge of the meanings of particular verbs, on the other hand. Although Schlesinger criticizes 'the current conception of cases' (which current conception exactly remains unexplained, however) for categorizing relations that are "out there"' (29), he himself appears to be falling into the same trap. Moreover, his criticism is hardly justified or relevant, since it is standardly recognized that case features and cases are best seen as choices inherent in the linguistic description of aspects of reality, as they are filtered through the verb meanings; or, in short, 'possible verbal entailments about the argument in question' (Dowty, 1991:552). The feature CHANGE is considered to be an AGENTIVE feature, even when it is associated with subject arguments that are entailed to UNDERGO a change of state or location, including those that are standardly regarded as Patients or Themes, such as *the vase* in (16c) *The vase broke* (35), rather than being volitional instigators of a change of state or location, such as *John* in (21a) *John runs five miles* (38). Matters get even more confusing when it is proposed that the feature CAUSE be used for subjects of middle verbs (145), as in (22a) *The woolens wash well*.

This use of case features disregards the well-accepted results in the rich literature on cases in the past twenty years or so. For instance, it is generally agreed that it is necessary to distinguish the following classes of verbs: (i) verbs that entail a change of location versus those that entail a change of state, (ii) verbs that entail a 'volitional involvement in the event or state', 'movement (relative to the position of another participant)' (Dowty's Proto-Agent properties) versus verbs that entail that one of their arguments 'undergoes a change of state' (Dowty's Proto-Patient property) Dowty (1991:572), and (iii) unergative versus unaccusative classes of verbs (see Perlmutter, 1978; among many others). Surprisingly, we are invited to view the disregard for such crucial distinctions as one of the strong points, rather than a weakness, of Schlesinger's approach. The reason is, according to Schlesinger, that this allows us to conceive of the syntactic category 'subject' 'as semantically much more homogeneous' (52), because, among other things, '[t]he concept of Agent has been redefined in a way that permits analyzing as Agents those subjects that have previously been accorded to other cases, e.g., the Instrumental and Patient' (57). Not only is such an argument unconvincing, but it contributes to the strong impression that the primary motivation for the selection and non-standard interpretation of case features is the maximization of the validity of the homogeneity hypothesis (i.e., one-to-one correspondence between semantic and syntactic categories). Given the lack of empirical motivation, the homogeneity hypothesis reduces at best to a methodological assumption.

The third problem concerns the stipulation that assignment of case features and cases is determined by the verb and its lexical entry (43). 'By referring to the lexical entry (…) the problem of converse verbs receives a straightforward solution' (55). For example, by virtue of the lexical

stipulation in the entry of *lead* the subject in *The officer is leading the band* has the agentive feature CAUSE, while the entry for *follow* 'specifies that the one who is ''going behind'' has the feature CAUSE (55). Hence, *the band* in *The band is following the officer* has the agentive feature CAUSE. Schlesinger claims that his lexical stipulation account of converse verbs is superior to 'the current conception of cases' that must assign *the officer* to the same case in both sentences, because they 'may be used to describe the same event' (55). This is allegedly enforced by 'the rule of case grammar' (29). Here, as in other places, it is not specified who endorses the erroneous 'current conception of cases' and the purported 'rule of case grammar'. At least since Fillmore's work in the seventies, it has been recognized that two sentences with different syntactic structures and the same truth-conditions may contain corresponding noun phrases that are not assigned to the same case. Take, for example, *Mary* in *Mary* (Agent) *sold a book to John for five dollars* and *John bought a book from Mary* (Source) *for five dollars*. Although truth-conditionally these two sentences may be regarded as equivalent and *Mary* can be viewed as fulfilling the same participant role of a seller in the commercial transaction event that the meanings of these two sentences share, with the verb *sell* Mary is associated with the Agent case role, while with the verb *buy* it is associated with the Source role. It is the task of the case and linking theory to MOTIVATE the assignment of *Mary*, qua seller, to two different case roles depending on whether it occurs as an argument of *sell* or *buy*. This is the problem that converse verbs of the *buy-sell* type pose and which needs to be solved. However, a mere lexical stipulation of the kind proposed by Schlesinger is clearly not a solution to this problem. Notice also that Schlesinger's lexical stipulation account predicts that the behavior of converse verbs of the *buy-sell* type is an idiosyncratic characteristic of the English vocabulary. This is certainly incorrect, given that similar classes of converse verbs with the corresponding semantic and syntactic argument structure can be found in a number of other languages. Clearly, the solution to the linking problems posed by converse verbs is to be sought in the cross-linguistic context and in terms of cross-linguistic, and even universal, generalizations. It is plausible to assume that the different saliency of participants in a given event or different pespectives on a described event also play a role in case assignment. For example, it may then be proposed that the different case role assignment to *Mary* in the above sentences with the *buy* and *sell* verbs reflects different perspectives on the commercial transaction event.

Schlesinger also proposes that case features can be acquired by what he calls 'contraction' across subentries of verbs. For example, the feature CONTROL, which is assigned to the human subject of *Gregory lay on the floor* by virtue of the lexical properties of the verb *lie*, is contracted by the inanimate noun phrase in the corresponding sentence *The city lies on the river estuary*. To claim that *the city* 'contracts' the agentive feature CONTROL

is unmotivated. Verbs of posture like *sit*, *stand*, *lie* are better analyzed as having an agentive (entailing CONTROL) and a non-agentive use (see Dowty, 1979:184). Moreover, it ultimately remains a mystery how exactly the device of 'contraction' works. Apart from a cursory suggestion that a feature can be contracted through the principle of metaphorical extension or 'by dint of similarity' (44), no further explanation is given. Among many questions the device of 'contraction' poses is the following one: How do we decide which subentry of a verb, such as *lie*, will inherently have the feature CONTROL and which subentry will contract this feature?

While the features of each verb are specified in the lexicon, the strength of a feature is determined by the particular sentence in which a given verb appears (41) or by the situation referred to (52). In this connection Schlesinger proposes that '[t]he entry for the verb *move*, for instance, will include an argument with the feature CHANGE, but the amount of motion, hence the strength of CHANGE, will depend on whether the sentence refers to a moving bus or a moving snail' (41), and that 'a noun phrase in motion (...) normally has greater strength of CHANGE (...) than one that only undergoes a change of state' (38). However, no conclusive linguistic argument is provided for these claims, which certainly are highly dubious. Taken literally, the weighing of features proposed by Schlesinger would seem to lead to the unjustified assumption that *John* in *John runs five miles* should have greater strength of CHANGE than *the vase* in *The vase broke*, for instance.

Finally, it is disappointing that we do not learn much about first language acquisition, which is claimed to be one of the main motivating factors for the proposed linguistic claims. Not only do most original psycholinguistic experiments reported in this book not pertain to first language acquisition they also do not seem to be convincing. In many cases psycholinguistic evidence is rather sketchy. For example, in section 5.3 'Implications for sentence production' (52), it is unclear what psychological reality, if any, is to be ascribed to the five steps described here, given that no empirical motivation is provided for them. Due to all of these problems, Schlesinger fails to achieve his goal of convincing us that his analysis of various phenomena of English syntax represents a significant contribution to the current research on case theory and cognition.

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Carson T. Schütze, *The empirical base of linguistics. Grammaticality judgments and linguistic methodology*. Chicago: University of Chicago Press, 1996. Pp. xv + 237.

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This excellent books fills an important gap in the literature of modern linguistics. Where else could you turn for serious discussion of our main data-source, grammaticality judgements? Even the ten-volume *Encyclopedia of language and linguistics* yields virtually northing – just four very brief references under 'grammaticality, intuitions of'. The fact is that most of us know that data is one of our main research problems, but we don't see it as a problem for research so we just stumble on, lurching from one data-crisis to another. If I think it's grammatical and you think it's awful, where do we go from here?

Schütze's main thesis is that linguistic theorizing has grown up, so it's time we used more grown-up data collection methods too. Thirty years ago linguistic theories were doing well if they could cope with even elementary patterns, so data was no problem. But nowadays any theory can accommodate sentences like *The farmer killed the duckling*, and the challenge is how to deal with the subtleties of binding, extraction islands and so on. The 'facts' are almost all provided by grammaticality judgements, and in some areas chaos reigns – theoretical points rest on subtle judgements that are themselves disputed, and no-one knows whether data disagreements reflect competence differences among the judges, wilful misreporting for the sake of one's favourite theory, or differences of methodology. Maybe different people are actually judging different things – semantics in one case, syntax in the other? Maybe even different minds are inherently sensitive to different properties of a sentence? Just to take one fascinating example out of a rich collection surveyed in this book (110), it seems that those with a history of left-handedness in their family are less sensitive than pure right-handers to the purely structural aspects of a sentence. If the status of an example

depends on the judge's handedness, where does this leave the theories that we base on these judgements?

The book is a thorough survey of a surprisingly rich research literature on grammaticality 'judgements' in a rather broad sense. It covers any kind of 'experimental' data-gathering in which people produce (to order) observable behaviour from which conclusions can be drawn about the status (in their competence) of some specific example. In naive cases the 'experiment' consists of a question: What do you think of this sentence? (or, worse, Is this sentence grammatical?) And in most cases, of course, the subject is also the experimenter. In more sophisticated research, the experimenter asks another person to perform some kind of operation on the sentence (e.g. changing its tense, or simply memorizing it). A large part of the literature is in psycholinguistics, where the relation between data and theory is the reverse of what we find in theoretical and descriptive linguistics – meticulous care over data, and great caution before formulating theories. I was particularly pleased to see the sympathetic treatment of the 'compliance tests' which were carried out in the 1960s and 1970s by Quirk, Greenbaum and their colleagues, but there is a great deal more besides, including the latest technology using brain-scans which can, apparently, show whether a sentence that we are processing is deviant grammatically or semantically (207)!

What the book does not try to survey is the use of corpora to supplement judgements, though it does agree that the more methods are used for checking a factual claim, the better. Moreover, one of the book's main themes is that we cannot assume a direct link between 'metalinguistic performance' (judgements) and competence, so judgements are no 'cleaner' than corpora. Schütze quotes one of the main researchers (Birdsong 1989) with enthusiasm (204):

> The hypocrisy of rejecting linguistic performance data as too noisy to study, while embracing metalinguistic performance data as proper input to theory, should be apparent to any thoughtful linguist.

It would be very good to have a companion volume on corpora now that there are vast corpora which can be searched quite easily by computer. It is still unusual to find such corpora quoted as evidence in an article on linguistic theory, outside the still rather specialised areas of historical linguistics and sociolinguistics. Most of us simply don't know what is available or how to use it. If judgements can't be trusted, we should feel better about using attested examples as evidence; and we can even test for non-occurrence of some pattern if we can calculate how often it SHOULD occur in a corpus of a given size. Moreover, the two methods can easily be combined by asking subjects to judge attested examples. If an example actually occurred, and was then accepted without protest by a relevant judge, then we really should take it seriously as data.

One of the most attractive features of the book is that it contains an 'executive summary' (section 6.3) which can be read on its own. This summarises the implications for our practice as linguists under three headings: materials (the examples that we ask people to judge), procedures (how we select subjects and what we ask them to do) and analysis and interpretation of results (how we use their judgements as evidence for analyses and theories). Each heading contains a list of practical recommendations. Basically their effect is to shift our data-gathering into the province of psycholinguistics. When in doubt about a pattern, construct a batch of examples, recruit a collection of suitable subjects, do the experiment, analyse the results statistically and report the outcome in such a way that the experiment can be replicated. As Schütze says (210):

> …linguists will have to be trained in areas that they traditionally have not been required to know anything about: statistics and experimental design in general, and the psychology of grammaticality judgements in particular.…every linguistics department should offer a course in experimental linguistics.…It would…seem to be a natural outgrowth of Chomsky's own suggestion that linguistics be viewed as a branch of cognitive psychology. Somehow, the focus on cognitive issues has not yet been accompanied by adoption of the scientific standards and concern with methodology of that discipline.

I have to confess that this recommendation fills me with horror. Just imagine how long it will take to do a piece of research! Instead of generating ten examples in a couple of minutes, those same examples will take me a month or two of research. Will this mean the end of linguistics as we know it? To calm my nerves I have to tell myself the following things:

- Experimentation is only needed where the data are troublesome. Where they really are uncontroversial business can go on as usual. Presumably we can normally assume data are uncontroversial until someone challenges them.
- Once one person has done the relevant experiment we can all build on their work, as in psychology; so we don't have to personally check every controversial sentence – though we are obliged to accept others' data unless we can produce equally solid counterevidence.
- Is it really better to carry on as at present? I can write an article based on my own data which others can reject out of hand simply because they disagree with my data, which is a waste of everyone's time. As far as I can see, Schütze's proposal removes the possibility of rejecting someone else's data simply because one disagrees with it.

Having said all this, however, I have one complaint about Schütze's recommendations. Why should we assume that data must come from a number of subjects? He himself accepts that individual speakers can have

unique grammars (201), and that a pattern may be true for some subjects even if other subjects disagree (personal communication); and the focus of most of our work is the individual speaker's competence, so there is no need to demonstrate that some feature is found across a range of speakers. (The same is even true in sociolinguistics if you think of linguistic variation as being located in the individual speaker rather than in the community – see Hudson (1996: 229).) Finding a feature in one person is enough to prove that it is possible in human language. What we need is a body of techniques for reliably exploring the competence of a single person.

This aim raises two serious questions. First, how can we guarantee replicability if other researchers do not have access to the same subject? (The same problem actually arises with groups of subjects: how can anyone else replicate my experiment without using the same group of subjects or one that is identical in the relevant respects, and how can we know what the relevant respects are?) And second, is it really wrong in principle for me to use myself as subject? This sounds dangerously comfortable, and Schütze certainly exposes the weaknesses of present practice, but is it not possible that some fertile imagination could produce a method that would avoid some of these weaknesses? It is easy to imagine ways in which present practices could be improved – for example, a simple piece of software which would automatically extract all examples from an article, strip off asterisks and question-marks, randomise the order and perhaps add some extra examples. Faced by such a list of examples to be judged a week after writing the draft, I wonder how many of us would give exactly the same judgements? In his final summary Schütze insists that subjects should be 'people with no linguistic training' (186), but his earlier survey shows that the research evidence is actually inconclusive. Linguistic training does seem to affect judgements, but it can be argued that it makes for more sensitive judgements (121), so maybe my judgements could be used after all?

Schütze's survey leaves no doubt that present practice is generally unsatisfactory, so we shall probably all have to change. If this does happen, it will no doubt be at least in part due to this book, which has virtues which I have not yet mentioned. It does not follow any party line. In general it accepts Chomsky's views on linguistic theory, and indeed it was written when Schütze was a student at MIT; apparently Chomsky even commented on the first draft of the book. Moreover, some of the most interesting points are where Schütze quotes very sensible views expressed by Chomsky on data-collection methods. (For example (19), in 1962 Chomsky wrote: 'I dislike reliance on intuition as much as anyone… We should substitute rigorous criteria just as soon as possible, instead of clinging to intuition'. And (36) in 1982 (33–34) 'It just seems absurd to restrict linguistics to the study of introspective judgements, as is very commonly done… Many textbooks that concentrate on linguistic argumentation, for example, are more or less guided by that view'.) And yet Schütze is equally influenced by Labov's

writings on method (though most of Labov's empirical work is irrelevant to this book), which many of us consider to be opposed to Chomsky's methods. This even-handedness is part of Schütze's generally open and scholarly approach in which the focus is on evidence and argument rather than on public relations. And finally, the book is well written – clear, entertaining (even funny in places), interesting and very thoroughly researched.

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This book is a revised version of Vikner's 1990 University of Geneva dissertation and, simply put, the best currently available survey of verb movement and expletive subjects in the Germanic languages. Given the recent explosion of work in these areas, that is no small accomplishment, and nobody working in Germanic syntax can afford to overlook the wealth of data and insightful analyses presented in Vikner's book, which sets new standards for the field of theoretically oriented comparative syntax.[1]

Two introductory chapters are followed in chapter 3 by a thorough review of the Verb Second (V2) phenomenon in Germanic matrix sentences. In keeping with much of the literature on this topic, Vikner analyses non-embedded V2 as X^0-movement of the verb to C^0 and XP-movement of either the subject or a topic to CPSpec. V-to-C movement is triggered by the

[1] The author of this review gratefully acknowledges comments by Hagit Borer.

presence of an inflectional feature in C^0 that needs to be lexicalized in order to be able to assign nominative Case to IPSpec under government (see below).

Chapter 4 extends this analysis to embedded clauses. In Mainland Scandinavian, the presence of a complementizer in C^0 blocks V-to-C movement (see den Besten 1981). Since these languages do not have V-to-I movement when V-to-C movement is blocked ('independent V-to-I movement'), embedded clauses in which material such as negation intervenes between IP and VP never surface with V2. There is one exception to this generalization: sentential complements of so-called bridge verbs sometimes surface with V2. Vikner follows Platzack (1986) in analyzing these cases as instances of selected CP-recursion. According to this analysis, bridge verbs select a CP-complement whose head can take another CP as its complement. V2 then takes place in the lower CP (see (1)).

(1) (a) $[_{VP}$ BRIDGE-V $[_{CP}$ $[_{C'}$ COMP $[_{CP}$ SUBJ$_i$ $[_{C'}$ V$_j$ $[_{IP}$ t$'_i$ $[_{I'}$ t$'_j$ NEG $[_{VP}$ t$_i$ $[_{V'}$ t$_j$]]]]]]]]]

(b) $[_{VP}$ BRIDGE-V $[_{CP}$ $[_{C'}$ COMP $[_{CP}$ TOP$_k$ $[_{C'}$ V$_j$ $[_{IP}$ SUBJ$_i$ $[_{I'}$ t$'_j$ NEG $[_{VP}$ t$_i$ $[_{V'}$ t$_j$ t$_k$]]]]]]]]]

In Icelandic and Yiddish, V2 is obligatory in all embedded clauses with the exception of certain embedded questions. Vikner analyses both complementizer-topic-verb sequences and complementizer-subject-verb sequences in these languages as cases of free (i.e. unselected) CP-recursion. The introduction of free CP-recursion is Vikner's major theoretical contribution to the V2 debate. Note however that at least for subject-initial embedded V2 sentences in Icelandic and Yiddish, there is a straightforward alternative to Vikner's analysis in (1a): both languages have independent verb movement to I^0 which can result in V2 in embedded clauses without verb movement to C^0, making CP-recursion unnecessary. This is illustrated in (2).

(2) $[_{VP}$ V $[_{CP}$ $[_{C'}$ COMP $[_{IP}$ SUBJ$_i$ $[_{I'}$ V$_j$ NEG $[_{VP}$ t$_i$ $[_{v'}$ t$_j$]]]]]]]

There is in fact evidence which suggests that Icelandic subject-initial embedded V2 clauses have a simple CP structure rather than a CP-recursion structure.[2] Platzack (1986) bases his argument for selected CP-recursion in Mainland Scandinavian embedded V2 clauses on the contrast between (3) and (4).

(3) $[_{CP}$ [Vilken fest]$_i$ $[_{C'}$ sa hon $[_{CP}$ t$'_i$ $[_{C'}$ att $[_{IP}$ vi$_j$ $[_I$ ∅] inte $[_{VP}$skulle
which party said he that we not should
$[_{VP}$ t$_j$ $[_{V'}$köpa roliga hattar]] $_{PP}$ till t$_i$]]]]]]]?
buy funny hats for

(Swedish, Holmberg 1986)

[2] The evidence is different in Yiddish. For arguments against free CP-recursion in this language, see Rohrbacher (1994: 68f.).

(4) (a) *[$_{CP}$ [Vilken fest]$_i$ [$_{C'}$ sa hon [$_{CP}$ t'_i [$_{C'}$ att [$_{CP}$ vi_j [$_{C'}$ skulle$_k$ [$_{IP}$ t'_j
which party said he that we should
[$_{I'}$ t'_k inte [$_{VP}$ t_k [$_{VP}$ t_j [$_{V'}$ köpa roliga hattar]] [$_{PP}$ till t_i]]]]]]]]]]?
not buy funny hats for
(Swedish, Holmberg 1986)

(b) * [$_{CP}$ [Vilken fest]$_i$ [$_{C'}$ sa hon [$_{CP}$ t'_i [$_{C'}$ att [$_{CP}$ [roliga hattar]$_l$
which party said he that funny hats
[$_{C'}$ skulle$_k$ [$_{IP}$ vj [$_{I'}$ t_k inte [$_{VP}$ t_k [$_{VP}$ t_j [$_{V'}$ köpa t_l]]
should we not buy
[$_{PP}$ till t_i]]]]]]]]]]?
for (Swedish, Holmberg 1986)
'Which party did he say that we shouldn't buy funny hats for?'

In Mainland Scandinavian, extraction is possible from embedded non-V2 clauses such as (3), but it is impossible from both subject-initial and topic-initial embedded V2 clauses such as (4). This contrast is explained if we assume that whereas Mainland Scandinavian embedded non-V2 clauses are simple CPs, Mainland Scandinavian embedded V2 clauses involve CP-recursion. Under this analysis, embedded non-V2 clauses contain one empty CPSpec, therefore allowing extraction (see (3)). Embedded V2 clauses on the other hand contain an additional CPSpec filled by either the subject or a topic, therefore blocking extraction (see (4)). Thus in Mainland Scandinavian, CP-recursion blocks extraction from both subject-initial and topic-initial embedded V2 clauses.

Consider now Icelandic. In this language, extraction from topic-initial embedded V2 clauses is ungrammatical, as in Mainland Scandinavian (compare (5b) with 4b)), but extraction from subject-initial embedded V2 clauses is grammatical, in contrast with Mainland Scandinavian (compare (5a) with 4a)).

(5) (a) [$_{CP}$ [Hvadha bók]$_i$ [$_{C'}$ sagdhir thu [$_{CP}$ t_i [$_{C'}$ adh [$_{IP}$ Jón$_j$
which book said you that J.-NOM
[$_{I'}$ vildi$_k$ ekki [$_{VP}$ t_k [$_{VP}$ t_j [$_{V'}$ gefa t_i Haraldi]]]]]]]]?
wanted not give H.-DAT
(Icelandic, Holmberg 1986)

(b) *[$_{CP}$ [Hvadha bók]$_i$ [$_{C'}$ sagdhir thu [$_{CP}$ t_i [$_{C'}$ adh [$_{CP}$ Haraldi$_i$
which book said you that H.-DAT
[$_{C'}$ vildi$_k$ [$_{IP}$ Jón$_j$ [$_{I'}$ t'_k ekki [$_{VP}$ t_k [$_{VP}$ t_j [$_{V'}$ gefa t_i t_l]]]]]]]]]]?
wanted J.-NOM not give
'Which book did you say that John didn't want to give to Harald?'
(Icelandic, Holmberg 1986)

The same reasoning which led to the conclusion that the Mainland Scandinavian examples in (4) involve CP-recursion now forces us to conclude that Icelandic subject-initial embedded V2 clauses such as (5a) do not involve

CP-recursion, but contain one empty CPSpec, therefore allowing extraction. Icelandic topic-initial embedded V2 clauses such as (5b) on the other hand contain an additional CPSpec which is filled by a topic and which therefore blocks extraction. In subject-initial embedded V2 clauses, V2 is due to V-to-I movement (see (2)), whereas in topic-initial embedded V2 clauses, V2 is due to V-to-C movement (see (1b)). This leaves topic-initial embedded V2 clauses as cases of CP-recursion in Icelandic, but given that many speakers of Icelandic behave like speakers of Mainland Scandinavian and reject embedded topicalization in non-bridge verb complements, the existence of free CP-recursion remains in doubt.

In chapter 5, Vikner shows that verb movement to I^0 is attested in Icelandic and Yiddish, but not in English and the Mainland Scandinavian languages. He adopts the proposal developed in Rohrbacher (1994) according to which this movement depends on the distinctive marking of the person features in the verbal paradigm.[3] This chapter also contains some interesting speculations on the systematic absence of V2 languages with V-to-I^0 movement but without free CP-recursion (which according to Vikner would be unlearnable due to the absence of positive evidence for a root/non-root distinction) and on the loss of V-to-I^0 movement in the history of the Mainland Scandinavian languages.

In chapters 6 and 7, Vikner uses his findings regarding verb movement to develop a theory of expletive constructions with NP- and CP-associates. The first descriptive generalization he tries to account for is that whereas expletive constructions with the associate in the internal argument position of the verb (i.e. in V′) are possible in all languages, expletive constructions with the associate in the external argument position of the verb (i.e. in VPSpec) are possible only in languages with both V-to-C and V-to-I movement (e.g. Icelandic), but impossible in languages without V-to-C movement (e.g. French), V-to-I movement (e.g. Mainland Scandinavian), or both (e.g. English). This is schematically represented in (6).

(6) $[_{IP}$ expletive $[_{I'}$ I $[_{VP}$ associate $[_{V'}$ V associate]]]]

√ V-to-I & V-to-C	√ V-to-I & V-to-C
*V-to-I only	√ V-to-I only
*V-to-C only	√ V-to-C only
*no V-movement	√ no V-movement

Vikner's account for (6) involves replacing the Case Filter with the following Licensing Condition: 'All phonetically realised NPs [and argument CPs] must be assigned case or be "alternatively licensed" – i.e., be governed by their theta-assigner or by I^0 (provided this I^0 has content and is not involved in the assignment of a case)' (183), where x governs y if and only if x c-commands y and no (minimality) barrier intervenes between the two.

[3] Vikner has subsequently developed a different theory, see Vikner (1995).

Vikner assumes that Case is not transmitted from the expletive to its associate and, following Rohrbacher (1994), that I^0 has content in all and only languages with V-to-I movement. In V′, the associate is theta-governed (and hence licensed) by V, resulting in grammaticality in all languages. In VPSpec, the associate neither receives Case nor is it governed by its theta-assigner. It therefore must be licensed by I^0. In languages without V-to-I movement, I^0 has no content and therefore cannot license the associate in VPSpec. In languages without V-to-C movement, I^0 assigns nominative Case to IPSpec under spec-head agreement and therefore cannot license the associate in VPSpec. As a consequence, this construction is ungrammatical in both types of languages. Only in languages with V-to-I and V-to-C movement where I^0 has content and nominative Case is assigned to IPSpec under government from C^0 (see chapter 3) can I^0 license the associate in VPSpec. As a consequence, this construction is grammatical only in the last type of language.

Passive expletive constructions constitute a crucial case. Here the associate is licensed in its underlying position in V′ by virtue of being governed by its theta-assigner V. As a result, passive expletive constructions with the associate in situ are grammatical in both Mainland Scandinavian (see (7a)) and Icelandic (8a)). But whereas the associate cannot be moved to VPSpec in Mainland Scandinavian (7b)) where this position is not licensed because I^0 does not have content, the associate can be moved to VPSpec in Icelandic where this position is licensed because I^0 has content (8b)).

(7) (a) at $[_{IP}$ der $[_{I}$ ∅$]$ $[_{VP}$ blev $[_{VP}$ $[_{V'}$ spist $[_{NP}$et æble$]]]]$
that there was eaten an apple
(Danish, ex. (80e), p. 202)

(b) *at $[_{IP}$ der $[_{I}$ ∅$]$ $[_{VP}$ $[_{NP}$ et æble$]_i$ blev $[_{VP}$ t'_i $[_{V'}$spist $t_i]]]]$
that there an apple was eaten
(Danish, ex. (58e), p. 192)

(8) (a) adh $[_{IP}$ thadh $[_{I}$ var$_j]$ $[_{VP}$ t_j $[_{VP}$ $[_{V'}$ bordhadh $[_{NP}$ eitthverteppli$]]]]]$
that there was eaten an apple
(Icelandic, ex. (80c), p. 202)

(b) adh $[_{IP}$ thadh $[_{I}$ var$_j]$ $[_{VP}$ $[_{NP}$ eitthvert eppli $]_i$ t_j $[_{VP}$ t'_i
that there was an apple
$[_{V'}$ bordhadh $t_i]]]]$
eaten
(Icelandic, ex. (58c), p. 192)

In order to account for this contrast, Vikner has to allow A-chains that meet the Licensing Condition twice. Thus in (8b), the associate is licensed via government by I^0 and its trace is licensed via government by V^0. This is essentially equivalent to double Case-marking, in pre-Minimalist terms a violation of the Chain Condition and in Minimalist terms a violation of Last Resort. The theory developed in Bobaljik (1995) accounts for the contrast in (7) and (8) in a more elegant way. In Bobaljik's theory, subjects (including

associates) never occupy VPSpec. In V-to-I movement languages like Icelandic AgrPSpec hosts the expletive, leaving TPSpec available as a second surface position for the associate passive subject in addition to its underlying position inside V′ (see (9a)). Languages without V-to-I movement like Mainland Scandinavian lack AgrP. Since TPSpec is occupied by the expletive, the associate passive subject can only surface in its underlying position inside V′ (see (9b)). Bobaljik thus replaces Vikner's complicated licensing mechanism with the availability of an additional functional specifier as the key for the distribution of expletives.

(9) (a) $[_{AgrP}$ expletive $[_{Agr'}$ Agr + V_i $[_{TP}$ associate $[_{T'}$ T $[_{VP}$ t_i associate]]]]]
(b) $[_{TP}$ expletive $[_{T'}$ T $[_{VP}$ V associate]]]

The second descriptive generalization Vikner tries to account for is that unergative expletive constructions are possible in languages with V-to-C movement (see the Danish example in (10a)), but impossible in languages without V-to-C movement (the English example in (10b) and its French counterpart).

(10) (a) at $[_{IP}$ der $[_{I}\emptyset]$ $[_{VP}$ har $[_{VP}$ t_i $[_{V'}$ danset nogen$_i$]
that there has danced someone
i haven]]]
in the garden (Danish, ex. (82c), p. 203)

(b) *that $[_{IP}$ there $[_{I}$ has$_j$] $[_{VP}$ t_j $[_{VP}$ t_i $[_{V'}$ danced someone$_i$] in the garden]]] (English, ex. (821), p. 203)

Vikner accounts for this contrast using a Government Requirement on the chain between an expletive subject and its associate according to which 'assignment of the thematic role (and/) or case assignment [to the chain] must take place under government' (183). As indicated in (10), Vikner assumes that the unergative associate lowers into the complement position of the verb. In its underlying position in VPSpec, the unergative associate does not meet the Licensing Condition in either language, since it does not receive Case, I^0 does not have content and the subject is not governed by its theta-assigner (i.e. V). After lowering to the complement position of the verb, the unergative associate meets the Licensing Condition in both languages since it is now governed by its theta-assigner. The Government Requirement is met in the V-to-C languages where Case is assigned to the chain via government of the expletive by C^0, but not in non-V-to-C languages where Case is assigned to the expletive by spec-head agreement with I^0. (Note that in both language types, theta role is assigned to the associate in VPSpec by spec-head agreement with V.) Hence (10a) is grammatical but (10b) is ungrammatical. Note however, that the Government Requirement can account for the contrast in (10) only if it is assumed that the post-verbal unergative associate

lowers into the complement position of the verb. Lowering of any kind is independently problematic, and movement into a complement position is specifically excluded both by the pre-Minimalist Projection Principle and by the Minimalist principle Extend Target. A less problematic account for (10) would involve extraposition. Since extraposed elements are not governed by the verb, Vikner's explanation would not carry over to these structures. But in the absence of any independent motivation for the Government Requirement (which only accounts for the cases in (10) and their passive counterparts), this account does not have much more explanatory content than the descriptive generalization it seeks to explain, and very little is lost if it is given up.

Although I have focused here on some problems for Vikner's analyses, it is important to keep in mind that these problems could not have been identified were it not for the care which Vikner takes in presenting his data and the rigor with which he pursues his predictions. This book will undoubtedly spawn much fruitful research, and I can only hope that it finds the widest possible distribution.

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