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REVIEWS

Cynthia L. Allen, *Case marking and reanalysis: grammatical relations from Old to early Modern English*. Oxford: Clarendon Press, 1995. Pp. xviii + 509.

Reviewed by ANS VAN KEMENADE, HIL/Vrije Universiteit Amsterdam

This book presents extensive and careful documentation of the history of those English constructions featuring the notorious impersonal verbs and passives.[1] The account is embedded in a discussion of the Old English (OE) system of case and its subsequent breakdown; the analysis relies on Lexical Mapping theory, in which the mapping between thematic role, grammatical function and case is central.

In the introduction, the aims of the book are laid out: a reexamination, based on newly collected facts, of the evidence for the hypothesis that the changes in constructions featuring impersonals and passives are due to the loss of the morphological case system.

Chapter 2 contains an overview of those parts of OE syntax most relevant to the book: case marking with particular verb classes, agreement, constituent order, formal subjects, and coordinate subject deletion. The latter may not immediately seem central, but since it is the only workable test for establishing the grammatical function SUBJECT relevant later in the book, it is of crucial importance to the author.

Chapter 3 is about case marking and the experiencer (EXP) verbs in OE. EXP verbs usually have two arguments, with the roles of EXP and Theme. These can be realized as two NPs, and here again there are three subtypes: both NPs can appear with a nonnominative case (mainly DAT for the EXP, GEN for the Theme, and the finite verb in default 3sg). In the two alternative types, either the EXP or the Theme can be nominative, and if so it agrees with the finite verb: Allen's evidence for assuming otherwise when the nominative is postverbal is very slender. A different way of realizing the two arguments is as NP and clause, where the EXP is sometimes nominative, but mostly dative. The clause sometimes appears in construction with *hit* 'it' or the demonstrative pronoun *þæt* 'that'.

In chapter 4 on the syntax of the EXP verbs in OE, an analysis is presented in terms of Lexical Mapping Theory (Bresnan & Kanerva 1989). The primary aim is to establish that EXPs, when preverbal, have the grammatical

[1] Thanks to Nigel Vincent for discussion of LFG and related matters. The usual disclaimer applies.

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role SUBJECT, though their case is overwhelmingly dative. Assuming without discussion that SVO is the common word order, Allen argues that dative EXPs occur preverbally considerably more often than unequivocal objects, which putatively shows that they are subjects. Real evidence for the SUBJECT status of the preposed dative EXP, notwithstanding the rarity of the examples, comes from coordinate subject deletion. Having established in chapter 2 that coordinate subject deletion in the vast majority of cases takes place under coreference with a nominative subject, and rarely with an object, Allen shows that EXPs in the various impersonal constructions, when 'preverbal', control coordinate subject deletion in about 50% of the cases. In view of the fact that objects can be 'preverbal', I would, however, prefer to restrict the conclusion to observing that this shows that the EXP is the subject in 50% of the cases. Rejecting, convincingly, the hypothesis of Elmer (1981) that *hit* subjects are inserted to satisfy the verb second target[2], Allen further argues that the systematic absence of the formal subject *hit* in constructions with preposed EXPs is accounted for by the assumption that the EXP is the subject. This latter claim remains to be supported: the absence of *hit* or *þær* is not unusual in all unaccusative constructions, with or without EXP, except those with weather verbs, contra Allen's implication (118) that empty subjects are virtually restricted to coordinate subject deletion contexts. Chapter 4 further gives an account of the mapping between semantic role, grammatical function, and case, accommodating in detail a variety of idiosyncrasies. The preposed dative EXP has the grammatical role SUBJECT. One phenomenon stands out in this section: while in double-NP constructions with a nominative Theme, the EXP is in Allen's terms often mapped as the SUBJECT, this is never true when both arguments are pronouns (143). An account for this is suggested in terms of the 'importance' of the Theme in the discourse, appealing to the fact that the pronominal Theme can usually precede a nominal EXP as well. This account is rather vague and implies that discourse considerations take precedence over grammatical function assignment. The constraint that an object pronoun can precede a nominal but not a pronominal nominative subject is a pretty strong one in OE, and it seems to me that one should surely want to try and account for it as a feature of grammar rather than discourse.

In chapter 5, on the loss of case marking, Allen documents in detail the loss of morphological case distinctions in the various dialects of Middle English. In regular transitive contexts, the distinction between various kinds of object case ceased to be signalled by morphological means by the middle of the thirteenth century, resulting in the loss of lexically determined case. The remaining domain for lexical case was impersonal contexts with a preposed dative EXP when the EXP is a pronoun with object case. This putatively

[2] It should be noted, however, that this is a separate matter from that of the relevance of V2, or the hypothesis that the first constituent meets an animacy target.

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argues against accounts which rely centrally on the loss of the case system to explain the demise of the impersonal constructions.

Chapter 6 is on the development of EXP verbs. Preposed dative EXPs continued to have the function SUBJECT until well after the demise of the morphological case system. Stressing the amount of lexical variation, Allen shows that the loss of the constructions was a gradual one, completed only by the end of the fifteenth century. Parameter resettings as proposed in the literature cannot, according to Allen, account for the loss.

Chapter 7 is devoted to explaining the loss of the preposed dative EXP. Following up on the arguments of the previous two chapters, Allen views the disappearance of the preposed dative EXP as the result of a gradual disfavouring of the option of case-marking subjects lexically. The pressure towards structural case-marking for subjects, compounded by the loss of case distinctions, gradually led to a situation where preposed dative EXPs were so infrequently used that there was insufficient evidence for the language learner to incorporate them in the grammar. The question of how such a general disfavouring is determined lexically, remains undiscussed.

Chapter 8 is on changes in passives of verbs which in OE had one dative object. Direct passivization of such verbs became the norm as early as the thirteenth century. Allen's account for this is that already in OE, such verbs had as object an NP with the thematic role Theme, which was assigned dative case. This is supported by the fact that the OE dative NP was replaced by a bare NP rather than a PP. Once the object case-marking distinctions were lost, this Theme was interpreted as a direct object with structural case, and was accordingly passivized.

In Chapter 9, on changes in passives of verbs with two object-like arguments, Allen attempts to establish that dative-fronted passives of ditransitives were lost by the middle of the fourteenth century, and argues that direct passivization of this object was not convincingly found before the last quarter of the fourteenth century. Allen concludes that dative-fronted passives were lost as a spin-off of a more general loss of fronted datives (there was never evidence that fronted recipients were SUBJECTs). Recipient passives did not develop out of dative-fronted passives, but resulted from the fixing of double object order, completed by the last quarter of the fourteenth century. The first NP following the verb (the earlier dative) was reinterpreted as the direct object.

Chapter 10 is the concluding chapter, giving a summary of the changes and discussing some of the implications for extant views of language change.

The book contains three appendices: the first contains a discussion of early examples of recipient passives (rejecting them as invalid); the second gives details of the data investigations; the third gives an overview of the texts cited.

It is impossible to review all aspects of the book. It is rich in historical detail, and there are inevitably many points to discuss and/or disagree with.

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This is, however, beyond the scope of this evaluation. I will therefore restrict myself to two interrelated points on which I would like to counterbalance Allen's position. This will touch on points of history, as well as theory and interpretation of data.

Allen's assumptions on constituent order, laid out in chapter 2, regrettably preclude an attractive integration of her results with the current debate on OE and ME word order. Allen rejects previous analyses of OE constituent order in terms of the verb second (V2) constraint, basically because they are not perfect, and goes on to ignore the positive insights emerging from them, including the well-established fact that there are rather systematic differences between the position of the finite and nonfinite verb. Her resulting positional distinction is between 'preverbal' and 'postverbal', while even casual inspection of the extensive literature should show that in a main clause with a lexical finite verb, the term 'preverbal' may refer to at least three distributionally distinguishable positions: the first position; the inverted position; a 'low' position, often sentence-final, in unaccusative constructions, including impersonals. Since a substantial part of the book is devoted to showing that dative EXPs, when 'preverbal', are in many cases subjects, it would seem more to the point to establish in detail what the possible positions for subjects with agent verbs generally are in OE. Similarly, in the discussion of object fronting, the relative position of subject and object is discussed without any reference to the verb. Since nominative subjects can be found sentence-finally in several kinds of contexts in OE, and objects are found easily in the first position of the main clause, and routinely in a position preceding the nonfinite verb, this simply does not tell us enough about the 'fronting' of objects.

The V2 issue reemerges in chapter 4: first rejecting the hypothesis presented in Elmer (1981) that preposed dative EXPs meet a V2 target, Allen states: 'All other things being equal, we would expect the nominal object of a main clause to be postverbal; preverbal nominal objects were possible in main clauses but normally unusual, and there is no reason why they should be so frequent in the type N construction [the construction with two nonnominative arguments–(AvK)]' (106). The relevance of this categorical statement is restricted to main clauses with one finite transitive lexical verb with a nominative subject. The object is probably postverbal in such cases because the finite verb satisfies a version of the V2 constraint, so that it precedes the object, and as a result of the relevance of V2, OVS orders are not unusual either. OE verb position can be properly recognized only when both the finite and nonfinite verbs are considered, and inspection of those patterns reveals that objects can occupy an array of positions, including a 'preverbal' one. Given the fact that nonnominative constituents can appear preverbally in general, it should come as no surprise that they do so more often in constructions without a nominative subject, and Elmer's (1981) animacy target, criticized somewhat ungenerously, does not seem to fare so

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badly here, especially in view of Allen's own facts about animateness. The same applies to the construction where the EXP is dative and the Theme nominative, or a clause: since the grammar allows the preposed dative EXP to appear clause-initially, it does so with considerable frequency as the only nonclausal argument in the sentence. It is probably correct to say that preposed dative EXP's did not meet a V2 *target*, pace Elmer (1981), but it does not follow that V2 is irrelevant. A V2 approach that explicitly accommodates unaccusative constructions can be found in van Kemenade (1992); Hulk & van Kemenade (1993). This also accounts for the fact that preposed dative EXPs became extinct, apart from some fixed expressions, by the end of the fifteenth century: the time when the loss of V2 with topics was completed. Allen's approach and one in terms of V2 would seem to complement each other very nicely: Allen makes a clear case that often, preposed dative EXPs are subjects and that a good deal of the synchronic and diachronic variation is lexical; on the other hand, a V2 account makes it clear why nonnominatives may appear initially at all, and meshes well with Allen's approach. I believe then, that Allen's account of the history of impersonals, while convincing for their lexical properties, errs on the nonstructural side. I will now turn to a point where she seems to me to err on the structural side.

In chapter 9, Allen is at pains to establish that there is only a tenuous historical connection between the loss of the dative-fronted passive and the rise of the recipient passive. Rather, recipient passives begin to be found when double object order was fixed. The resulting view of this instance is a radical reanalysis, one that Allen professes not to subscribe to in general, and which was made around 1375. This case is made acceptable, however, by the fact that the evidence for the old construction was completely gone before the reanalysis was made. The chapter is rather puzzling. Having been impressed throughout the book by the care with which Allen attempts to accommodate every last example, I was surprised that her date for the loss of the dative-fronted passive is about 1350, whereas she records poetic examples until a century later. And in the discussion of the rise of the recipient passive, early examples which are ambiguous between a dative-fronted passive and a recipient passive are interpreted as dative-fronted (390, ex. (249–250)), as if ambiguity by definition points to the older analysis. Similarly, Allen observes that there is some evidence from beyond the date of the putative reanalysis that double object order was not entirely fixed. It is quite unclear to me why the data would force us to interpret this reanalysis as being this abrupt, unless it is because Allen doesn't want the fronted datives to be reanalysed as nominative because there is no evidence that they had the SUBJECT grammatical role. On the basis of the facts presented, it would seem more plausible to say that the fronted dative at that point in history *could* be reinterpreted as nominative on the strength of its position, although it was not the SUBJECT; that this interpretation was reinforced by the fixing of

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double object order, as according to Allen; and that dative-fronted passives were lost in the second half of the fifteenth century, when the loss of V2 was coming to completion.

In conclusion, I recommend this book for its detailed, careful and explicit historical account, even though it follows from the above that I think Allen could have reached further by being less dismissive of current approaches to English historical syntax, in particular to clause structure.

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Denis Bouchard, *The semantics of syntax: a Minimalist approach to grammar*. Chicago: The University of Chicago Press, 1995. Pp. xiii + 525.

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The semantics of syntax purports to defend and illustrate a restricted view of semantics, both on theoretical and empirical grounds. It contains two chapters dealing with general matters, and three chapters applying the proposed approach to specific problems: Verbs of movement in French, Psych verbs, and Functional projections in S.

The author partitions semantics into three layers. Contrasting what he calls 'situational semantics', which deals with background knowledge and pertains to general cognitive capacities rather than linguistics, with 'linguistic semantics', solely concerned with linguistically relevant aspects of meaning, he further selects as his object that part of the latter which affects syntactic form, or 'G-semantics' (for Grammar semantics). From this perspective, existing semantic approaches (the mentalist conception of Jackendoff 1983, 1990, which the author seems to have particularly in mind, as well as truth-

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conditional and all denotational approaches) do not qualify as G-semantics or even linguistic semantics, because they all use extra-linguistic notions, such as theta-roles, which have to do with the way human experience is organized, and which, the author claims, have no effect on grammar. What makes this point of view interesting is the author's determination to ground it empirically. He concentrates on two problems, with detailed analyses of French data: the mapping between grammatical function and interpretation for the arguments of psychological verbs, and the polysemy of movement verbs.

As is well-known, the co-existence of the two classes of psych verbs (*fear* vs *frighten*), where experiencer and trigger seem to exchange their position (*Maria fears storms/ storms frighten Maria*), prevents a straightforward and systematic alignment of theta-roles and grammatical functions. Carefully examining the answers which have been given to the problem, the author points out that they all have to resort to linking between function and theta-role for each verb class, which, in his view, amounts to simply listing the possibilities. Moreover, he shows the arguments given in favor of the relevance of theta-roles in grammatical processes to be unconvincing. Thus, he concludes that classifications of verbs based on theta-roles are misconceived. Similarly, the polysemy of so-called movement verbs is problematic if the hypothesis that movement is the basic meaning is maintained, because it leads to an enumeration of the different uses, obscuring what they have in common. The difficulty disappears if the domain of space is not given priority: although such priority may be justified when one considers conceptualization, it is not so from a linguistic point of view. Abstracting away from extra-linguistic considerations, one has a better chance to get at the central meaning of polysemous entities.

The proposal can be summarized as follows. Predicates are associated with an abstract semantic representation, based on lexical decomposition and formalized as a tree structure. Syntactic structures are also represented as trees. Since one-to-one correspondence is untenable (given lexical decomposition), the mapping is a homomorphism, preserving dominance relations. It is further constrained by the Full Identification Principle, which says that 'every syntactic formative must have a corresponding element in the semantic representation', and 'every formative of the semantic representation must be identified by a morphosyntactic element in the sentence with which that representation is associated' (93). Although Full Identification countenances a meaningful functional category such as Tense, which expresses the anchor on the moment of speech, it precludes the use of AGR, because this functional category fails to correspond to an element of the semantic representation. Accordingly, the author explains in the last chapter why there is in effect no advantage in positing AGR, discussing most notably French data on adverbs and more specifically negation, which motivated positing AGR in the first place.

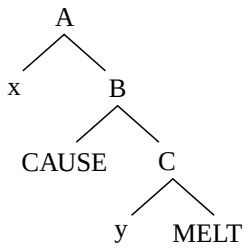
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The tree representation is at the center of the proposal. It must be emphasized that it is not here purely a formal tool, but is associated with a meaning: two nodes can be sisters or mother and daughter only if a certain relation holds between them; the semantic tree is seen as providing an 'orientation' relation between nodes (roughly, a higher node is 'oriented' towards a lower and c-commanded one, 64–66). A simple example of the semantic-syntactic interface is given by the analysis of verbs like *melt* which have a transitive (causative) and an intransitive (inchoative) use:

(1) (a) John melted the ice
(b) The ice melted

The semantic representation of *melt* is given in (2).

(2)



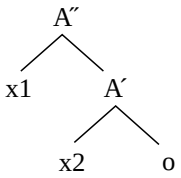
In (1a), the two variables are identified with the arguments *John* and *the ice*, and the nodes B and C of the semantic representation (which are left unspecified) are 'chunked' into one syntactic node, their respective heads being themselves chunked into the V *melt*. The same semantic tree is associated with (1b), with the higher variable identified with the NP *the ice* and the lower variable co-indexed with, and bound by, the higher one. The analysis is empirically supported by data showing that the subjects of inchoative verbs have properties of the entity that brings about the event as well as properties of the entity that undergoes the change.

A more complex illustration is offered by movement verbs. The author studies six French movement verbs in detail, in order to show that, in each case, it is possible to reduce their diverse uses to one common abstract core. For example, nine uses are associated with *venir* ('to come'): movement (*Max vient de Paris* 'Max is-coming from Paris'), progredience (*Max vient déjeuner* 'Max is-coming to-lunch'), origin (*Ce mot vient du latin* 'That word comes from Latin'), extension (*Cette route vient de Montréal* 'That road comes from Montreal'), time (*Max vient de partir* 'Max has just left'), end-reaching (*Si le directeur venait à mourir* 'If the director came to die'), involvement (*Ne venez pas me dire que Jean est malade*! 'Don't come and tell me that Jean is sick!'), availability (*Cette robe vient en trois tailles* 'That dress

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comes in three sizes'), and measure (*Marie lui vient à l'épaule* 'Marie comes to his shoulder'). The common core is given in (3) (121).

(3)



The element 'o' stands for the deictic center (ME-HERE-NOW); it follows from the properties of the tree, that x2 is in some relation with o, that A′ and A″ are projections of o, and that x1 is oriented towards x2 and towards o. In a sentence, x1 is identified with the subject, so that *venir* indicates that the subject is in relation with the deictic center o. The different uses are deduced from the properties of the argument or adjunct phrases, in conjunction with background knowledge. In the first use, the P *de* is associated with a source interpretation, and both the subject of the V and the complement of *de* denote spatial entities; typical properties attributed to actants as spatial entities will tell us that the orientation in this case means movement.

In spite of its many merits to which we return below, it is not likely that this book will convince semanticists in general that they should limit their work in the way proposed here, either in the definition of their task, or in choice of tree representation. Semantics is an attempt at modeling the way in which languages (or speakers) talk about the world. It is not clear what might be gained in defining a sub-part of the field (G-semantics) from which certain notions are expelled because they involve a representation of the world: such notions will come back in any explicit account of the interpretation, in accounting for the difference between acceptable and non-acceptable sentences and discourses, and in representing lexical meaning. The two case studies offered do not convincingly make the point. It is an important debate whether polysemous lexical items have a core meaning, but, even if a very abstract core like that proposed above for *venir* can be constructed, it is impossible to deduce the different 'uses' without appealing to 'real-world notions'. To get the second meaning of *venir*, for instance, the author appeals to animacy for the subject and the distinction between stage-level and individual-level predicates for the infinitival. Similarly, the discussion of psych verbs shows that the notions of experiencer and trigger are much too crude and probably misleading, and that distinctions based on the notion of point of view are relevant, not that any appeal to world-related notions should be banned.

As explained above, the semantic tree is said to incorporate an 'orientation'. Although the idea may seem strange at first, it is rightly emphasized (64 ff.) that the formalizations used by linguists are not devoid

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of meaning. In itself, the tree only represents groupings, and is associated with a different meaning in each field (genealogy, decision trees etc.). Linguists would agree that the syntactic tree is associated with constituency (Miller 1993). The author's idea, then, is that a different meaning ('orientation') should be associated with the lexical semantic tree. We are told that 'orientation' means the same thing as it does in real world situations, and that the determining factors differ from one conceptual domain to another. This is not as clear as one would like. In fact, the notion is not clearly distinguished from movement, but rather appears to be something like a disposition towards movement. If so, it is possible that we do not escape from a metaphorical vision of trees here, and it remains to be seen what it would do outside the domain of movement verbs (the notion is not an important ingredient in the analysis of psych verbs).

Nevertheless, this book has much to commend it. It is written in a purposeful way. The bibliographical coverage is impressive; in particular, it is not restricted to American publications, as is too often the case, but includes discussions of analyses or hypotheses proposed by French and Canadian linguists. While the aim of the book is very general, the properties of the constructions that are given as examples are taken very seriously, scrutinized and discussed in detail, because of the unfailing determination of the author to ground his position in a precise examination of the data.[1] Thus, the book is of interest to linguists of all persuasions. It is full of interesting observations and discussions. The chapter on psych constructions is particularly enjoyable, taking the reader through a critical assessment of all the properties that can be found in the literature, and enlarging the discussion from psych verbs to psych constructions, with non-psych verbs having a psych use. Thus, this chapter is particularly useful for anybody interested in a thorough description of a semantic class of verbs, and in the discussion of the use of theta-roles, and the relation between syntactic and (lexical) semantic properties in general.

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[1] The only place where the reviewer disagrees with the acceptability judgements concerns the distribution of French adverbs in chapter 5. It would be interesting to know whether the proposed judgements have to do with the bilingual situation in Quebec.

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Michael Brody, *Lexico-logical form: a radically Minimalist theory*. Cambridge, MA: MIT Press, 1995. Pp. vii + 155.

Reviewed by ANNA ROUSSOU, University of Wales, Bangor

In this monograph Brody argues for a non-derivational model of grammar which takes the relationship between sound and meaning to be direct, without any need for mediating levels of representation, such as S- and D-structure. LF then is the only syntactic level to which the lexicon has direct access; this is the lexico-logical form (LLF). Move α is captured under the notion of chains. In the absence of movement operations lexical items must occur in their PF position at LF. Thus LF-properties are recovered from PF, a conclusion which is most desirable from a learnability point of view as well. Contentive elements can be spelled out in different positions within a chain, depending on the morphological properties of a given language.

The overall goal of the monograph is definitely achieved: a number of redundancies characteristic of the GB framework as well as of Chomsky's (1993, 1995) Minimalism are eliminated, giving rise to a more elegant picture of grammar. The approach is original and the discussion thorough. The argumentation is clear and long-standing problems (e.g. Subjacency, parasitic gaps, etc.) are successfully analysed from a new perspective.

Brody's main argument is that a grammar that incorporates both chains and Move α leads to undesirable redundancy. Support for a chain-based approach comes from Full Interpretation (FI) and the distribution of thematic positions in chains (Chapter 1). Consider the following examples:

(1) There must have arrived many people.
(2) It seems that Mary is bright.

In (1) and (2) the expletives and their associates form a chain: [*there*, *many people*] in (1), and [*it*, CP] in (2). Given that expletives receive no interpretation at LF, the associate moves and replaces/adjoins to it, so that FI is satisfied (Chomsky 1986, 1991). However, if the associate *many people* moves in (1) then it should have scope over the modal *must*, contrary to fact. If, on the other hand, movement does not take place, the question is how FI is satisfied.

Brody argues that the same problem arises for other elements such as traces of head- and NP-movement which do not qualify as variables and therefore receive no interpretation at LF, but nevertheless must be present to link the moved category to its thematic position. He argues that this contradiction is solved once we take FI to apply not to single categories but to chains. Thus once the expletive and the associate form a chain, as in (1)–(2), FI is satisfied and there is no need for movement.

The distribution of thematic positions within a chain is captured under the MAIN THEMATIC CONDITION (MTC): only root positions can be thematic (the

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D-set). This implies that a chain can have more than one thematic position provided it is a root one, as shown in chapter 3 regarding parasitic gaps. The MTC itself follows from the Generalized Projection Principle in (3), which is illustrated by the French example in (4):

(3) Projectional requirements (i.e. categorial and selectional/thematic) can only involve positions that belong to the D-set.

(4) Mary_x embrasse_y e_x e_y Pierre
Mary kisses Pierre

According to (3) the categorial (project a VP) and selectional/thematic requirements of the verb *embrasse* are satisfied in the root e_y position of the [V, e_y] chain (given that in French V is in I). Similarly the subject *Mary* is selected in the root t_x position (the VP-internal subject hypothesis). Selectional and categorial features must identify all chain-positions. Identification is achieved under feature percolation; since percolation is always upwards, projectional requirements can only be satisfied at the root, hence the MTC.

Syntactic structures are assembled under the operations of Project (including chain formation) and Insert: once projection and chain formation has taken place, lexical insertion takes place directly at LF. This process is instantaneous and ensures that no intermediate structures are created, as opposed to Chomsky's (1993, 1995) cyclic derivations. In (4), then, each category projects in its root position; placeholders are used in VP for the subject and the object. The relevant chains are [*Mary*, e_x] and [V, e_y] The next step involves insertion of lexical items and the LLF representation is created. Lexical items are then already in their PF position at LF, so SPELL OUT takes place directly from LF.

The question that arises at this point with respect to Form Chain is what ensures that the right number of positions is present. Brody notes that this reduces to the question of the lexical input. One could argue though that this problem is overcome if Form Chain is somehow restricted by interpretation: for example interpretive properties of V seem to require that the number of empty positions be matched to the number of functional heads related to V, such as T, Agr, C (i.e. the extended projection of V).

Further evidence in favor of a chain-based approach comes from Subjacency effects (chapter 2). In standard terms overt movement of *what* in (5a) gives rise to a Subjacency violation, while LF-movement does not, as the indicated reading in (5b) shows:

(5) (a) ??What did John wonder who bought t?
(b) Who wondered who bought what?
(For which pair (x, y) x wondered who bought y.)

In the LLF framework the reading of (5b) is derived by the presence of a

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scope marker SM (an expletive) associated with the *wh*-in-situ (the contentive):

(6) SM_x Who wondered who bought $what_x$?

Brody offers an alternative generalization for (5); suppose that the relevant distinction is not between overt vs. covert movement, but between primary and secondary (wh-) chains: the former cannot cross islands, while the latter can. In (5a) then the primary *what*-chain (i.e. the one that satisfies the +wh C under spec-head) crosses an island and ungrammaticality is predicted. In (5b), on the other hand, the *what*-chain is secondary (the primary one is that formed by *who*) and therefore can occur within an island, hence the grammaticality. In other words secondary chains just, like parasitic gaps, circumvent islands. Moreover, just like parasitic gaps, secondary chains cannot be separated from the primary chain by more than one island:

(7) Who was against proposals to leave without waiting for whom?
(*Which pair (x, y) x was against proposals to leave without waiting for y)

Thus Subjacency constrains both secondary and primary chains.

Wh-in-situ in Japanese-type languages can occur within multiple islands. Following standard assumptions Brody suggests that Japanese exhibits large scale pied-piping, allowing for the wh-feature to percolate and turn the whole clause/island that contains the *wh*-element into a *wh*-phrase. The difference then between English and Japanese is that the former, but not the latter, does not allow for percolation of +wh across sentential boundaries.

Although this approach derives the right results, one might wonder why there should exist such a parameter that allows for feature percolation to be freer in languages like Japanese. Alternatively what allows for feature percolation, but not chain-formation, to cross islands in Japanese-type languages, especially if feature percolation is a basic requirement on chains, since it contributes to identification of all chain-members. In Brody's system feature percolation and chain-formation are connected. However, it seems that in the case under discussion these two 'mechanisms' must be dissociated in a rather ad hoc way. Of course such a solution seems to be necessary in the absence of any other more satisfactory analysis of the Japanese data.

The possibility of multiple thematic positions in a chain, *modulo* the MTC, is discussed in connection with parasitics gaps, as in (8) (Chapter 3):

(8) Which book did you criticize without reading?

Brody argues that 'although all positions must belong to some chain and chains must be maximal, a given position may belong to more than one chain' (86). Thus (8) essentially involves the following two chains:

(9) [which book, t]
[which book, e]

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In (9) the MTC is satisfied: the only thematic positions are the root ones.

The well-known contrast between (10) and (11) is now accounted for:

(10) ?Who did Bill believe [t to have visited you [without you having invited e]]?

(11) *Which girl did you expect t to meet everyone who liked t?

In both examples the primary gap occurs in subject position. In (10) the parasitic gap is within an adjunct clause (an island), while in (11) it is within a complement clause. (12) and (13) illustrate the relevant configurations and chains for (10) and (11) respectively:

(12) (a) wh_x tl_x $[_{VP}$ $t2_x]$ [pg]
(b) [wh tl t2]
[wh tl pg]

(13) (a) wh_x tl_x $[_{VP}$ $t2_x$ [pg]]
(b) [wh tl t2 pg]
(pg = parasitic gap)

t2 is the trace in spec, VP, the thematic position of the subject. t2 and pg cannot be in the same chain in (12), since the former does not c-command the latter and vice versa. Given that chains must be maximal, wh and tl (the surface position of the subject) are included in both chains. In (13) t2 c-commands the parasitic gap, which occurs within a complement clause, so they are both included in the same chain. The ungrammaticality of (11) now follows: the resulting chain contains two thematic positions, one of which (namely t2) is non-root. In (10) though only the surface position of the subject, tl, counts for the maximal chain, so there are two root thematic positions, t2 and the parasitic gap. Thus the ungrammatical (11) is explained as a violation of the MTC with no need for any extra machinery.

A clear advantage of this approach is that any c-command restrictions between primary and parasitic gaps do not need to be stated specifically for these constructions. Once chain-formation is taken to be the relevant notion, the ungrammaticality of (11) is directly accounted for in chain-theoretic terms. C-command only enters as a condition incorporated in chain-formation.

Finally, Brody argues that the connection between parasitic gap chains and wh-in-situ, in terms of circumventing islands, becomes more apparent if the parasitic gap is taken to be associated with a scope marker in Spec, CP. The *wh*-operator of the primary chain and the scope marker are united under absorption. Although this makes the two constructions look alike, it is not clear that this is necessary (at least for parasitic gaps), since both the parasitic gap chain and the primary one 'meet' to form a maximal chain under a c-command (essentially connectedness) configuration.

There is one more point that I would like to raise in connection with the MTC. Brody argues that the MTC accounts for the lack of movement from

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one thematic position to another: that would involve two thematic positions of which only one is root (cf. (11)). Thus a verb like HIT in (14a) which assigns a theta-role to its subject and a theta-role but no Case to its object cannot exist. Let us furthermore assume the VP-shell configuration in (14b) in which the light *v* has a different feature specification from V:

(14) (a) *John$_x$ HIT
(b) [John$_x$ I [$_{VP}$ t]$_x$ v [$_{VP}$ t2$_x$ HIT]]]

One could argue then that there are two different thematic root positions involved: t1$_x$ selected by *v*, and t2$_x$ selected by HIT. However, given that t1$_x$ c-commands t2$_x$ they should both form a single (maximal) chain, exactly as in the case of the ungrammatical (11). If so, the chain [John, t1, t2] includes two thematic positions, one of which is non-root, violating the MTC.

Positional parameters are discussed in chapter 4. First, Brody argues that the principle of Procrastinate (Chomsky 1993) which takes covert movement to be the default option is undesirable since it implies that grammars that show maximal differences between LF and PF are to be more highly valued. The principle of Earliness (Pesetsky 1989) has the opposite effect: it forces movement to take place as soon as possible, even if this operation may not be necessary in later stages of the derivation. This contradiction does not arise within the LLF model. Since lexical items are already in their PF position at LF, 'overt' movement from P1 to P2 means that the lexical item is spelled out on the P2 position of the chain; 'covert' movement from P1 to P2 accordingly means that the lexical item is spelled out on the P1 position of the chain; which position is spelled out is determined by the principle of Transparency: 'the contentive category in the chain must be in the highest position licensed by morphology' (104). Thus in the LLF framework 'strong' features are simply those satisfied by categories (the contentive is in P2), while 'weak' features are those satisfied by chains (the contentive is in P1).

Consider for example *wh*-questions in English.

(15) (a) Who did you see?
(b) *scope marker$_x$You saw who$_x$?

In (15a) *who* satisfies the wh-criterion (which is now stated in terms of chains). (15b), where a scope marker satisfies the wh-criterion, is ruled out as a violation of Transparency: the *wh*-phrase does not occur in the highest position licensed by morphology. In languages with partial *wh*-movement, Transparency forces the contentive to be realised in the intermediate Spec,CP (probably licensed as a focused phrase) while the matrix Spec,CP is occupied by a scope marker (overt or empty) that marks scope and satisfies the wh-criterion. The use of scope markers eliminates the need for LF movement from an A′-position (i.e. the intermediate Spec,CP).

Thus Transparency seems to account for parametric variation in a straightforward way. However, what needs to be discussed more is in what

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way the morphological properties of a given language determine the spell-out positions in a chain. For example what exactly is the difference between English and German *wh*-phrases so that the first allows for full extraction out of a *that*-clause, while the latter moves the *wh*-phrase to an intermediate position.

Quantifier Raising (QR) is discussed in connection with the absence of LF-movement (although the status of QR as movement is in general debatable). In the LLF framework one could assume that the scope position is marked by a scope marker which enters a c/m-command relation with the quantifier phrase (the scope relation does not need to be that of a chain). An issue relating to QR is that of Antecedent Contained Deletion, as in (16):

(16) (a) John {suspected [everyone that Mary did]}.
(b) [Everyone that Mary did] John {suspected t}.

(16b) is the QR-structure that Fiengo & May (1990) propose, so that infinite regression is avoided, since the anaphoric VP is contained within its antecedent (included in curly brackets in (16a)). Brody argues that QR in (16) is not necessary (in any case it is excluded in the LLF model) once we assume a modified version of the 'vehicle change' concept of Fiengo and May (1990). In particular, (16a) can have the following LF-structure:

(17) John {suspected [everyone that Mary did {suspected t}]}

t stands for the variable correlate of the NP headed by the quantifier *everyone*. (17) ensures that there is no complete identity between the elided VP and its antecedent and infinite regression does not arise. This analysis is in accordance with other recent proposals that take quantificational elements to receive their interpretation in their surface position (cf. Reinhart 1995).

Finally, Brody considers reconstruction phenomena and argues (contra Chomsky 1993) that at LF full copies are present in all chain positions. Deletion of copies is a matter of PF and not of syntax. Consider (18):

(18) **Mary** wondered [which claim that pictures of **herself** disturbed **Bill**] **he** made.

The presence of *herself* in the *wh*-associate forces the non-reconstructed version, while proper application of principle C (i.e. *he* and *Bill* cannot be coreferential) forces reconstruction. Brody argues that this contradiction is solved if a full copy of the *wh*-phrase is present in both positions at LF:

(19) Mary wondered [which claim that pictures of herself disturbed Bill] he made [which claim that pictures of herself disturbed Bill]

Principle A is satisfied since *herself* has a c-commanding antecedent in (at least) one of the positions of the chain. Similarly, principle C correctly excludes coreference between *Bill* and *he* in any of the positions. Thus the presence of both copies allows for interpretation to be determined chain-

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internally (i.e. in the relevant positions), while the only deletion rule that applies is the result of SPELL OUT. In this respect the LF-structure is indeterminate as desired (this is the principle of Partially Determined Full Interpretation). This approach eliminates the problems that arise under Chomsky's analysis of copies: reconstruction effects are now accounted for in a more straightforward way (although the discussion of A-chains and copies is less clear).

As the above discussion shows the (L)LF approach provides a more restrictive theory of grammar: the lexicon and PF have direct access to LF with no intermediate levels of representations and/or derivations. Although Chomksy's (1993, 1995) Minimalist framework also takes LF and PF to be the only levels of representation, it still allows for the same lexical item to occur in two different positions at LF and PF in cases of covert movement, maximizing the difference between PF and LF. The operation Move/Attract captured under the mechanism of feature-checking also leads to redundancy since it requires the same feature to occur twice, on the attractor and the attractee, with subsequent deletion of one of the two in the syntax. In the LLF model, on the other hand, the relevant feature appears only once and is shared by all members of the chain; thus there is no deletion of features/categories in syntax.

Before closing the discussion I would like to raise a final point regarding the use of empty positions in chains. Brody argues for a representational model of grammar, arguing that there is no derivational component involved, and therefore no cyclic operations (recall also that Project and Insert take place in one step). Note, however, that exactly as in the standard cases of movement (head-, A-, and A′- movement) copies are present; in chain-theoretic terms copies are also required to be present in all chain-positions. That is, Form Chain gives rise to a representation that involves a number of copies not just in the head and the foot of the chain but also in intermediate positions. Thus the representation assembled under non-cyclic Project (and Insert) is very similar to that derived by a model that allows for movement (cf. Manzini & Roussou (1997) for further discussion); thus intermediate structures are replaced by the necessary presence of intermediate copies. One would expect that in a radically-minimalist model there are no intermediate chain positions either: what is relevant is those positions that satisfy projectional requirements (LF) and Transparency (i.e. the spell-out).

Despite the above reservations, Brody's book is a major contribution to syntactic theory. It is very well written and the discussion is thorough and original in both conceptual and empirical matters, based on a set of minimalist assumptions. It is essential reading for anyone interested in linguistic theory.

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Reviewed by ANDREW CARNIE, Harvard University

When one works in a specialized field such as Celtic language syntax, it is a rare pleasure when a whole book devoted to the grammar of one's language specialty appears in print. Although in recent years numerous papers have addressed issues in the generative syntax of Irish, in the past 20 years only two other books have been devoted solely to the topic: McCloskey (1979) and Stenson (1981). Nigel Duffield's book *Particles and projections in Irish syntax* thus fills a very obvious void, being the first book in almost fifteen years to examine the syntax of Irish from a generative (or more precisely minimalist) perspective.

Duffield's goal is a unified characterization of functional projections, the particles that fill these projections, and word order alternations of both a clausal and NP-internal nature. The first chapter of the book sets out the scene in terms of the assumptions of minimalist syntax and some of the issues and concerns raised by the grammar of Irish. The second and third chapters deal with issues in the particle system of Irish, and in deriving VSO order in tensed clauses. In the fourth chapter, Duffield looks at word order phenomena in non-finite clauses (with a particular emphasis on Ulster Irish). Finally, the last chapter revolves around the parallelisms to clausal syntax found in the nominal system of Irish, comparing it in detail to similar systems found in Semitic languages.

Throughout the book, there is an underlying theme which relates syntactic structures to the well-known Initial Consonant Mutations (ICM) of Irish.

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From this perspective this book then is not only of interest to syntacticians, but also to phonologists and morphologists interested in ICM. Duffield departs from standard assumptions about ICMs (the idea that they are only lexical properties of specific words), and claims there are two types of ICM phenomena: Lexical and Functional. Lexical mutations are of the idiosyncratic type associated with specific lexical items. Functional mutation, on the other hand, is a syntactic phenomenon, occurring when certain functional projections are lexicalized (either by particles or by head-moved lexical items). (1) below is Duffield's basic claim:

(1) (a) Lexicalized T^0/D^0 triggers the Lenition F-mutation.
(b) Lexicalized C^0 triggers the Eclipsis (nasalization) F-mutation.

On the basis of this characterization, Duffield makes several very controversial claims which we will consider in detail below.

In the second chapter of the book, Duffield develops a remarkably elaborate system for deriving the basic VSO word order of tensed clauses. He claims that subjects in Irish are VP internal in VSO clauses. More controversially, he also claims that the verb in VSO structures is not at the left edge of the inflectional complex, but rather in some relatively low head $(Agr(O)^0)$:

(2) $[_{CP}\ C^0 \ldots [_{TP}\ T^0\ [_{NEGP}\ Neg^0\ [_{AGRP}\ Agr^0 + V_V\ [_{VP}\ Subject \ldots t_V]]]]]$

This contrasts heavily with most recent research in Irish syntax (see for example Bobaljik & Carnie 1996, among others), where the verb is taken to lie in the highest inflectional head below C^0. The evidence that Duffield presents for this approach comes from the particle system. Duffield observes that the three heads not occupied by V at spellout (C^0, Neg^0, T^0) form a unitary class, in that they contain the information found in the preverbal particles of the language:

(3) (a) **Ní** thuigim.
NEG understand
'I do not understand.'
(b) **Ar** thuig tú?
[+Q]-PAST understand you
'Do you understand'
(c) Dúirt sí [$_{CP}$ **nár** thuig tú].
said she that.NEG.PAST understand you
'She said that you don't understand'

With respect to ICM, Duffield claims that if the T^0 head is in some way lexicalized (either by negation or by the past tense morphemes *do* and *-r*), then the T^0 head triggers lenition on the first consonant of the following verb.

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If on the other hand C^0 is in some way lexicalized, eclipsis/nasalization occurs. Notice that there are two crucial parts to this analysis: first the verb must not have raised through T^0, since lexicalized T^0 is the trigger for the mutation. This in turn forces the second crucial assumption: if the verb is not in T^0, then it must be lower (in Agr^0), which means that post-verbal subjects must be VP internal. Let us consider these two claims. First, let us consider VP internal subjects. Bobaljik & Carnie (1996) claim on theoretical grounds that by minimalist assumptions the movement of arguments in finite clauses must be a superset of movement of arguments in non-finite contexts. Non-finite clauses in Irish show a derived SOV word order, where the object can be shown to have shifted around verb and subject to the specifier of some agreement projection. Since the subject precedes the object, it follows that the subject is also VP external. McCloskey (1996) presents evidence from VP-adjoined adverbs like *riamh* 'always/ever' that the subject must be external to the VP. These adverbs appear between the surface subject position and the surface object position in finite clauses, thus showing that the subject is VP external. In chapter four, however, Duffield easily overcomes these objections by splitting the verb phrase into two halves, with the upper VP containing the subject, an intervening inflectional head (Asp^0) whose specifier serves as the overt landing site of the object, and a lower VP containing the thematic position of the object:

(4) abc

(4) $[_{CP}\ C^0 \ldots [_{TP}\ T^0\ [_{NEGP}\ Neg^0\ [_{AGRP}\ Agr^0 + V_V\ [_{VP}$ Subj $t_V\ [_{ASPP}$ Obj $t_V\ [_{VP}\ t_V\ t_{obj}]]]]]]]$

The other claim – that the verb does not move into T^0 – is more controversial and difficult to confirm. As noted by Duffield himself, verbs in Irish show a full range of tense inflectional suffixes. This is problematic for the view that the verb is not in T^0. Duffield attempts to account for this fact by claiming that these markers do not show tense but rather mood and aspect. To this reviewer this claim seems very strange. These morphemes show a full range of tense forms ranging from past (*-amar*, ∅, etc.) to present (*-eann*, etc.) to future (*-faidh*, etc.). The preverbal particles only seem to vary on a past/non-past axis. It seems backwards to claim that the full range of tense inflection is not T^0, but the more limited set instantiates the node. It should be noted, however, that Duffield's basic claim can be maintained with only a minor modification. Ó Sé (1990) analyzes the temporal morphology in Irish preverbal particles, and the equivalent particles in copular clauses, as realis/irrealis mood. Duffield's system translates directly into this characterization. Mood is realized in the preverbal particle system and is lexicalized as a lenition trigger. On a related note, Duffield seems to assume that the only positioning of NegP is one below TP and above AgrP. Laka (1991) argues for an A-bar projection lower than CP (and perhaps MoodP) but higher than the highest A inflectional projection (TP or AgrP): ΣP. This

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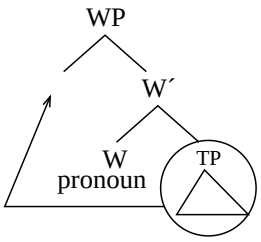
position is associated with negation in complementizer particles in Basque. The extension to Irish should be obvious: Negation might be this Σ^0, rather than the Neg^0 (lower than T^0) discussed by Duffield. What is particularly interesting, however, is that the important insights of Duffield's analysis can be maintained independently of what actual functional projections are involved.

In addition to this analysis of particles and VSO structure, the first chapter of Duffield's book contains an analysis of an otherwise puzzling phenomenon in modern Irish: pronoun postposing. While in finite clauses with full NP objects the word order is VSOX, the word order in clauses with weak pronominal objects is VSXO:

(5) Bhuail mé leis an ord é
hit I with the hammer it
'I hit it with the hammer.'

Duffield explains this effect in terms of Wackernagelian second position, where the pronoun occupies the head of WP (an A-bar projection higher than TP but lower than C^0), and the rest of the clause moves to the specifier of WP, as part of a generalized topicalization operation:

(6)



This account also attempts to explain the apparent 'reverse' order of clausal adverbials found in Irish. If this account is correct then there should be a direct correlation between reversed order adverbials and postposed pronouns crosslinguistically. Only further comparative work will confirm whether this prediction holds true or not. A more pressing problem has to do with the limitations of the pronoun post-posing phenomenon. The above account predicts that weak pronouns will follow any finite complements embedded under TP (which in turn is in the specifier of WP). This is false (Ó Siadhail 1989: 209):

(7) *Chuala mé raite [$_{CP}$go mbíodh sé ann] é
heard I said that be.HAB he there it
'I heard it said that he used to be there.'
cf. Chuala mé é raite go mbíodh sé ann.
Chuala mé raite é go mbíodh sé ann.

Pronoun-postposing is a strictly clause bound phenomenon.

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The strong claim of Duffield's work is that in Irish, complementizers always cause eclipsis/nasalization on the initial consonant of the following word. As he notes, however, there are two glaring exceptions to this claim: the negative complementizer found in non-finite clauses *gan*, which inflicts no mutation, and the so called direct relative ([+wh]) complementizer a^{L1}, which causes lenition. To explain these exceptions, Duffield claims neither of these elements is in reality a complementizer, outward appearances to the contrary. *Gan*, he claims, is a negative element that occupies the specifier of NegP. This analysis seems both sound and plausible. The claim that a^L is not a complementizer is more controversial however, as among other things it calls into question the well motivated analysis of Irish wh-complementizers advanced by McCloskey (1979, 1990). Duffield claims that a^L, unlike the indirect relative marker a^N (which is a complementizer in Duffield's system), is an element base adjoined to T^0, thus accounting for the fact that it is a leniter rather than an eclipsis/nasalization trigger. In order to account for the fact that this morpheme appears overtly with wh-words and in other wh-contexts such as relatives, Duffield claims that the specifier of TP may function when overtly marked (by a^L in Irish) as a Topic Phrase. As support for this claim he notes that 'when removed from context, many XP-a^L-V-YP strings are ambiguous between a relative-clause interpretation and a topic-structure interpretation' (196). I find this evidence confusing, since there are many cases where XP-a^N-V-VP strings with an unambiguous complementizer a^N also function like topicalization structures. This means that in Duffield's system there are at least three different locales for topicalization. First we have the specifier of CP, headed by a^N. Next there is the specifier of TP, headed by a^L. Finally, there is the specifier of WP, headed by weak pronominals. This seems overly complex for a relatively straightforward phenomenon, and only seems to be motivated[2] by the need to maintain the mutation hypothesis in (1). Further problems arise with this analysis, however. First, there is the problem of where the a^L morpheme comes from. Duffield does not want it to be generated under T^0, since it has a morphologically decomposable alternant that shows reflexes of the tense

[1] Orthographically this word is actually <a>, phonologically it is a /ə/. Duffield follows McCloskey in writing this particle as a^L (where ^{L} is a mnemonic for 'lenition') to distinguish it from the many other particles which are written *a* (such as the indirect relative a^N).

[2] Duffield has other arguments for the idea that a^L is not a complementizer. For example, he presents citations to the claim that a^L is historically derived from the same morpheme that shows up marking the past/non-past distinction in complementizer and negative heads. From the perspective of synchronic grammar this evidence is clearly weak. He also claims that, unlike complementizers, in the class of irregular verbs, the suppletive dependent form is never used with a^L. Since the standard analysis of dependent/independent verb form alternations is one of selection, this argument disappears, as one could simply claim that a^L selects independent forms, whereas other complementizers select dependent forms.

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morpheme, *a-r*^{L} 'a^{L} + past'; instead he base generates it as adjoined to T^{0}. It remains a mystery as to why or how this should occur. A further problem lies with the fact that the a^{L} morpheme is well known to mark cyclic *wh*-movement. In McCloskey's classic (1990) analysis of *wh*-movement, whenever a *wh*-word has moved through the specifier of a CP, the morpheme a^{L} appears as the head of that CP. Under Duffield's analysis, this cyclic behavior of a^{L} is mysterious: by stipulation each clause embedded under a *wh*-word must base generate a topic-identifying a^{L} morpheme adjoined to its T^{0} head. The link to cyclic *wh*-movement seems to have been abandoned.

Chapter 4 represents one of the most polished and complete sections of this book. It presents an analysis of SOV word order in Ulster infinitivals in terms of object raising to a functional projection. Duffield argues that object movement in Modern Irish is to some position internal to the VP; namely to the specifier of AspectP, which is headed by another particle a^{L} (not to be confused with the one found in direct relatives) (8):

(8) $\ldots[_{\text{VP}}$ Subj v $[_{\text{ASPP}}$ Obj $[_{\text{ASP}^0}$ $a^{L}]$ $[_{\text{VP}}$ V $t_{\text{obj}}]]]$

This derivation is highly convincing since, as noted above, it explains the problematic adverbial placement facts, as well as some theoretical concerns.[3] Duffield links this word order positioning to another phenomenon in the language, the $ag \Rightarrow a^{L}$ rule. In matrix clauses, when an object is extracted from a clause bearing perfective aspect marking (*ag*), then the perfective marker shifts form to a^{L}, homophonous with the morpheme in (8):

(9) [An t-airgead]$_{i}$ a bhí an bhean **a**$^{\mathbf{L}}$ thabhairt t_{i} don fhear
the money WH was the woman PROG give to.the man
'the money that the woman was giving to the man'
cf. Bhí an bhean **ag** tabhairt an airgid don fhear
'The woman was giving the money to the man'.

On the basis of this fact, Duffield associates the a^{L} morpheme in (8) to the Aspectual head (rather than, for example, $AgrO^{0}$). While Duffield's arguments about object movement to a position that is VP internal are entirely convincing, the identification of the head a^{L} as Asp^{0} seems highly suspect. First, there is the fact that this particle, like any other agreement morpheme in Irish, shows a complete range of person forms when the subject is null:

(10) Ba mhaith leis [*pro* mo^{L}/do^{L}/a^{L}/a/$ár^{N}$/$bhúr^{N}$/a^{N} (m)b(h)ualadh
would-like with.3S 1S/2S/3MS/3FS/1PL/2PL/3PL hit
'He would like to hit me/you/he/she/us/you(pl)/them.'

[3] In particular, Duffield solves the main problem with Bobaljik & Carnie's (1996) analysis, where the movement of a subject around the surface position of the object creates a violation of the minimality based economy condition of shortest move.

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Second, there is a strong inconsistency in Duffield's argumentation about this particle. He observes '…the fact that a^L directly substitutes for *ag* – as opposed to being projected in addition to the progressive morpheme – suggests that "Object Agreement" and "Aspect Phrase" are one and the same VP-internal functional projection' (246). Notice, however, that in chapter 2, Duffield argued for three distinct functional heads above AgrP: C^0, T^0, and Neg^0, despite the fact that all the information contained in these nodes is invariably contained in a single particle. Finally, Duffield seems to have missed the fact that overt Aspect heads, such as the recent perfective morpheme *tar eis*, can in fact appear overtly in conjunction with the a^L morpheme. Worse yet, these aspect morphemes are separated from a^L by an object:

(11) Tá Seán **tar eis** an teach **a**$^{\mathbf{L}}$ thógáil
be John PERF. the house a^{L} build
'John has just built the house.'

The fact that these aspectual elements are heads can be seen by the fact that they block head movement of the verb to initial position and force auxiliary insertion. These arguments all point to an analysis where the a^L morpheme in non-finite clauses and in sentences like (11) is identified with $AgrO^0$.

In the last chapter, Duffield presents very convincing arguments from adjective placement that the genitive constructions in Irish involve raising both the head noun through two functional projections (Num^0, Agr^0) to D^0 in a manner familiar from standard analyses of semitic Construct State Nominals. The argumentation in this section is very thorough and complete, so I will not discuss it further. However, I would like to take up the claim made by Duffield that D^0s, when lexicalized, are lenition triggers. As noted above, Duffield makes a critical distinction between F(unctional)-mutation and L(exical)-mutation. In particular, he claims that only functional mutation spreads through syntactic categories, whereas lexical mutation does not. Empirically this claim is simply false. Lexical mutation (such as that triggered by plural masculine nouns) can spread to two conjoined adjectives:[4]

(12) na diail^{L} **bh**eaga agus **mh**óra
the devils little and big
'the big and small devils'

The empirical coverage of Duffield's work is impressive. Based on a relatively simple assumption, he manages to develop a comprehensive (albeit complex) system accounting for a wide variety of phenomena.

[4] A more accurate description of the data seems to be that lenition may spread, but eclipsis is strictly local.

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This book is an outgrowth of Duffield's (1991) dissertation, based in part upon that work, in part on new research and new assumptions meeting more current minimalist assumptions. The fact that this book is a revised version of an older thesis, however, causes it to be a less useful tool than should be merited by the analysis it contains. The book, in particular the second chapter, moves back and forth from minimalist to preminimalist GB assumptions with almost dizzying frequency, obscuring an otherwise innovative and interesting analysis. The book is also painfully badly edited, with many errors in the data, some of which are crucial[5] to the analysis. For example, in chapter 5, example (16c) (279) shows that lexical eclipsis cannot spread across a syntactic domain. The sentence is marked as grammatical, but is crucially UNGRAMMATICAL. A similar problem is seen in chapter 5, example (14a) (277) (the asterisks here are mine, not Duffield's):

(13) (a) *a aon **th**each (b) *a h-aon **th**each (c) *a n-aon **th**each
his one house her one house their one house

These examples were meant to show that in most cases, lenition is a local phenomenon, where the lenition trigger (*aon*) is string adjacent to the target word (*teach*), crucially blocking other mutations (such as anti-lenition (13b) and eclipsis (13c)) triggered by less adjacent particles. Although examples exist which show this very phenomenon, the examples given above in (13) do not as they are all ungrammatical – simply because /t/ never lenites after a homorganic nasal like the /n/ at the end of *aon*. Duffield marks these incorrectly as grammatical.

There are also surprising gaps and misattributions in the references of this work. For example, there is no reference to Fassi Fehri's (1993) work on construct state nominals, which proposes a nearly identical DP internal structure and movements to the one proposed here. The idea that Irish object shift involves movement to the specifier of AspP is misattributed to Noonan (1994), where in actuality Guilfoyle (1993) was the first to apply the analysis to Irish. On page 259, the claim that the $ag \Rightarrow a^{L}$ rule applies only in northern dialects of Irish is attributed to McCloskey (1983). This is incorrect, McCloskey never makes this claim, nor for that matter is the claim correct.

[5] There are some other less crucial but nonetheless unfortunate errors in the data as well: (i) p. 19, example 20a the word by word gloss should read 'the priest parish'; (ii) p. 148, example 14a should read '…an t-amhrán a rá arís'; (iii) p. 182, example 83b and 84b should be 'Na rudaí' not 'An rudaí'; (iv) p. 182, example 84b 'Na rudaí' is misglossed as 'what', but should be 'the things'; (v) p. 191, example 101b 'a dheir' should be 'a deir'; (vi) p. 241, example 63f 'cá hair' should be 'cá air'; (vii) p. 241, example 63f is incorrectly translated. It actually means 'We didn't know what he was looking at'; (viii) p. 244, example 67b 'Na cheisteanna' should be 'Na ceisteanna'; (ix) p. 277, example 13b 'ar h-ocht gcapall' should be 'ar ocht gcapall'; (x) p. 320, example 80b 'dheathár' should be 'deathár' (see discussion above in the main text about lenition of dentals). Many of these errors involve mistakes in the initial consonant mutations, which is particularly unfortunate in a work with a special focus on initial consonant mutations.

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This statement is important to Duffield's analysis since it is meant to serve as evidence for the functional identity of the a^L and *ag* particles. What makes the statement so particularly strange, is that in footnote 21, Duffield refers to the application of this process in the southern dialect spoken in Dún Chaoin, in West Kerry – thus providing a clear counterexample to his own prediction. One factor likely to frustrate the non-Celticist is in the inconsistency in marking the ICMs: sometimes they are marked with underlining, other times they are ignored.

It is thus a truly unfortunate fact that the poor editing and exposition in this work obscures an otherwise well worked out and articulated theory, and thus limits the usefulness of the book to people who are not experts in Celtic syntax. If the reader can put these minutiae aside, however, they will undoubtedly find insights in this work that will challenge their views on both the syntax of Irish and, more generally, the minimalist approach to grammar.

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Paul Fletcher & Brian MacWhinney (eds.), *The handbook of child language*. Oxford: Blackwell, 1995. Pp. x + 786.

Reviewed by MARGARET DEUCHAR, University of Wales, Bangor

This weighty volume brings together the contributions of thirty-six scholars in the field of child language, writing between them a total of twenty-five chapters. Coverage is broad, with nonnormal language development included. The volume is attractively presented, and impressively free of typographical errors.

There are three main parts to the volume: 'Theory, method, and context', 'The emergence and consolidation of linguistic abilities', and 'Nonnormal language development'. Meisel begins a section on 'theoretical approaches' in the first part by presenting a useful, critical account of parameter setting as an important current approach to language acquisition. He discusses the controversial questions of what exactly should be parameterized and whether or not parameters can be reset. He concludes that the popularity of the parameter theory may be due to its attractiveness as a metaphor for the human language capacity, although this is still poorly understood. In the next chapter, Plunkett presents connectionism as an empiricist approach in contrast to nativism. He argues that connectionist networks represent the input as rich rather than impoverished, the latter view being characteristic of the 'symbolic' (linguistic) approach. Ochs and Schieffelin then argue for greater consideration of the role of language socialization in grammatical development. This involves the adoption of a more ethnographic approach of the kind familiar to anthropologists and sociolinguists, but which is not yet common among acquisition theorists. They argue that the absence of such an approach in an area like bilingual acquisition explains a dearth of studies on the acquisition of code-switching by young children. (This lack, is however, beginning to be remedied in studies such as Petersen 1988, Lanza 1992, Deuchar 1995, Deuchar & Quay 1995, Vihman 1998.)

A subsection on 'methods' contains two chapters, one by Bates, Dale & Thal, and the other by MacWhinney. In a study demonstrating the range of individual differences in language acquisition, Bates et al. exemplify the usefulness of the MacArthur Communicative Developmental Inventories (CDIs), which involve collecting data based on parental report rather than on recorded interactions. The use of CDIs in amassing large datasets and focusing on representative behaviour is beginning to be more widely accepted, and the value of the method is reflected in the chapter in various new findings, such as dissociation between comprehension and production, which have often previously been assumed to be associated in development. MacWhinney then introduces and outlines the Child Language Data Exchange System (CHILDES), a by now well established computerized database of child language, together with systems of transcription and

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analysis. (More details of this can be found in MacWhinney 1995, the revised version of the manual.) In his chapter MacWhinney reports on an exciting new development whereby it will be possible to link digitized speech to transcription. Some automatic phonetic transcription of high-frequency words (in English, initially) will be available in addition to the currently existing codes for manual phonetic transcription. However, I suggest there may be a danger in providing too easily accessible transcriptions of adult pronunciations to researchers who are possibly all too readily influenced by adult models in transcribing child speech.

In a third subsection of Part I entitled 'Social and contextual influences', Snow reviews the now enormous literature on child directed speech (CDS) and the role it plays in language acquisition. New points to be noted in this area include a reconsideration of the role of negative evidence, exceptions to 'universal' tendencies, the usefulness of the CHILDES database, and the increase in crosslinguistic studies which allow the specific effects of CDS on language structure to be more closely examined. Overall, in the empirical studies reported, I note there is still a strong general emphasis on maternal as opposed to other kinds of adult input, and I think it may now be time for the balance to be redressed. Hickman's chapter in the same subsection focuses on how children learn to organise discourse, especially on how their developing language represents the domains of person, space and time. In general, she finds that discourse organisation is a late development in all languages considered, but that the particular course of development is affected by the language being acquired. In the next chapter on bilingual acquisition, De Houwer reviews an impressively wide range of literature, including relatively inaccessible studies. She also deals with many issues, reporting a wide range of views, some of which have achieved more consensus than others. For example, many would agree with her that it can no longer be claimed that very young bilingual children use no translation equivalents. On the other hand, a less common view is that one should limit oneself to utterances consisting of elements from one language only in studying the relationship between a young bilingual's two languages (236). One issue raised which has been widely discussed is that of whether there are two grammatical systems from the start in a developing bilingual. My own case study is cited in support of what De Houwer calls the 'Separate Development Hypothesis', but I would argue that separate morphosyntactic systems can only be established from the point at which it is clear that the child is speaking one language rather than another (Deuchar & Quay 1997). I should also like to point out that the language environment of my case study is inaccurately described. The correct details can be found in Deuchar & Clark (1996).

In the final chapter of Part I, Ely & Berko Gleason's chapter on socialization across contexts clearly follows Ochs & Schieffelin's advice about the importance of studying language socialization, though they limit

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themselves to 'Western', English-speaking children who are also mostly middle class. Negative evidence is particularly clear in language socialization since, as they say, parents teach their children what not to say as well as what to say.

A chapter by Locke begins the subsection of Part II which deals with early speech development. He describes infants as travelling 'along a developmental growth path that leads to linguistic capacity' (302) rather than acquiring language as such. He emphasizes the importance of interactive and attentional mechanisms. His approach would seem to apply particularly to Western societies where children interact mainly with their mothers (as in the chapter by Snow) and little is said about the role of other caregivers. One strength of this chapter is that it mentions sign language acquisition, an area sadly neglected in this otherwise comprehensive volume. In the next chapter, Kent & Miolo focus specifically on the phonetic abilities of children in their first year of life. One theme of their chapter is the issue of continuity versus discontinuity between babbling and later speech development, on which they find in favour of continuity in general, while acknowledging that there is some counter-evidence. On the question of infant phonetic systems, they make the important point that the possibility of making segmental transcriptions of infants' speech does not necessarily indicate that the infants have segmental phonetic systems. Developing the same theme in the next chapter, Menn & Stoel-Gammon suggest that while discontinuity with speech applies to early babble, continuity is more characteristic of later babble. They also argue that children's earliest phonological units appear to be whole words.

In a new subsection on 'learning words', Barrett compares various approaches and presents his own multiroute model of early lexical development. Clark then reviews what is known about how children start acquiring a lexicon, and also points out how little is known about the later development of the lexicon. This is followed by an account of the role of verb syntax in verb learning, which also helps to explain why verbs are acquired relatively late.

In the next subsection on 'learning grammar', Golinkoff & Hirsh-Pasek show how a focus on language comprehension can reveal that the child has capabilities which are not evident in language production. Following this, Peters considers strategies in the acquisition of syntax, pointing to the particular importance of prosody. Radford then sets out to demonstrate how early sentence development can be explained within the framework of government and binding theory. The same framework is used by de Villiers to discuss the extent to which the empty category principle operates in the same way for children as for adults.

The existence of Part III in this book, on nonnormal language development, reflects the fact that this area has recently attracted more interest as an important area of language acquisition. The first chapter in this

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section, on computational approaches to the analysis of language impairment by Miller & Klee, could in fact have been placed next to that by MacWhinney, mentioned earlier, since both deal with the computer analysis of language, and since Miller & Klee are concerned with the analysis of normal and impaired child language. The SALT Computer Program, described in some detail by Miller & Klee, could be compared to the CHILDES transcription system described by Macwhinney, which also deals with both normal and impaired language. Direct comparison of the two systems is unfortunately not found in the Handbook, however. The next chapter, by Leonard, is the first one specifically on language impairment, and argues that studies of phonological impairment can be useful in evaluating models of phonological development in general. Grammatical impairment is dealt with by Fletcher & Ingham in the next chapter, which is a selective review showing the difficulty of distinguishing linguistic from cognitive impairment. It includes some interesting crosslinguistic data. However, in relation to German it seems somewhat misleading to represent the verb-final patterns in SLI data from Clahsen (1991) as evidence of a 'grammatical deficit', since as Clahsen himself points out in his book, verb-final patterns are very common in normal German-speaking children also (see Clahsen 1991: 194).

Craig, in her chapter on pragmatic impairments, argues for a functionalist approach which focuses on the relation between form and function. This perspective, she argues, is more revealing of pragmatic impairments than a modular approach. The question of language development in Down Syndrome children is first raised in chapter 4 by Bates et al., but then taken up again in Chapman's chapter. A wide range of literature is reviewed on both production and comprehension. Chapman suggests that more research should be conducted taking into account variation in auditory short-term memory performance. Such research might help account for the success of interventions based on visual representation such as signing, reading and writing. Eisele and Aram's chapter on lexical and grammatical development in children with early hemisphere damage addresses fundamental questions such as the extent to which functional units of language have neurological correlates, and the extent to which these units can be localized in the brain. They argue that, although there is evidence for a functionally modular organization of language, the localization of various aspects of linguistic behaviour in specific regions in the brain is still elusive. They also argue against a simplistic view of the language lateralization hypothesis and conclude that 'a complete acquisition of language requires the normal functioning of both hemispheres from the earliest point in development' (688).

In all, I consider this to be a useful book, despite its weight and price. It has something to offer both the child language specialist, who can follow up specific themes through the extensive bibliography, and the general linguist, to whom it provides a state of the art survey.

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Katalin É. Kiss (ed.), *Discourse configurational languages* (Oxford Studies in Comparative Syntax). New York & Oxford: Oxford University Press, 1995. Pp. i + 393.

Reviewed by Anna Siewierska, Lancaster University

This volume is a collection of studies on how to analyse, within the generative framework, clause structure in languages in which the discourse-semantic functions of topic and/or focus are expressed via particular structural relations. É. Kiss terms such languages discourse configurational, hence the title of the volume. Though the term discourse configurational language suggests that the volume deals with a hitherto unrecognized language type, this is not actually the case. The languages which are discourse configurational in É. Kiss's sense, i.e. have a structural topic or (identificational) focus position or both, include most of the major European languages, with the notable exception of English, Danish, Swedish and Norwegian (É. Kiss to appear). Some other discourse configurational languages listed by É. Kiss in the introduction to the volume are: Korean, Chinese, Nepali, Japanese, Hindi (Asia), Somali, Aghem, Kikuyu, Yoruba, Berber, some Chadic languages (Africa), Haida, Omaha, Quechua, the Mayan languages (the Americas) and Ilonggo (Austronesia). While discourse configurationality is

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evidently a widespread phenomenon, it has not until recently been considered as requiring special treatment within generative syntax. This volume seeks to rectify this situation by showing the necessity of taking discourse configurational languages into account in formulating hypotheses about Universal Grammar and elaborating the theoretical constructs which would enable such languages to be incorporated within the existing models of generative syntax.

The volume features an introduction by the editor and the following 11 papers: 'Structural focus, structural case, and the notion of feature-assignment' by Julia Horvath; 'Aspects of discourse configurationality in Somali' by Marco Svolacchia, Lunella Mereu and Annarita Puglielli; 'Residual verb second and verb first in Basque' by Jon Ortiz de Urbina; 'Structural properties of information packaging in Catalan' by Enric Vallduví; 'An F position in Western Romance' by Juan Uriagereka; 'Focusing in Modern Greek' by Ianthi Maria Tsimpli; 'NP movement, operator movement, and scrambling in Hungarian' by Katalin É. Kiss; 'Discourse configurationality in Finnish' by Maria Vilkuna; 'Focus and topic movement in Korean and licensing' by Hyon Sook Choe; 'The theory of syntactic focalization based on a subcategorization feature of verbs' by Mi-Jeung Jo and 'Focus in Quechua' by Pieter Muysken. Most of the papers elaborate on analyses previously advanced by the respective authors or other contributors to the volume and consequently, for those unfamiliar with the earlier work, make rather difficult reading. Though in the introduction É. Kiss provides a summary of the major issues dealt with in the volume, her discussion is quite compact and it is not always clear which of the positions that she discusses are currently held by the authors and to what extent their views coincide with her own. This is particularly problematic with respect to the notions of topic and focus which appear to be variously interpreted throughout the volume. Thus, for example, whereas for É. Kiss and some of the other contributors, both notions are clearly semantic rather than discourse based, for Vallduví they are informational notions. Informational as opposed to purely identificational foci are also discussed in the articles by Choe on Korean and by Svolacchia, Meru & Puglielli on Somali. Since within the prescribed space limitations I cannot hope to do justice to the contents of each paper, below I will only provide an overview of the range of syntactic treatments of the topic and focus offered.

There is no consensus among the authors as to the treatment of the structural topic. While it is considered to be external to the notional predicate, the syntactic category of the notional predicate appears to be subject to cross-linguistic variation. The topic is argued to be external to a VP (Hungarian, Finnish), IP (Basque, Catalan, Somali, Korean, Hungarian), T(ense)P (Greek, Hungarian) and Ev(idential) Phrase (Quechua). The actual location of the topic in the languages considered also appears to vary. It is claimed to occupy the specifier position of the CP (Somali, Korean) or TP

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(Hungarian) or to be adjoined to IP (Catalan), TP (Greek) or EvP (Quechua). In languages in which the topic is accompanied by a resumptive pronoun, i.e. Somali, Greek and Korean the topic is considered to be base generated in its surface position rather than extracted from the predicate by movement. Though in Greek this analysis is superficially incompatible with the fact that the topic observes island constraints, generally taken as indicative of movement, Tsimpli overcomes this problem by claiming that topicalization in Greek involves movement of a null operator at LF rather than at S-structure. An analogous analysis is proposed by Choe for Korean topics, which he argues are restricted to NPs (contrary to Jo), to account for the fact that subjacency does not hold in topic sentences while weak crossover effects are observed. The presence of multiple topics also receives various accounts. In Somali multi topics are derived by allowing the CP to be freely recursive. In the case of Hungarian, É. Kiss argues for placing one topic in [Spec, TP] and adjoining the others to the TP. This, she claims, is achieved by NP movement, a transformation which she redefines as creating a primary predication relation between the moved category and the source category.

Most of the discussions of focus build on the views of Horvath (1981, 1986) or Brody (1990). The former assumes that UG has a syntactic feature [+Focus], the formal properties of which are on a par with structural case features. The source and assigner of the focus feature is the V. In languages with a structural focus, the V assigns the focus feature to a constituent that it governs and is adjacent to. In languages with no structural focus, on the other hand, the focus feature is assigned freely to any category, i.e., focus is in-situ. According to Brody's alternative analysis, focus is an abstract functional head with its own projection. The focus operator occupies the specifier position of this functional projection. The V, which optionally carries the feature +F, moves to F (the head of the FP projection) in part by virtue of the Focus Criterion in (1).

(1) (a) The specifier of an FP must contain a [+F]-phrase.
    (b) All [+F] phrases must be in an FP.

The parametric variation between languages with a structural focus position and those with focus in-situ is captured with reference to the Focus Criterion, in that in the former it is observed at S-structure while in the latter only at LF.

In her contribution to the volume Horvath argues for a modified version of her original analysis under which the focus feature is assigned not by the V but by a functional head, the category of which (typically Infl or C) may vary across languages. She also argues against attributing the difference between languages with structural focus as compared to languages with focus in-situ to parametric variation at the level of the Focus Criterion on the grounds that this does not provide a systematic account of the cross-

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linguistic variation in the S-structure location of focus positions. A different critique of Horvath's original analysis is presented in the paper by Jo who argues that focus is not a subcategorizational feature of V, but is related to the V's ability to select predicate complements. This, in the case of languages in which predicate complements are adjacent to V, such as Korean and Armenian, he interprets as being suggestive of the existence of an A′-position within the VP. The A′-position is in turn a potential landing site for focus. One of the advantages of this analysis suggested by Jo is that no stipulations need to be made about the directionality of focus assignment (it follows from the position of subcategorized nonarguments in a language) and furthermore the directionality of focus assignment may be the opposite of that of case assignment and theta-marking, as is the case in Hungarian.

Tsimpli in her contribution, on the other hand, adopts a version of the structure proposed by Brody in which the value of the focus feature carried by the head of the FP projection is subject to parametric variation; in languages with structural focus the focus feature is always [+F], in languages with focus in-situ it is [−F], and in languages such as Greek it may have either value. Movement of a focus phrase to the specifier position in the syntax is argued to follow from a modified version of the Focus-Criterion which requires that a [+F] head must be in specifier-head agreement with a [+F] operator. Movement of focus phrases at LF, in turn, is seen to be motivated by scope requirements. A similar analysis of focus is advocated in the paper by Choe for Korean, a language which allows more than one constituent to bear focus. Choe argues that both information foci and contrastive foci in Korean may move in syntax or at LF and are licensed in the same way. He also argues for a parallelism between foci and wh-phrases rather than one between foci and case assignment, as advocated by Horvath. The paper by Uriagereka also assumes an FP projection but its head is not only focus but any operator expressing point of view. Within the context of the minimalist framework he elaborates an analysis which seeks to account for some fundamental differences among Romance languages involving clitic placement and different types of focusing in terms of the strength of the features of F and the extent to which they are matched by the heads raised to F.

In contrast to all the above analyses, Vallduví argues that in Catalan, no focus movement is involved in focusing but rather the non-focused constituents are moved whereas the focus remains in-situ. Under his analysis everything that is not new information is dislocated from the IP. Vallduví, unlike all the other contributors, also explicitly argues for a cross-linguistically uniform abstract level of representation of Information Structure which mediates between surface syntax and the informational component.

In the light of the above analyses, discourse configurational languages do not appear to require radical revisions of generative syntax, but rather

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relatively minor, though significant, adjustments such as extending the range of functional categories or recognized syntactic features and their values and allowing for a greater range of movements at S-structure as opposed to merely LF. Nonetheless, the exact nature of these adjustments will undoubtedly occupy much of linguistic theorizing in the years to come. The proposals contained in the papers in this volume are bound to pave the way for subsequent analyses. Any practitioner of generative syntax seriously interested in cross-linguistic variation will need to consult them.

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Peter Ladefoged & Ian Maddieson, *The sounds of the world's languages*. Oxford & Cambridge, MA: Blackwell Publishers Inc., 1996. Pp. xiv + 425.

Reviewed by KATHARINE DAVIS, University of Washington

Peter Ladefoged and Ian Maddieson, well-known researchers at the University of California at Los Angeles, have compiled a large number of articulatory and acoustic studies into a framework that is accessible to phoneticians and phonologists alike. At last the world has a comprehensive text that describes, in the authors' words, '… all the segments that are known to distinguish lexical items within a language' (2). As discussed in the first chapter, this criterion defines a consistent level of linguistic analysis throughout the book. For example, [ŋ] is included in the discussion of English because it distinguishes the word *rung* from *run* and *rum*; the fact that its distribution is limited does not negate its status as a distinctive sound. In addition, prosodic features are not treated in this volume, even though they may also distinguish words, because they are not segmental. Finally, the authors note that their discussions are based largely on observations of careful speech, the style most likely to exhibit distinctive productions.

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Subsequent chapters each deal with a particular class of speech sounds: Places of Articulation, Stops, Nasals and Nasalized Consonants, Fricatives, Laterals, Rhotics, Clicks, Vowels and Multiple Articulatory Gestures. A Coda chapter provides a summary of phonetically supported oppositions which the authors feel should be considered in universal feature theories, and an Appendix lists the languages and language families mentioned throughout the text. This book will find a wide audience of phoneticians, phonologists, and other speech researchers, but it is by no means an introductory text. Readers must be familiar with vocal tract anatomy and instrumental output such as waveforms, spectrograms, spectra, palatograms, and air pressure readings in order to fully appreciate the material presented.

The authors point out that, although evidence from a large number of studies is presented, the book is not an exhaustive literature review. Ladefoged & Maddieson have for the most part selected studies which give the reader a coherent view of the sound in question. Considering that their book is already over 400 pages long, this may be a wise choice; however, it does result in the exclusion of certain points of view. For example, in the section on breathy voiced stops, although they mention that the amplitude of vocal fold vibration may be attenuated before the stop release in languages such as Hindi and Owerri Igbo (61), they do not report a study of Nepali which shows that pre-release voicing may be entirely absent in some breathy voiced stops (Poon & Mateer 1985). Thus, in the breathy stops of some languages, pre-voicing may not be a required part of the contrast (such a scenario would be allowed according to the stop phonation matrix on p. 100, but this view is not considered explicitly in the text). In the same chapter, there is a discussion of aspiration in which the authors conclude that '… aspiration is a period after the release of a stricture and before the start of regular voicing …' (70), without mentioning that some researchers believe the relevant segmentation point to be the onset of the following vowel's higher formants rather than the onset of periodicity (Fischer-Jorgensen & Hutters 1981). The reader should bear in mind that, although the summaries presented in *The sounds of the world's languages* are quite admirable, some valid possibilities have indeed been left out.

Even considering the above caveat, it can be said that Ladefoged & Maddieson provide the reader with a huge variety of phonetic facts. The chapter on clicks is typical of the depth of information found in *The sounds of the world's languages*. First the authors give a general description of the sound class ('… the essential component is the rarefaction of air enclosed between two articulatory closures formed in the oral cavity, so that a loud transient is produced when the more forward closure is released' (246)), followed by a brief summary of the language families and geographical locations in which clicks are found. Next comes a detailed discussion of articulatory properties, including place of articulation. Although languages are not known to contrast clicks at more than five places, determining the

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actual place of articulation of any particular click can be complicated because the extent of the occlusion varies during production. Evidence from cine-radiological and palatographic studies sheds some light on this issue, but it is noted that different speakers often use different production strategies to achieve similar auditory results. Ladefoged & Maddieson then provide acoustic descriptions of click contrasts based on waveform analyses. Crucial factors include the amplitude and duration of noise at release, the timing of maximal intensity, and region of spectral energy. The situation is further complicated by the involvement of the posterior closure, laryngeal settings, and nasality, which can result in productions such as the voiceless aspirated velar nasal accompaniment to the alveolar click in Nama. All of these factors are considered in depth using evidence from additional waveforms, air pressure readings, oral and nasal airflow readings, and tables of minimal and near-minimal contrasts. The potentially overwhelming amount of information is clearly conveyed and never strays far from the essential question of what determines phonetically and phonologically relevant click contrasts (see pp. 275–278 for an interesting discussion of this issue).

Much of the material is instructional for its methodology as well as its phonetic facts. For example, the section on the acoustic structure of voiced nasals describes a study conducted by the authors. Ladefoged and Maddieson guide the reader step-by-step through a process that enables them to infer the articulation of Arrernte voiced nasals from acoustic data. First, it is explained how the frequency of a nasal zero (area of reduced energy in the frequency range or anti-resonance) has an inverse relationship to the volume of the oral cavity in front of the velo-pharyngeal port. In other words, a labial nasal should have the lowest frequency anti-resonance and a velar nasal should have the highest. It is then explained how this inverse relationship may be disturbed by changes in tongue body position, i.e. a lower tongue position at a retracted location may result in a zero that is even lower than that of a more forward closure position. The average zero frequencies of Arrernte nasals turn out to be: dental = 1506, alveolar = 1403, retroflex = 1634, and palato-alveolar = 2094. The lower value of the alveolar compared to the dental suggests a laminal dental production. A laminal dental will actually create a smaller oral cavity than an apical alveolar due to the increased contact between the tongue surface and the roof of the mouth. Thus, without any x-ray or palatogram equipment, the authors have inferred the articulation of Arrernte nasal stops. This type of study is especially valuable to phonologists who might want to confirm the feature status of a particular segment. Such information can frequently be obtained using simple phonetic methods, and the studies described in *The sounds of the world's languages* may well encourage more phonologists to apply these methods in their own research.

Some questions have so far defied phonetic explanation. Many linguists have wondered what it is about Spanish taps, Finnish trills, English

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approximates, and German uvular fricatives that makes us consider them all 'r-like'. The chapter on rhotics addresses this question at length, giving the most credence to Lindau's (1985) suggestion that each one shares a common property with every other, but that these shared properties are not consistent across the whole class. That is, a trill's short closure duration may resemble a tap, and its open phase may resemble an approximate (245). Many historical changes can be explained in this way, but Ladefoged & Maddieson point out that non-rhotics can be just as similar (e.g. taps and trills are like stops in that they all have closures). The rather disappointing, if accurate, conclusion is that we think of these sounds as a class of rhotics because we represent them all with the letter 'r' in our orthography. It is comforting to learn from the chapter on vowels that 'Rhotic vowels always have a lowered frequency of the third formant' (313).

Diagrams, figures, tables, tracings, and pictures contribute enormously to the book's value. The illustrations are very informative, sometimes reproduced from the original articles and sometimes provided by the authors' own archive of phonetic analyses. Occasionally, Ladefoged & Maddieson might have made a better choice, for example, on p. 195, spectrograms are presented to illustrate differences among four coronal laterals; although a table of formant values does appear nearby, spectra would probably have made the point more clearly. However, apart from a typo or two, the only obvious mistake appears on page 311 where the vocal tract photos of strident v. non-strident vowels appear to be reversed relative to the caption. On the whole, the authors have done a remarkable job integrating hard phonetic data with the explanatory text.

Ladefoged & Maddieson have put together a well-written, well-organized volume that is certain to become a standard reference in the field. The book supplies excellent background information for phoneticians and phonologists working on specific linguistic issues, and will also be very useful for computer scientists and perception researchers who are often not aware of certain phonetic properties and linguistic contrasts that may affect their research results. The danger is that people will come to rely on it too heavily, perhaps forgetting that new investigations are quite likely to turn up new facts. We should not become complacent and take everything in *The sounds of the world's languages* as the final answer. We should instead keep asking questions, perfecting techniques, and uncovering new facts for the next edition.

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David Pesetsky, *Zero syntax*. Cambridge, MA: MIT Press, 1995. Pp. xviii + 351.

Reviewed by THOMAS ERNST, Rutgers University

The main goal of *Zero syntax* is to meet the challenges posed by Experiencer predicates (such as *fear*, *anger*, and *annoy*) for a restrictive view of the linking between argument structure and syntax.[1] A common assumption is that a given set of θ-roles is always to be mapped onto some particular array of positions in a clause (formulated as the UTAH in Baker 1988). But if this holds, then why should the Experiencer associated with *fear* be a subject, while the Experiencer of *frighten* is an object? Pesetsky argues that a combination of a fine-grained analysis of θ-roles, plus some syntactic movements after mapping, allow preserving the UTAH.

The book derives its title from Pesetsky's use of abstract zero morphemes to build the account of such alternations. Most centrally, Pesetsky proposes a zero causative 'CAUS' to account for the fact that alongside object-experiencer verbs like *annoy* there is no corresponding verb meaning 'to be annoyed'. He also posits a zero morpheme 'SUG' for cases like *John's manner is proud*, where the adjective is really [*proud*+SUG], i.e. 'suggestive of pride' (since only a sentient being, not a manner, can be proud), and a zero-preposition G for double-object constructions, by which *give John a book* is really *give John* [$_{PP}$ *G a book*].

Pesetsky's justification of his analyses of CAUS and G, and of the theoretical apparatus needed for this enterprise, form the bulk of the book. Two proposals are especially crucial: his analysis of subject experiencer verbs as bound roots combined with CAUS in syntax to form object-experiencer verbs; and the dual-track system by which every sentence has two phrase structures, one 'layered' and one a 'Cascade'. Below I will focus mostly on these two proposals.

The first two chapters present an overall introduction and a discussion of linking problems, showing what the general difficulty is and how specific

[1] I would like to thank David Pesetsky for discussion of some of the material in this review.

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approaches to the experiencer-predicate problem handle it. Chapter 3 distinguishes three types of Themes occurring with experiencer predicates; Target, Subject Matter, and Causer. Thus in (1a) (Pesetsky's (30a)) *the article in the Times* is a Target ('evaluated' in some way by Bill); in (1b) (= 36a) *John* is the Experiencer and *the television set* the Subject Matter; in (1c) the latter is the Causer:

(1) (a) Bill was very angry at the article in the *Times*.
(b) John worried about the television set.
(c) The television set worried John.
(2) The article in the *Times* angered/enraged Bill.

It might have seemed that (2) (Pesetsky's (30b)) poses a problem for the UTAH, as it apparently shares one θ-structure with (1a). But with the finer-grained semantic distinctions shown here, it does not, and so the two sentences need not be derivationally related. As (3) illustrates, Causer and Target/Subject Matter may not cooccur; this Target/Subject Matter Restriction (henceforth T/SMR) figures crucially in Pesetsky's justification of CAUS and Cascade structures:

(3) *The article in the *Times* angered Bill at the government.

Chapter 3 also provides a major piece of evidence for both SUG and CAUS, from nominalization patterns. Given Myers' Generalization, by which words formed by zero-derivation block further derivational morphology, the data shown in (4a, b) (Pesetsky's (208b), (211b)) are accounted for. The causative *annoy* is made up of a bound root $\sqrt{}$*annoy* + CAUS, and the presence of the latter blocks nominalization; *angry* is [angry + SUG], and so likewise cannot be nominalized:

(4) (a) *Our constant annoyance of Mary got on our nerves.
(b) Your remarks were angry/*your remarks' anger

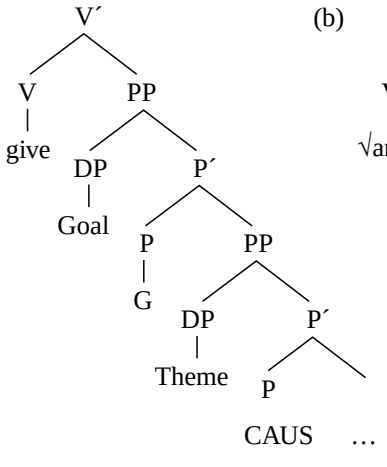
Chapter 4 provides a justification for Pesetsky's bound-root analysis of subject-experiencer predicates, analyzing them as parallel to French reflexive verbs. Such roots require their external argument be Controlled, and this can only be done either with reflexive clitics (as in French) or by replacing the original external argument with Causer, in causatives. Thus (2) above allows the overt verb *anger* (= $\sqrt{}$*anger* + CAUSE, where $\sqrt{}$*anger* means 'be angry') because the zero morpheme replaces the original T/SM argument of the bound root with Causer. Chapter 5 provides justification for a third zero morpheme G, a preposition introducing the theme argument in ditransitives like *give*; it Case-marks this theme, and incorporates into V, as indicated by the predicted impossibility of nominalizations (**Sue's gift of Mary* (*of*) *a book*).

These strands of the analysis come together in chapter 6. Crucially, both G and CAUS must raise, by head-movement, to incorporate with V. They do

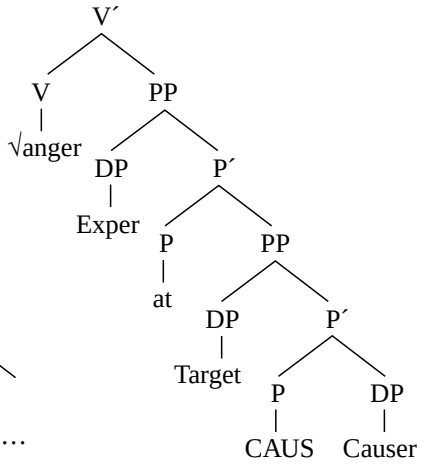
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so in a Cascade structure (a novel variant of 'VP-shells' à la Larson 1988), illustrated in (5a, b), in which the first complement of V may occupy the Spec of its sister, which is a PP (adapted from Pesetsky's (511)–(512)):

(5) (a)



(b)



(5a) is grammatical, with CAUS raising to G and the amalgam raising to *give*. (5b) (for (3)) is ungrammatical, because $\sqrt{}$ *anger* is illegitimate without CAUS, but the latter cannot raise past *at* due to the Head Movement Constraint. Thus the T/SMR is explained, given CAUS and this sort of Cascade structure, because the P of the T/SM argument (*at* in (5b)) will always block raising of CAUS, and without CAUS the Control requirement on bound roots like $\sqrt{}$ *anger* cannot be met. In legitimate cases like 1c Causer raises to subject position (to avoid suppressing $\sqrt{}$ *anger*'s θ-role, a Projection Principle violation, Pesetsky posits a base-generated CAUS affix on V so that Causer is the subject to begin with). The lower CAUS must raise to check off strong features (parallel to Chomsky's 1995 Affix Checking operation).

The rest of chapters 6–7 fleshes out the principles needed for Cascade syntax and the parallel, linked system of Layered syntax, i.e. the more traditional disposition of complements as sisters in a flat structure, with adjuncts adjoined above them. The former is necessary to handle the so-called Barss-Lasnik effects illustrated in (6), which motivated the VP-shell family of analyses (by supporting 'down-to-the-right' mapping of double objects as in (5a), while Layered structure is still needed to capture most patterns of constituency and semantic interpretation.

(6) (a) The therapist gave Karen$_i$ herself$_i$. (Anaphor-Binding)
(b) They gave every worker$_i$ his$_i$ paycheck. (Pronoun-Binding)

Zero syntax is a well written book, couched for the most part in clear, precise statements, with useful interim summaries, statements of goals, and

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other signposts for the reader. It is well thought-out and organized, it attempts to cover a wide range of related data, and, notably, for the most part it is careful in identifying and addressing potential problems. It has no major editorial problems, with very few typographical errors and a useful index.

Of course, the more important question is whether Pesetsky's analysis stands up, and here the answer is less clear. Four points bear mentioning in evaluating the success of his proposals.

(i) The data are often murky, with conflicting or overly subtle judgments on crucial sentences. For example, the analysis in (5a) requires contrasts like (7a, b) to justify the causative nature of double object sentences (Pesetsky's (494), from Oehrle 1976).

(7) (a) The war years gave Mailer his first big success.
(b) *The war years give his first big success to Mailer.

Everyone I have consulted finds (7b) slightly odd at worst, and perfect with *a big success*. Similarly, Pesetsky argues from cases like (8) (his (294b)) that reflexives are barred from *by*-phrases, a fact used to avoid a problem for his bound-root analysis (102 ff.).

(8) ?*Bill was taught by himself to ride a bicycle.

Again, most people I have consulted find (8) only slightly bad, and perfect with contrast on *himself*, especially if the PP is extraposed; what unacceptability there is in (7)–(8) may surely be chalked up to low-level pragmatic factors, as has often been pointed out.

(ii) Several counterexamples to his proposals can be handled only at some cost. (9a) should only be possible with CAUS raising to $\sqrt{}$ *irritate* as in (5b), since *with* blocks this movement just as *at* does for (3); in (9b) (Pesetsky's (538a)) *into* should do likewise.

(9) (a) Mary irritated John with her mistakes.
(b) John broke the cookie into little pieces.

Pesetsky proposes that in cases like (9b) 'weak features' on the verbal affix CAUS allow the lower, zero-preposition CAUS not to raise, and no Head Movement Constraint violation results. But this is a stipulative move, in the absence of independent evidence for when features are weak and when they are strong. As for (9a), it is crucial that prepositional CAUS be an adjunct, so that it occurs lower than these PPs in (5b), so perhaps it could be accounted for if *with her mistakes* is even lower. But this begs the question of why adjunct CAUS occurs higher than a (rather argument-like) instrumental PP.

Similarly, if ditransitives like *give* always take an NP and a PP (headed by either G or *to/for*) as in (5a), we should not find the distinction in (10a, b)

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(which follows naturally if the NP/PP difference is maintained for the second complement).

(10) (a) *Fran gave Jim quietly [$_{PP}$ G a book].
(b) ?Fran gave a book quietly [$_{PP}$ to Jim].

(iii) Despite Pesetsky's care in justifying his positions, they raise many theoretical questions. For example, why should only the zero-preposition G incorporate into English verbs – that is, why doesn't its overt analog *to* incorporate, parallel to Bantu applicative morphemes (Baker 1988)? Also, can we live with an analysis requiring several CAUS or G morphemes, with different properties (195, 154), all abstract and detectable only indirectly?

(iv) Lastly, although Pesetsky carefully works out the required principles and the correspondences, the dual system including both Cascade and Layered syntax is a substantial addition of machinery to the theory. It is largely motivated by the fact that VP-shell analyses get many constituency and interpretation facts wrong – these are to be represented instead on Layered structures. But the main motivation for VP-shells depended on the rejection of *precedence* as a structural condition to handle Barss-Lasnik effects. To my knowledge, no defense of VP-shell theories has given an argument against precedence aside from its being unnecessary and thus eliminable. Yet if it is retained (see Jackendoff 1990, Williams 1993, Ernst 1994; even Pesetsky invokes precedence, e.g. p. 234), a major argument for VP-shells loses its force, and so, correspondingly, do Cascades. Since one specific justification for Pesetsky's Cascade version of VP shells (its solving the problem that the c-command-based conditions reponsible for Barss-Lasnik effects seem to ignore c-command in PPs) is mitigated by the existence of other possible solutions within a traditional layered structure, the independent motivation for Cascades is weakened. It may be that the success of Pesetsky's proposals provides compensating evidence, but as long as one still admits the need for traditional, layered syntax, one must have very strong justification for Cascades to offset the addition of complexity that they represent.

The conclusion must be that the proverbial jury is still out on whether Pesetsky is right about zero morphemes, Cascades, the bound-root analysis of subject-experiencer predicates and the specific mechanisms that tie these together.

However, we must balance this with the richness of Pesetsky's proposals, his serious attempt to reconcile evidence for Cascades v. evidence for Layered syntax, and his attention to both empirical and theoretical detail. From this perspective, *Zero syntax* is valuable for its ability to raise interesting issues and provide provocative data. If for nothing else, Pesetsky's fine-grained treatment of experiencer predicates, with its detailed consideration of θ-roles,

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nominalization possibilities, and so on, is useful in considering the linking problem. His proposal that causation may be introduced (at least in part) by an adjunct preposition has implications for the analysis of ergatives and resultative constructions, especially with respect to the issue of lexical v. syntactic derivation. And it also forces consideration of distinctions among adjuncts and of the argument/adjunct distinction: CAUS must be mapped onto a lower position than some (other) adjuncts (recall (9a)), and Pesetsky's account of adjunct islands must recognize a group of adjuncts which act somewhat like complements.

Finally, there are two areas that seem to me especially under-discussed, for which Pesetsky's ideas are particularly useful. First, he accords a special role to prepositions in Cascades. Though he solves the c-command problem at a cost, his careful working out of the phrase structural and θ-theoretical implications of prepositions' role in Cascades marks *Zero syntax* as one of those rare books that take prepositions seriously.

Second, there is the matter of the two sets of phenomena that appear to motivate both Cascades/VP-shells and Layered syntax. Since Larson (1988) the VP-shell approach has become a (perhaps *the*) major conception of VP-structure, to the point where Kayne (1994) and its derivatives completely deny the possibility of right-adjunction. Pesetsky squarely faces the strong evidence for Layered, adjoin-up-to-the-right phrase structure, and is one of the very few works that attempts to reconcile the competing evidence. This alone is worth the price of the book.

In sum, *Zero syntax* is a thought-provoking treatise on experiencer verbs and phrase structure, with specific analyses that may meet with skepticism, but also rich in ideas and carefully worked out detail. Considered alongside the many mere mechanical applications of the latest theoretical fad, it stands out favorably.

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Neil Smith and Ianthi-Maria Tsimpli, *The mind of a savant: language learning and modularity*. Oxford: Basil Blackwell, 1995. Pp. xviii + 243.

Reviewed by Luca Bonatti, University of Paris VIII at Saint Denis and New York University

This is a very interesting book that any linguist, cognitive scientist or philosopher of mind will love to read. It is a single case study of a savant, Christopher (C), with exceptional language abilities. Generally, savants are very good at some specific cognitive domains but their mastery of language is poor. As Smith & Tsimpli observe, the converse is very rare and this is already a good reason to appreciate their book, which offers the most detailed existing analysis of a case of this sort.

Smith & Tsimpli have two main objectives. One is descriptive; they carefully assess C's deficits and abilities, as the eyes of the linguist would see them. The other is more properly theoretical. They use C's case to test Fodor's modularity thesis and Anderson's theory of intelligence, and to draw general conclusions about the organization of the mind. While I think that their first task has been achieved, I have some doubts that C's case is really appropriate for their second task. I will begin by presenting and critically discussing their descriptive work; I will then examine their more general conclusions for cognitive architecture.

C, a thirty-three year old right-handed native English male, was born with a damaged brain, possibly as a consequence of hydrocephaly. Recent MRI showed a 'moderate cerebral atrophy with wide sulci over both hemispheres' (4), that is, a diffused malformation causing overall retardation and malfunctions. Smith & Tsimpli document its quite severe effects on C. His hand-eye coordination is so poor that in his ordinary life he is unable to look after himself. His cognitive abilities are no less impaired. In many non verbal tests C performs roughly at mental age 9. He fails Piaget's conservation of number task, as well as the false belief task that four-year olds, but not autistic children, pass.[1]

Yet in striking contrast, as Smith & Tsimpli document, his performance on standard verbal tests is normal or better than normal. More strikingly, he has some degree of knowledge 'ranging from fluency to the bare elements' (12) of some fifteen languages, and can learn traits of new languages very fast. Still, in spite of his exceptional abilities, C's linguistic competence is not uniform. He finds it easy to acquire the lexicon and some morphosyntactic properties of a language, but although he can do literal translation across languages, when translating he often misses the intended sense and 'produces

[1] However, C does succeed in a version of it, the 'Smarties' task. Smith & Tsimpli have an elegant explanation for this apparent inconsistency that I cannot discuss for lack of space.

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an output that is flawed to the point of incoherence' (156) even in the languages he knows well. Add to this complicated picture the fact that even in his native language C's performance is not flawless.

What could generate such a complex cognitive profile? What does C really know about languages, that allows him to be so proficient in spite of his deficits, and why are his linguistic abilities, albeit exceptional, always limited? Smith & Tsimpli try to clarify these difficult points with a clever series of studies. They first try to assess C's native language proficiency and the nature of his violations from the norm (Ch. 2). They further explore how C masters languages that he learned without explicitly controlled tutoring (Ch. 3). Then (Ch. 4) they set up two more controlled cases ideally suited to assessing C's learning strategies. In the first one, Smith & Tsimpli teach him Berber, a real (hence possible) natural language previously unknown to him. In the second, they invent a non-existent language – Epun – whose properties are not shared by any existing and probably by any possible natural language. Crucially, in both case studies the order and nature of the data presented to C was monitored and in the second his learning patterns were compared to those of normal subjects. So Smith & Tsimpli put themselves and the reader in an optimal position to explore C's learning mechanism and its sensitivity to linguistic universals. The plan is very well conceived. Let me sum up Smith & Tsimpli's main conclusions on these topics.

After carefully analyzing C's natural language proficiency, Smith & Tsimpli argue that his competence can be considered intact. The conclusion is not trivial because C does diverge from normal speakers in ways that might indicate a defective grammar. For example, he rejects sentences involving topicalization, dislocation and sometimes extraposition.

Their argument has two steps. First, they show that C also deviates from the average speaker in other disparate domains. He does not seem to fully understand ironies, metaphors, and puns; he tends to interpret rhetorical questions literally; he doesn't accept sentences when language is mentioned rather than used; and he also finds garden-path and center-embedding constructions exceedingly difficult. Smith & Tsimpli suggest that all these contexts have two things in common. They require many computations (e.g. center-embedding) and/or interpretive, or 'metarepresentational' second-order, abilities (e.g. ironies and rhetorical questions). Both factors – and this is the important fact – are beyond grammar proper.

Second, they tentatively (but convincingly) argue that those same two factors that evidently cause C many difficulties suffice to explain his rejection of topicalizations, dislocations and extrapositions. Such structures require BOTH properly syntactic operations, such as operation-variable structures or binding at LF, AND a further distinct, post-LF level for coreference assignments. Because, as Smith & Tsimpli show, *wh*-movement constructions or relative clauses pose him no difficulties, Smith & Tsimpli conclude that C masters the necessary syntactic operations and therefore his deviant

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judgments must be caused by difficulties at this further, post-grammatical level of representation. This, like other problematic contexts for C, requires extra processing efforts that tax C's central abilities too heavily (56–57). Thus C's deviations from native speakers' judgments come from the interface between grammar and central processes, and are not due to grammatical deficits.

Their conclusion is well argued, but maybe too well argued. There is an alternative possibility that doesn't require all their fine-grained syntactic analyses: C's reported deviant judgments might just depend on his interpretation of the tasks. Smith & Tsimpli often notice his excessive willingness to cooperate with the linguist, as if he were almost obsessively concerned with form exactness. So, perhaps he answers like we would do if we were asked to be picky and single out the less prototypical (more 'awkward') English constructions. But if I am right, in this case (although not in general; see below) so much the better for Smith & Tsimpli's conclusion. If C's odd linguistic judgments are an effect of task interpretation, this is a further, more direct reason to conclude with them that his native linguistic competence is intact.

But what really makes C special is his exceptional foreign language competence, so it is very important to be clear on what this competence is. There, however, matters are much more complex.

Smith & Tsimpli first describe C's proficiency with languages learned without controlled tutoring (mostly Greek and Spanish), also trying to draw lessons for current debates in second language acquisition. I cannot do justice to their detailed analysis here and will concentrate on their main conclusion as I understand it. Across many different tests, Smith & Tsimpli constantly find that C masters and learns the vocabularies of foreign languages much better than their grammars. So Smith & Tsimpli write that 'the basis of C's exceptional second language learning abilities lies in his "enhanced" lexical sub-component … in contrast, structural differences between his first and other languages appear difficult for him …' (85). Also, L1 grammatical interferences in C's L2 productions are 'overwhelming' and 'abnormally persistent' (81; see also 119–120).

How should one interpret this difference between lexical and grammatical abilities? There are at least two, quite different, possibilities. One is to say that C doesn't really KNOW foreign languages, but only their vocabularies. The other is to say that C really knows the languages – hence their grammars too – but for some reason he cannot deploy his grammatical knowledge as clearly and fast as his morpholexical knowledge. Smith & Tsimpli envision both possibilities, but don't clearly adopt one. At times they suggest that perhaps C has only ONE grammar, that is, that 'C's syntax is basically English with a range of alternative veneers' (122), the deviations from English (such as C's acceptance of null subjects in Greek, Spanish and Italian) being easily explained by low-level generalizations over the morphological component.

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Other times they suggest that C's bad performance is due to 'grammatical inhibition' (121) caused by the high computational costs of syntax (as opposed to lexical) access and processing. The two explanations, although not incompatible, are different. The latter explains C's poor grammatical abilities by appeal to performance flaws (once again, a 'flawed interaction of the modular and the central' (121)) in the presence of a possibly rich L2 grammatical knowledge, whereas the former denies that C knows L2 grammars. Even if it is certainly difficult to embrace one single explanation, it does make a difference for the value of Smith & Tsimpli's analysis which one is right. Often Smith & Tsimpli use C to elucidate some issues in L2 learning that are unresolved even for normal subjects. Notably, they ask whether L2 acquisition involves parameter resetting. They spend many pages in exploring the alternatives, and try to argue that in C's case it doesn't. However, if the first possibility holds and C only knows English grammar, then this issue does not even arise. But if the second possibility holds and C does indeed know many grammars, then all such parts of the book are entirely to the point. Also the reliability of C's metalinguistic judgments depends on which explanation is correct. If C always responds by consulting only L1 grammar, then his L2 judgments are just native intuitions in another guise and give no information on his second languages. If, instead, C does know L2 grammars, then his intuitions may reflect them and Smith & Tsimpli's detailed analyses are worth the effort. Smith & Tsimpli are aware of the problem (see p. 122) but leave it as they find it.

Uncertainty on this point also partly affects the last (otherwise excellent) series of studies, aimed at assessing C's learning strategies under controlled conditions. Smith & Tsimpli state that, in teaching C new languages by controlling the order of data made available to him, they want to 'test the predictions made by the principles and parameters framework of current linguistic theory' (123). However, their aim makes sense only under the assumption that C does indeed learn grammars. Yet, their results are often compatible with the possibility that, confronted with a new language, C only minimally accommodates English to the new lexicon and does not acquire a grammar at all. So for example Smith & Tsimpli test whether, in the absence of positive relevant data, C can realize that Berber (which is generally VSO but still allows SVO in declarative clauses, and allows null subject constructions) has prepositions rather than postpositions and is pro-drop. C does seem to get both facts right, but what should one conclude from this? English too has prepositions, so this is the null hypothesis anyhow. And C might accept Berber's null subject sentences not because, as Smith & Tsimpli claim, his generalizations are achieved 'via the pressure of UG' (137), but for other totally different reasons. He might use rules of thumb such as 'Rich morphology, no obligatory subject' obtained through general induction or

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by analogy with other pro-drop languages like Italian, to which he has been exposed.[2]

In fact, if C's generalization were grammatical in nature and taken 'under the pressure of UG', then we would expect other consistent behaviors that C doesn't exhibit. Consider for example the gap between sentence acceptance and sentence production. Smith & Tsimpli find that C ACCEPTS Berber sentences whose structures differ from English (e.g. null subject sentences, or VSO constructions), but he PRODUCES only Berber sentences with English word order. Had he made a generalization about the GRAMMAR of Berber, why should he apply it in parsing mode and not in production mode? If anything, one should expect the opposite. Furthermore, he rejects other constructions that should be associated with null subject parameter setting. Smith & Tsimpli interpret this as evidence that C does not reset parameters in L2 learning (132; 137), by evidently assuming that C does learn a grammar for Berber. But all these discrepancies might mean that C is not LEARNING A GRAMMAR AT ALL, but just using English. A fortiori, surely he is not resetting parameters, but this is trivial and uninformative for L2 learning.

Once again, Smith & Tsimpli don't exclude this possibility. They write that 'it is possible that despite speaking many languages, C really only has one grammar' (129). They resort again to the difference between encapsulated and central processes, arguing that a line should be drawn between 'those aspects of [C's] linguistic behavior that are a function of his encapsulated language faculty and those that are a function of his very considerable encyclopedic knowledge' (129) and evoke the possibility that C PRODUCES Berber SVO constructions because his encapsulated (English?) grammar leads him to do it unconsciously and automatically, whereas he ACCEPTS VSO constructions because his encyclopedic knowledge makes him consciously aware that such order exists (131). But this amounts to the recognition that, also for Berber, C's knowledge is not strictly speaking GRAMMATICAL knowledge, and this has the further consequence that Smith & Tsimpli's detailed linguistic analyses often ask questions that C's case may not be suited to answer.

A more complex pattern emerges from the last type of investigation Smith & Tsimpli undertake. They invent a language, Epun, which includes rules that don't exist in any known language and are likely to be linguistically (but not logically) impossible. So in Epun emphasis is expressed with a suffix occurring at a fixed (structure-independent) ordinal position in a sentence, and this is incompatible with basic principles of linguistic theory. Epun also violates apparently universal morphosyntactic principles, in that it expresses agreement between verbs and complex co-ordinate noun phrases by means of

[2] Indeed, C also *accepts* – although he doesn't *produce* – sentences with freer word order than Berber allows, like V XP S sentences that are ungrammatical in Berber but not in Italian.

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constructions like 'I and Mary love3rd-person-feminine-plural flowers'. Notice, however, that agreement violations like these obviously are less extreme and of a different nature than the presence of structure-independent syntactic constructions.

Other rules, albeit structure-dependent, still violate some conditions on UG. So Epun has no overt negation: whereas the normal word order of positive sentences is SV(O), negation is expressed by a word order change into VS(O), without any morphological change. Smith & Tsimpli hold that a formal account of such a rule would appeal to obligatory verb raising in negative sentences, which in its turn would require a structure having both the head and the specifier of NEG empty, ad this runs counter to UG because it violates recoverability conditions.[3]

Smith & Tsimpli compare C's learning patterns for Epun with that of controls (beginning linguistics students) and find three interesting results. First, neither C nor the controls were able to guess the right syntactic rule for impossible structure-independent operations (emphasis). Second, C – but not controls – could find at least one of the odd morphological rules for agreement. Third, controls – but not C – could discover impossible structure-dependent rules (e.g. negation). Smith & Tsimpli interpret the first finding as a demonstration that 'strong' violations of basic principles of UG are too difficult for everybody, whereas they tentatively take the second as a further demonstration that C's morphological abilities are superior to his syntactic abilities. But they rightly proceed with care here, given the sharp difference in the kinds of violations mentioned above. As for the third finding, once again they appeal to the difference between modular and central processes. They argue that when a rule is not 'strongly' impossible but nevertheless forbidden by UG, normal subjects can recover it by using central resources, but C's poor general intelligence forbids him to do so – hence controls' success and C's failure.

Thus general resources, syntax and lexicon all have their own role to play. When UG really forbids something, nobody can learn, whether by 'linguistic' or 'general' means. When, instead, UG is only 'weakly' violated, general intelligence can succeed where modular syntax fails. And, as if on a parallel track, morphology deploys its own learning procedures independent from both syntax and general learning, as witnessed by C's success in retrieving odd morphological rules where normal controls fail.

This is what Smith & Tsimpli's excellent descriptive work tells us. It is now time to see how they think it supports the general image of the mind they propose.

Let me begin with a note of caution. It is not at all easy to evaluate Smith

[3] It should be noticed, however, that the rule is also semantically incorrect, because it wouldn't allow for negation to range over complex sentences containing several main clauses tied by propositional connectives like 'It is not the case that A or B or C and D'.

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& Tsimpli's overall model. One source of difficulty is inherent in any single case study: conclusions on such a basis, whether positive or negative, are always risky. To be sure, Smith & Tsimpli don't base their model on C: rather, they use C to confirm and sometimes accommodate a model they already possess. But then its overall plausibility is independent of the present case study and a fair assessment would require a detailed analysis of each of their numerous proposals. However, the greatest source of difficulty comes from Smith & Tsimpli's change in tactics. In their last chapter, they abandon the carefully slow path followed in the analysis of C's linguistic abilities, shift the engine into fifth gear and submerge the reader with a cascade of boxes, and of hypotheses about their interconnections and their accessibility. All this, once again, goes well beyond C's case.

So, rather than discussing their model, I will limit myself to some simpler tasks. I will first show how Smith & Tsimpli explain C's performance by means of some of its aspects. I will then present and criticize Smith & Tsimpli's argument that C's case calls for a modification of Fodor's modularity thesis and Anderson's theory of intelligence.

I have already shown how the interplay between language structures and general knowledge is used by Smith & Tsimpli to explain C's linguistic behavior. But Smith & Tsimpli think that they can also make room for the many general cognitive oddities of C, by exploiting one or the other features of their rich model. So, for example, they must explain why he is good at verbal tasks and bad at spatial tasks. To do this, they appeal to the fact that the mind has two separate processors, one for spatial inputs and one for verbal inputs (to which the language module has access and is accessed by). They thus propose that C's deficit is due to his enhanced verbal processor and his defective spatial processor.

They must also explain why C knows many lexicons, has good conceptual knowledge, yet is very poor at translating. So they propose that not all verbal activity is carried out by the language module and the verbal process. Translation is accomplished by less constrained central processes; morphology instead lies deeper down in the language module; and the lexicon is bifurcated into a 'linguistic' encapsulated component and a purely 'conceptual', non encapsulated, component at the interface with general knowledge. So for word-for-word translation the mind only needs to consult the language module; for sentence-to-sentence translation, it needs to use general knowledge about the appropriate use of language in context; and for explaining the meaning of a word, it needs to consult the lemma associated with it at the interface part of the lexicon. Thus Smith & Tsimpli explain C's dissociation by appealing to his intact morpholexical component, which accounts for his good knowledge of word definitions and word-for-word translations, and to his weak central controls, which explain poor sentence-to-sentence translation.

They also have to explain why C can answer questions of general

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knowledge very well but is almost incapable of carrying on a conversation. To do this, they exploit again their proposed bifurcation in the lexicon. They argue that to answer questions of general knowledge one only has to consult the information in the conceptual lexicon at the interface between the language module and central processes proper. In this interface, they assume that 'Question-answering [to simple questions] takes place largely on the basis of a short-cut strategy which bypasses those parts of the system which are defective' (171). A well-functioning conceptual lexicon suffices, whereas good functioning of the central executive is needed for engaging in successive conversation. C has the former but lacks the latter – hence his pattern of behavior.

So much for the relation between C's behavior and Smith & Tsimpli's model. The authors argue that their results call for some changes in Anderson's theory of intelligence and in Fodor's modularity thesis. Let us see why.

For Smith & Tsimpli, the modularity thesis should be modified to make room for only partially modular information processors, quasi-modules that are not informationally encapsulated (unlike the real stuff) but are nevertheless fast, efficient and somewhat constrained in their flows of information. They especially have in mind TOMM, the Theory of Mind Module. They argue that it is not a module in Fodor's (1983) sense because its vocabulary is 'derived from conceptual representations rather than from a domain-specific vocabulary' (175), and because it exploits 'central' information. They thus argue that TOMM is 'central' and nevertheless amenable to scientific study, and so conclude that, pace Fodor, some parts of the central system are open to scientific investigation (e.g. 34).

They may be right that an extension of the modularity framework is needed, but their arguments for the centrality of TOMM are not convincing. Their first reason is that TOMM exploits 'conceptual' representations. However, the distinction between 'conceptual' and 'perceptual' is far from clear, and it is even less clear how it bears on the modularity issue. If anything the way to make it clearer is precisely by appeal to modules. What makes the representation of 'red' perceptual is that redness is computed fast, almost immediately, and so on – in short, the fact that colors are treated by a module. So it is question begging to say that TOMM is not a module BECAUSE it appeals to conceptual information: this may just be another way to say that TOMM is not a module. Their second reason for the centrality of TOMM is that they feel that TOMM exploits central information. But this is just an intuition, however strong, and it may turn out to be false. In order to find out if and when TOMM exploits 'central' information, many experiments tracking the microprocess of information exchanges are needed, and Smith & Tsimpli mention none. There is nothing different in this case than in any other domain where questions of modularity arise. Consider lexical retrieval: is it modular or not? It seems so intuitive that a strongly

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biasing context determines the selection of word meanings on line. However, it's just by doing very careful experiments that the question can be settled, and the results can actually show the opposite (e.g. Swinney 1979). Smith & Tsimpli have no way to rule out the possibility that, just as in the lexical retrieval example, at a certain level of processing TOMM is closed to central information and is fully modular in Fodor's sense. In short, much more detailed evidence is needed to make a case for central quasi-modules.

As for Anderson, Smith & Tsimpli do show that his overall theory needs many changes in order to account for C, but here my reservations are of another kind. Anderson (1992) did two things. He proposed an architecture for various mental activities, but very little of it is original. He also proposed that intelligence – Spearman's factor G – should be identified with the speed of the basic (central) processing mechanism (BPM). This is the most original aspect of his theory. It is largely independent from his as well as from other possible architectures for mental processes. Now, Smith & Tsimpli make no use of it. They even suggest that the BPM might be only a 'constraint on the operation of other parts of the system rather than being a kind of module in its own right' (214), which is an elegant way to say that they don't need Anderson's theory. So, even if they are correct in many of their criticisms of Anderson, they don't use his basic proposal and one wonders why they take his work as a point of reference in the first place.

This concludes my critical presentation of Smith & Tsimpli's book. Although I consider it an excellent work, I have advanced various reservations about the interpretation of some results and the way to interpret their consequences for cognition. All my doubts have a common origin. They stem from a perceived discrepancy between the data and the strength of the theoretical apparatus used to account for them. I actually think there is an alternative interpretation of C's case. Suppose you were taken by the obsession to compulsively and unmethodically learn new languages. Where would you start from? Just like real second language learners, or like foreigners embedded in an unknown linguistic community, you would probably start by collecting lexical items and would attach to them the grammar of your language, little by little refining your strategy with the help of some rules of thumb. As a consequence, you would be good at word for word translation, but terrible at sentence translation. Also, practice would make you faster at learning new vocabulary, but not at improving other linguistic abilities, unless you decide to train them as well. Also, you would guess a morphological rule more easily than a syntactic rule, because that's where your general induction strategies can exploit the larger database. So, in most relevant linguistic respects, you would perform very much like C, with all the extra wit that your better general intelligence might add. But nothing specifically LINGUISTIC is going on, and nothing specifically revealing for the architecture of the mind.

Now two questions should be raised. In what way is C different from any

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other obsessive savant, besides the specific nature of his obsession? And what lessons could C teach us about the architecture of the mind? I think that for the first question Smith & Tsimpli have offered no compelling evidence that C is more than a savant obsessed by vocabularies. As for the second question, notice that we need not bother about what a module is, and what central intelligence is. In fact, we needn't even mention theories of intelligence or of modules, and besides a very general distinction between central system and periphery we need very little else. You may still like or dislike Fodor's or Anderson's theories as you wish; what C's case and its analysis by Smith & Tsimpli will give you is largely independent of your architectural tastes. My critical comments should not be misunderstood. Alternative ways of interpreting C are possible only thanks to Smith & Tsimpli's excellent analytic work, which allows one to see what may and may not be going on in this fascinating case.

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Carol L. Tenny, *Aspectual roles and the syntax–semantics interface*. Dordrecht: Kluwer Academic Publishers, 1994. Pp. x + 245.

Reviewed by Louisa Sadler, University of Essex

In broad terms, the central hypothesis of this book is that the interface between syntax and lexical semantics is aspectual in nature (the Aspectual Interface Hypothesis). In particular, Tenny makes three claims: that the universal linking principles projecting semantic participants to syntactic positions or roles are sensitive to the aspectual roles of entities, rather than to thematic roles, other chunks of lexical conceptual structure (LCS), or Dowty proto-role entailments; that aspectual structure is a subpart of event structure, which also contains the external part which is not relevant for mapping; and that the separation of aspectual structure from conceptual structures such as LCS is motivated by a number of linguistic phenomena. These three issues are approached in separate chapters, though there is a good deal of overlap.

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Chapter 1 focuses on linking and aspectual roles. Tenny takes the syntactic side of linking to be a syntactic argument structure which (structurally) represents a tripartite distinction between external argument, direct internal argument and indirect internal argument. Clearly, the mapping to the syntax from this representation will be more or less trivial depending on the model of syntax assumed. Her hypothesis is that only aspectual properties are involved in universal generalizations about linking – the aspectual property of delimitedness or boundedness (distinguishing accomplishments and achievements from activities and states) is key.

The book proposes three (universal, aspectual) constraints on the relationship between the syntactic argument structure and the lexical semantics, and provides definitions of three aspectual roles *MEASURE*, *PATH* and *TERMINUS*. The central constraint, the MEASURING OUT CONSTRAINT ON DIRECT INTERNAL ARGUMENTS (11), focuses on the role of the direct internal argument in delimiting the event:

i. The direct internal argument of a simple verb is constrained so that it undergoes no necessary internal motion or change, unless it is motion or change which 'measures out the event' over time (where 'measuring out' entails that the direct argument plays a particular role in delimiting the event).
ii. Direct internal arguments are the only overt arguments which can 'measure out the event'.
iii. There can be no more than one measuring-out for any event described.

The notion of measuring out an event, which involves the existence of some scale of measurement and a temporal bound (delimitedness), is perhaps most familiar in the notion incremental theme. Measuring out, as defined by Tenny, involves change along a single gradable parameter. Tenny's approach is wholly informal – she provides only a prose definition of the key aspectual roles of *MEASURE*, *PATH* and *TERMINUS* upon which her work focuses, noting however that the framework of Krifka (1992) (which deploys the notion of a homomorphism from objects to events) should in principle be extendible to support an appropriate formalization. The reader is provided with an intuitive feel for the key notion of measuring out through the presentation of a battery of tests concerning co-occurrence with adverbs like *halfway*, adverbs of comparison (*more*) and degree of completeness (*quite*), rate adverbials (*slowly*) and punctual adverbials (*in an hour*).

There are three types of measure which fall under the Measuring Out Constraint, the incremental themes (elements the creation, alteration or destruction of which measure out the event, as in (1)), arguments undergoing a change of state (2) and the path objects of route verbs (3).

(1) John built the house in three weeks.
(2) The gardener ripened the fruit.
(3) Sue walked the Appalachian Trail.

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Several interesting sections of Chapter 1 look at verb alternations from the perspective of measuring out. Tenny argues that resultatives and particles require a measuring-out reading for the direct internal argument *hammer the metal flat*, *eat the apple* (*up*). Notice that *eat* is ambiguous between the delimited and non-delimited readings, but the particle enforces the delimited reading and places the additional requirement that the entire apple provides the temporal bound to the event.

Cognate objects, so-called fake reflexives and expletive body parts provide (optionally measuring) direct internal arguments to normally intransitive verbs:

(4) John laughed a mirthless laugh.
(5) John shaved himself.
(6) I cried myself to sleep.
(7) I cried my eyes out.

The *his way* construction also measures out the event, this time by means of a *PATH* and *TERMINUS* (see below):

(8) John insulted his way across the room.

Unspecified object deletion and the conative construction both delete a measuring argument, with a concomitant aspectual change:

(9) Brian ate a pizza in/*for 5 minutes.: Brian ate *in/for 5 minutes.
(10) cut the bread: cut at the bread

Tenny argues that verb classes should be distinguished on the basis of the (presence or absence of) aspectual roles and that alternations such as those illustrated above are essentially processes affecting the aspectual grids of verbs. Thus result predication instantiates a process of aspectual grid merger, and constructions with cognate objects, expletive body parts or fake reflexives involve the optional addition of an element to the aspectual grid (110–112):

(11)

cognate object	$V \rightarrow V\ NP_{event\ of\ V\text{–}ing}$
	$[] \rightarrow [(MEASURE)]$
fake reflexives	$V \rightarrow V$ him/herself Resultative
	$[] \rightarrow [(MEASURE)]$
expletive body part	$V \rightarrow V$ his/her $NP_{body\ part}$ Resultative
	$[] \rightarrow [(MEASURE)]$

According to this view advocated in this book, all broad range rules (in the sense of Pinker 1989) make reference only to aspectual roles, while (language

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specific) narrow range rules which limit the applicability of operations to certain subclasses of verbs may be stated using non-aspectual vocabularies (for example, contact is a key notion for the English conative alternation).

The incremental theme and change of state verbs have been widely discussed in the literature, and it is arguable that the material in this Chapter adds little to our understanding of these verbs. The discussion and analysis of the path object verbs, on the other hand, is both more interesting and innovative (and more open to challenge). These verbs have both delimited and non-delimited readings, and yet they appear to have no measure:

(12) walk the trail for/in an hour
perform the sonata for/in an hour
climb the ladder for/in an hour

Tenny argues that paths should be seen essentially as measures which are defective in that they lack inherent endpoints. They do not undergo any change or motion. Because they measure out, they link as direct internal arguments. A path has an externally imposed terminus, and the sequence of aspectual roles *PATH*, *TERMINUS* is equivalent to *MEASURE*. With these verbs, the terminus can be implicit, or made explicit in a goal PP (*walk the trail to its end*), while in some cases the path object itself can be implicit, but the delimited reading is then imposed if a terminus is made explicit:

(13) John rolled the car to the garage.
John walked (the path) to school.

The TERMINUS CONSTRAINT ON INDIRECT INTERNAL ARGUMENT (68) governs the mapping to indirect internal argument positions and thus the expression of the *TERMINUS* aspectual role:

i. An indirect internal argument can only participate in aspectual structure by providing a terminus for the event described by the verb. The terminus causes the event to be delimited.
ii. If the event has a terminus, it also has a path, either implicit or overt.
iii. An event as described by a verb can have only one terminus.

This constraint singles out goals as special, for they are termini, permits the *PATH TERMINUS* combinations or bi-partite measuring-outs, and otherwise permits any number of non-delimiting indirect arguments.

The Terminus and Measuring Out constraints map the aspectual roles Tenny identifies to internal argument positions and are supplemented by the NON-MEASURING CONSTRAINT ON EXTERNAL ARGUMENTS (83):

> An external argument cannot participate in measuring out or delimiting the event described by a verb. An external argument cannot be a measure, a path or a terminus.

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In fact, the claim that aspectual roles map only to internal argument positions is not as strong a restriction on the aspect/syntax interface as it might at first seem. It does not rule out *measure* subjects, but rather requires a syntactic analysis whereby they enter the syntax as internal argument and undergo some syntactic derivation (e.g. unaccusatives and passives).

The second chapter considers the relationship between event structure and the notion of aspectual structure which has emerged in Chapter 1. Tenny locates the aspectual structure (the roles that she has identified in Chapter 1) as a component (subpart) of event structure, in the representation of some verbs (statives, for example, do not involve measuring out and thus have no aspectual structure in the intended sense), associating (perhaps equating, the discussion is rather unclear on this point) it with the notion of event nucleus (Moens & Steedman 1988). The other subpart of event structure is the external part containing the action engaged in by the agent or external argument. The representation of a path object verb would be `[ __ [PATH, TERMINUS]]`, and an unergative verb such as *run* would be `[__ [ ]]`.

The rest of the chapter is given over to justifying the external/internal structuring of event structure by presenting a number of syntactic phenomena which are sensitive to the nature of the event nucleus, and thus refer to aspectual information. These include Russian perfective verb prefixation, the distribution of accusative vs. partitive case marking in Finnish, English verb–particle combinations, English resultative predication, English passive nominals, middles and the notion of affectedness, Japanese numeral quantifiers (affectedness), and Haitian predicate clefting.

The final chapter turns to the relationship between aspectual structure (as she conceives of it) and linguistically oriented conceptual structures (such as LCS). She argues that part of LCS reduces to aspectual structure and should be factored out into a separate level of representation, because it is this (and only this) information which is relevant to linking and is amenable to precise definition. Tenny holds that a number of phenomena discussed in the literature in relation to conceptual structures should be seen as involving the interaction of conceptual and aspectual structures. It is difficult to see what really is at stake here, given that the aspectual structure that she has in mind may be straightforwardly read off and extracted from those conceptual structure representations, as she herself observes. Hence an operation may add a *MEASURE*, but the kind of measuring will depend on thematic conditions to be stated over conceptual structures. Likewise, for verbs like *put* and *place*, the property of obligatorily requiring a terminus is dependent on the degree of manner information in the verb itself – it appears to be possible to omit the *TERMINUS* only if a BY manner clause is present in the LCS.

This is an easy and interesting book to read, and in many ways an important one. The role of aspectual structure in the organization of the lexicon and in linking continues to be a topic of wide interest, and Tenny's

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book provides a clear and simple introduction to these questions. The discussion of verb alternations in English in the course of the presentation of the aspectual linking constraints provides a concise, clear and quite comprehensive overview. The material presupposes no particular theoretical standpoint, and should be easily accessible to students with a wide range of backgrounds.

There are however a number of significant omissions in the book.

There is very little discussion of verbs which do not measure out, such as statives *John likes calculus*, unergatives *Jane shouted*, non-delimiting transitives *Leslie pounded the wall* and non-delimited readings of transitives *Chris played the sonata for an hour*. The Measuring-Out Constraint on Direct Arguments is carefully formulated with such cases in mind, but the reader might wish for more discussion of these cases if she is to be convinced by the correctness of the Aspectual Interface Hypothesis as stated (note that this also denies the role of properties such as sentience and volitionality in linking): 'The universal principles of mapping between thematic structure and syntactic argument structure are governed by aspectual properties relating to measuring-out. Constraints on the aspectual properties associated with direct internal arguments, indirect internal arguments, and external arguments in syntactic structure constrains [sic] the kinds of event participants that can occupy these positions. Only the aspectual part of thematic structure is visible to the universal linking principles' (116).

The aspectual interface depends crucially on the tripartite distinction in argument structure between external, direct internal and indirect internal arguments. This leads to the exclusion of a number of important phenomena, and thus limits the interest of the approach. A case in point is resultative phrases, many of which fall outside the Terminus Constraint on Indirect Arguments, since they are APs rather than nominal arguments introduced by a prepositional predication. More generally, the linking theory is partial in dealing only with nominal arguments, and saying nothing about the linking to predicational or propositional arguments (such as, for example, the XCOMP function of LFG). There is also no treatment of the Double Object Construction, about which Tenny observes 'Double object constructions confound this tripartite distinction by having an extra argument which appears in some ways like a direct internal argument and in some ways like an indirect internal argument' (81).

In this volume, Tenny sketches out a model of the syntax/lexical semantic interface, involving Conceptual Structure, Event Structure (containing Aspectual Structure as a subpart), Argument Structure (external and internal arguments) and the syntactic structure as levels of representation. This is a complex model, and the reader should ask whether all these structures are carefully justified. At a number of points in the book, I did not feel this was the case, and particularly in Chapter 2 I would have welcomed much more extensive justification of the external–internal distinction in Event Structure

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assumed. In similar vein, I would have welcomed a section in the book spelling out in some detail precisely what this theory implies for the nature and structure of lexical knowledge, what must be specified, what is factored out in generalizations over the whole or part of the lexicon.

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Akira Watanabe, *Case Absorption and WH-Agreement* (Studies in Natural Langauge & Linguistic Theory, Volume 37). Dordrecht: Kluwer Academic Publishers, 1996. Pp. xi + 271.

Reviewed by TATSUYA SUZUKI, Nanzan University

This fine work on Arg-based Case theory in the framework of the Minimalist Program clarifies similarities between passive and causative constructions with respect to Case absorption on the one hand, and demonstrates that Case checking and *wh*-agreement are closely related to each other on the other, based on ample data from various languages.[1]

Chapter 1 is an introduction to the theory that is employed throughout the book, one of the most recent versions of generative grammar, known as the Minimalist Program (henceforth MP) (cf. Chomsky 1993, 1994). In this particular model, structural Case is explained in terms of a Spec-head relation in AgrP. Thus, first of all, readers must become familiar with such Agr-based Case theory, dispensing with the government-based Case theory that was employed in government and binding (GB) theory, which is the immediate predecessor of the current one.

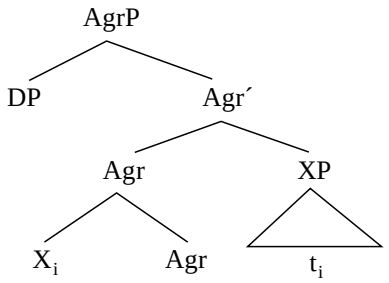
[1] This review is partly supported by Nanzan University 1997 Pache grant I-A for promoting research.

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It may be said that MP has entered another phase with the introduction of the Attract F theory of Chomsky (1995, Chapter 4), which eliminates Agr projections entirely from the theory. But it would be an error to have any prejudice against the Agr-based Case theory elaborated in this study simply because of that. This is because, as the discussion in Chapters 3 and 4 especially suggests, are Agr-based Case theory may indeed provide more principled accounts of various constructions than the Agr-less Case theory.

One of the crucial proposals that the author makes in this study is that Agr is a Case-absorbing head. It can absorb a Case feature either from DP or from a Case bearing head. In his system, the Case feature of DP in the Spec of AgrP is transferred to AGR, when it matches with the Case feature of the Case bearing head X in (1), which is adjoined to Agr. Then it becomes invisible together with Agr when the Agr becomes invisible at LF.

(1)



(Watanabe's (1.16))

Based on the assumption that Agr can take or absorb only one Case feature, Watanabe assumes that the Case feature of a Case bearing head remains without being transferred to Agr. If nothing eliminates this Case feature, the derivation crashes by definition because all Case features are required to be eliminated by the end of derivations. (Chomsky (1995: chapter 4) makes this point somewhat clearer by saying that Case features are [−interpretable], and [−interpretable] features must be eliminated for convergence.) The mechanism to save the derivation is the process of follow-up checking. He proposes that an immediately higher functional head will check the outstanding Case feature of the Case bearing head. This particular version of Case theory is called the THREE-LAYERED CASE CHECKING HYPOTHESIS. This is the heart of the study in the book.

Suppose that there is no follow-up checker of Case. This is instantiated in the case of passives. Consider the example (2):

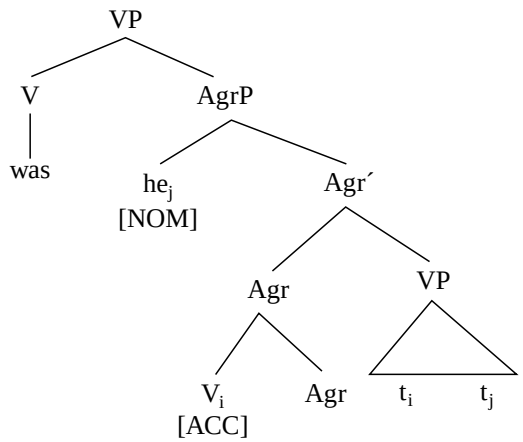
(2) (a) He was scolded.
(b) …[$_{VP}$ was [$_{AgrP}$ Agr [$_{VP}$ … scolded he …]]]
[ACC] [NOM]

When the internal argument is raised to the Spec of participial AgrP on its

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way to the Spec of Agr-sP, the verb is adjoined to the participial Agr, as in (3).

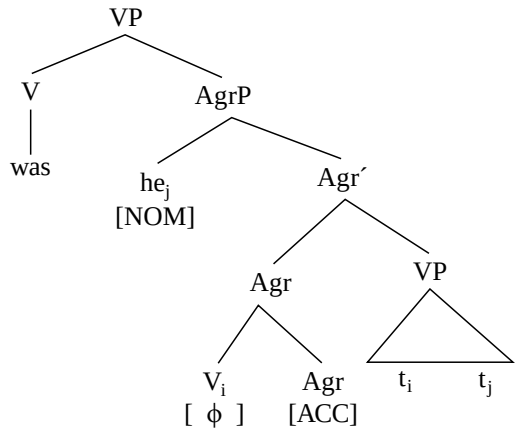
(3)



(Watanabe's (1.22a))

Notice that there is no follow-up checker for the accusative Case feature in (3) because of the poverty of the clausal projection of the embedded clause. But if the accusative Case feature is transferred from V to Agr, as in (4), it may become invisible when the Agr becomes invisible at LF.

(4)



(Watanabe's (1.22b))

This is the process of Case absorption in Watanabe's theory. In this theory, as he states (14), it can be said that both Case checking and Case absorption are essentially the same process with respect to Agr in that Case features are transferred to the appropriate Agr. They then become invisible together with the Agr when it becomes invisible at LF.

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The mechanism of the Case feature transfer may be supported by the facts about clitic doubling in northern Italian dialects, Fiorentino and Trentino. In the author's view, the subject clitic (indicated as SCL in the examples below) is a realization of the phonetic feature transferred from the DP to Agr-s. The case of Trentino is interesting, for it does not allow subject clitic doubling when the subject appears postverbally, as exemplified in (6).

(5) El Mario el parla.
the Mario SCL speaks
'Mario speaks.'
(b) La Maria la parla.
the Maria SCL speaks
'Maria speaks.' (Watanabe's (1.25))

(6) (a) Ha telephoned qualche putela.
has telephoned some girls
'Some girls have telephoned.'
(b) *L'ha telefoná qualche putela.
SCL-has telephoned some girls (Watanabe's (1.27))

When the subject is placed postverbally, no DP occupies the Spec of Agr-sP, thus there is no Case feature transfer. Although the Case of the postverbal subject DP is supposed to be checked at LF, that process should not affect PF. Therefore, there is no subject clitic when the subject is placed postverbally in Trentino.

The three Layered Case Checking Hypothesis necessitates the C^0 projection as a follow-up checker. This means that a full clausal structure should be something like (7):

(7) $[_{CP}$ C $[_{Agr\text{-}sP}$ Agr-s $[_{TP}$ Tns …

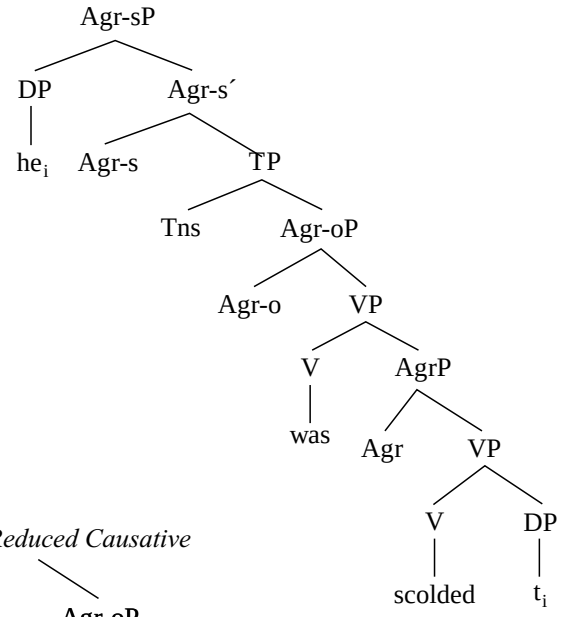
The author concludes, in terms of the distribution of PRO, that ECM and raising constructions have clausal structures that lack a C^0 projection. Both ECM and raising constructions do not allow PRO subjects. In MP, PRO is licensed by Null Case. But neither ECM nor raising constructions can have Null Case. Note here that if the Tense of those constructions had Null Case, it could not be checked off because it has no follow-up checker.

Chapter 2 is devoted to the defence of the clausal structure that the author proposes, namely (7). He presents persuasive data from various languages, and confirms the validity of the view that C^0 functions as the follow-up checker of Case that is carried by Agr as a result of the adjunction of Tense to Agr.

One of the highlights of the first half of this study is the analysis of passive and causative constructions in terms of Case absorption. The author proposes that passive and causative constructions have the following structures respectively.

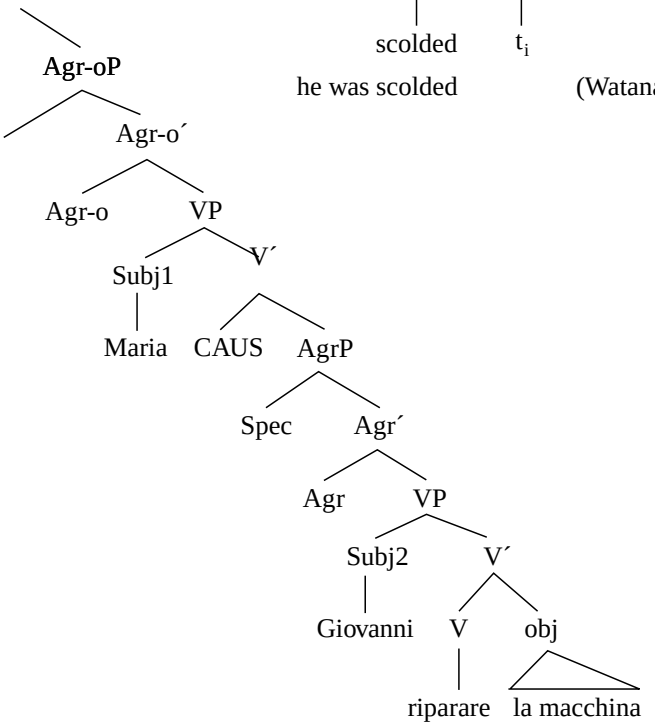
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(8) *Passive*



he was scolded (Watanabe's (3.4))

(9) *Reduced Causative*



Maria ha fatto riparare la macchina a Giovanni.
Maria has made repair the car to Giovanni
'Maria made Giovanni repair the car.'

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These two constructions share an important property with respect to Case. This is the process of Case absorption, in which the accusative Case of the embedded verb is transferred to the Agr that dominates it when the verb is adjoined to the Agr.

In the case of passive, the accusative Case feature of the participial verb cannot be checked off because there is no T head that serves as a follow-up checker for it in this construction. (It is also assumed that *be* cannot check off the accusative Case feature of the participial verb.) The Case feature of the internal argument is checked off at a higher position, namely at the Agr-sP domain, but the accusative Case feature of the participial verb must find another way of being checked, because of the lack of the follow-up checker, in order to be eliminated for convergence. The process will be Case absorption. The accusative Case feature is transferred to Agr when the participial verb is adjoined to the Agr that dominates the verb.

Likewise, in the case of the Causative, the accusative Case feature of the embedded verb can not be checked off unless it utilizes Case absorption by Agr. The Causative verb cannot function as a follow-up checker because only functional heads are assumed to be qualified for that purpose. The accusative Case of the internal argument is checked at the Spec of the embedded Agr-oP when the embedded verb is raised to the embedded Agr-o, but the Case feature of the verb itself still remains unchecked because of the lack of a follow-up checker. As a result, Case absorption by Agr will save the derivation, just as in the case of the passive.

Chapter 4 presents extensive discussion of A-bar movement. This chapter deals with interactions between Case checking and A-bar movement. First the author demonstrates that the Case checking system in his theory correctly describes a property of *wh*-agreement like (10). This is the *wh*-agreement pattern of Palauan, a Western Austronesian language.

(10) (a) When the local-subject is extracted, the verb retains realis morphology but loses subject agreement.
(b) When something other than the local-subject is extracted, the verb takes irrealis morphology, retaining subject agreement.
(Watanabe's (4.6))

Since C, Agr-s and Tense are all involved in Nominative Case checking in the Three-Layered Case Checking Hypothesis, it would not be surprising at all that *wh*-agreement distinguishes between *wh*-extraction of subjects and that of non-subjects.

Next, the author attempts to provide a unified account for what is known as *wh*-agreement phenomena, which may otherwise be considered to be merely a broad term that covers various kinds of separate phenomena that relate *wh*-movement and special morphology. His conclusion is surprisingly, but adequately, simple: *Wh*-agreement phenomena are reduced to morphological choices that are allowed by Universal Grammar.

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The following is an example of *wh*-agreement from Hausa:

(11) Mee suka/*sun cee yaaraa sun sayaa?
what 3PL-IR-COMPL/3PL-R-COMPL say children 3PL-R-COMPL BUY
'What did they say the children bought?' (Watanabe's (4.27))
(IR = irrealis, R = realis, COMPL = complementizer)

Verbs take the irrealis form when there is a *wh*-movement in this language. Although the above example suggests that only the operator, not intermediate traces, induces *wh*-agreement, there is in fact a dialect in which intermediate traces also require a verb to take the irrealis form.

(12) Mee suka/*sun cee yaaraa suka sayaa?
what 3PL-IR-COMPL/3PL-R-COMPL say children 3PL-IR-COMPL buy
'What did they say the children bought?' (Watanabe's (4.28))

From the author's point of view, this suggests that dialects may vary with respect to the morphological realization of *wh*-agreement in that the form of *wh*-agreement induced by operators and that caused by intermediate traces may differ. This leads to the idea which the author advocates that *wh*-agreement phenomena are reduced to morphological choices that are allowed by Universal Grammar. In other words, the form of *wh*-agreement induced by intermediate traces may happen to be the same as the one that is found in the case without *wh*-agreement.

The arbitrary nature of morphological realization of *wh*-agreement is highlighted, for example, by analyzing the *that*-trace effect of English from the perspective of the theory of *wh*-agreement. it is well-known that there is a subject/non-subject asymmetry with respect to *wh*-extraction in English, as exemplified in (13).

(13) (a) Who do you think [ϕ/*that [*t* solved the problem]]?
(b) Which problem do you think [ϕ/that [he solved *t*]]?
(Watanabe's (4.29))

However, a different pattern manifests itself when the *wh*-movement occurs in relative clauses, as shown in (14).

(14) (a) the guy [$_{CP}$ *ϕ/that [*t* solved the problem]]
(b) the guy [$_{CP}$ ϕ/that [everybody believes [$_{CP}$ ϕ/*that [*t* solved the problem]]] (Watanabe's (4.34))

Various kinds of government-based analyses were proposed in GB theory, namely the accounts based on the Empty Category Principle. From the perspective of *wh*-agreement, however, the *that*-trace effect is merely reduced to the thesis that morphological realization of *wh*-agreement distinguishes

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between intermediate traces and heads of *wh*-chains. This seems to be a significant contribution to the theory of grammar in that the *that*-trace effect finds its natural place in the grammar among other theoretical issues to be explained in formal syntax.

Throughout the book, the author never bores the reader. His attempt to analyze all of the French stylistic inversion, Japanese nominative/genitive conversion, and Chamorro *wh*-agreement in terms of *wh*-agreement is quite challenging and intriguing. This study successfully demonstrates the significance of Agr-based Case theory and *wh*-agreement in many languages. It then raises the non-trivial question of how the insights that were made available in the Agr-based Case theory can be incorporated into an Agr-less theory like Chomsky's (1995), or more fundamentally, whether or not they simply pose challenges to it. There is no doubt that this book has won a firm status in the essential literature for the study of Case absorption and *wh*-agreement.

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Anna Wierzbicka, *Semantics: primes and universals*. Oxford: Oxford University Press, 1996. Pp. xii + 500.

Reviewed by WILLIAM FRAWLEY, University of Delaware

This book is the latest installment in Anna Wierzbicka's semantic research program, which involves two overall lines of argument: analysis based on a limited set of semantic primitives; demonstration of the role of such analysis in grammatical description. The book is comprised of revised, previously published papers, so it reads less as a single sustained argument than a collection of representative analyses. Consequently, going through the book from beginning to end is best appreciated by the initiated and dedicated. Still, a selective reading of the chapters based on need and interest is well worth it, especially if the need and interest are comparative semantic data, where Wierzbicka is at her best.

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Part 1, 'General issues', contains seven chapters on the methodological and metatheoretical aspects of empirical, comparative, conceptual semantic analysis. The introduction, Chapter 1, presents the case for primes that capture both semantic universality and relativism: invariants in language-specific configurations (to paraphrase the subtitle of her previous book, Wierzbicka 1992). Two important points emerge from the introduction: primes can be identified systematically (e.g. they are not related derivationally: *I* is not the same as ∼ *YOU*, so both are primes), and the metalanguage for semantic description is simple and straightforward (her Natural Semantic Metalanguage).

These points are made in the midst of heavy bashing of Chomskyan linguistics and modularity. At certain points, these attacks strike me as unfair – in my view, she misstates Chomsky's position on semantics in the role of grammar, and she cites Edelman's dismissal of modularity but none of the massive evidence for it. At other points, they are unexpected – she ultimately sides with categorical concepts and against a Lakoff-style semantics. In the end, I think they are even unnecessary because you can hold her position without having to be anti-Chomskyan or anti-modular.

The next two chapters respectively inventory the semantic primitives and describe their rules of combination and expression in the metalanguage. The original set of fourteen primes (Wierzbicka 1972) now numbers fifty-five, a large but not unreasonable figure. There are some obvious atoms – SEE, SAY, BIG, WHERE – and some surprises – LIKE, WORD and PEOPLE. These go together in sentence-like formulas to capture the semantic essence of a lexical form.

What this analysis shows is the difference between semantic-conceptual analysis and cognitive analysis. Cognition overdetermines meaning. For example, Spelke (1994) claims that initial cognitive knowledge must include a representation of the boundedness and internal coherence of an object if we are to explain infants' early spatial cognition. But for Wierzbicka (unlike Jackendoff 1990 on this point) semantic structure can be adequately captured by a primitive like SOMETHING, without finer encoding. Insofar as semantic primitives are a kind of initial semantic knowledge, they appear to be overdetermined by initial cognitive knowledge. It would be interesting to compare Wierzbicka's list of fifty-five primitives with other partial inventories of a priori knowledge.

Chapter 4 ('Prototypes and invariants') and Chapter 5 ('Semantic primitives and semantic fields') cover two standard issues in lexical analysis. The former chapter is an articulate defense of deterministic category membership and a convincing counter to the adoption of prototypes in semantic analysis. The latter is a study of the definition of natural kinds, cultural kinds, speech act verbs, and emotions.

Chapter 4 deserves to be read by the linguistics community at large since it takes on one of the fundamental issues dividing schools of thought.

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Wierzbicka shows how fuzzy category effects do not demand intrinsically fuzzy criteria for category membership. There can be many causes of gradient behavior, such as the interaction of otherwise deterministic semantic components. Perhaps even more important, she shows that the appeal to intrinsically gradient categories often masks a failure to follow through on analysis. If people cannot agree that a lilac is a kind of tree, then this does not require that *lilac* be assigned probabilistic status for 'tree'. As Armstrong, Gleitman & Gleitman (1983) nicely showed in their classic paper, prototypes can be processing effects: people claim that 3 is a 'better' prime number than 91! Moreover, prototypes are not incompatible with invariant semantic category membership as long as the proper analysis is done: a definition of *apple* that includes reference to 'red', 'green', and 'yellow' can produce the prototype effects of the classic red apple.

Chapter 6 ('Semantics and "primitive thought"') holds that there is no primitive thought by showing how languages in traditional societies embody universals. Chapter 7 ('Semantic complexity and the role of ostension in concept acquisition') gives the case against ostension in the development of semantic categories (though I wonder if Wierzbicka is really against NOMINALISM, not ostension). These papers might very well appear to linguists to be scoring points in academic debates already settled, probably because they were originally written for other audiences.

Part 2, 'Lexical semantics', contains five papers on word meaning and lexicography. Chapters 8 ('Against "against definitions"') and 9 ('Semantics and lexicography') show that her metalanguage is an accurate and complete defining language. Wierzbicka makes this compelling point with some severe criticism of existing dictionaries and further attacks on the Chomskyan school. As to the former, she shows how judicious use of primitives and persistent lexical analysis can produce simple, yet complete, definitions of words often incompletely defined in current dictionaries. Still, the lexicographers would reply that they are not out for comprehensiveness but accuracy within a defining tradition (thanks to Enid Pearsons for this observation). As to the latter, she criticizes Fodor for arguing against definitional approaches to concepts. However, my sense of Fodor's position is that he is not talking about dictionaries at all, but the technical philosophical notion of the semantics of mental predicates, which he does say cannot be defined componentially. I think both Fodor and Chomsky would in fact agree with Wierzbicka that dictionaries, presently constituted, are really just collections of lexical hints and could be improved.

The last three chapters of the section cover color terms (Chapter 10), where Wierzbicka argues for a conceptual, not perceptual or neurophysiological, analysis of color semantics; natural kinds (Chapter 11), where she defends the dictionary as 'harder fact' than the encyclopedia; and ethnobiological categories (Chapter 12), where she presents a more realistic view of linguistic taxa for flora and fauna (e.g. 'plant' is not a category because it has no

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named members). The work on color is especially good. Wierzbicka reviews in detail the ongoing debate on Berlin and Kay's original work, essentially coming down against the received view. With lots of comparative data, she shows that the conceptual-semantic analysis of color must capture the ambient meaning of the terms: e.g. green means 'grass-like'.

In Part 3, 'The semantics of grammar', are Chapter 13, 'Semantic rules in grammar', 14, 'Transitivity and reflexives', and 15, 'The semantics of evidentials'. This work pushes an old and controversial point in Wierzbicka's work – syntax reflects meaning or, better, even morphosyntactic oddities have quite regular semantic correlates. These claims have always left me on the fence. Sometimes, they seem to be a 'just so story'. Polish *usta* 'mouth' is morphologically plural because in Polish, the conceptualization is plural (*mouth* = 'two-lip thing'), whereas in English it is singular (386). I find this hard to accept.

However, at other times, the arguments are strangely compelling, especially because of the wide range of data and cultural-conceptual analysis. In Chapter 14, she claims that all grammatical reflexives are manifestations of semantic sameness: 'something happened to the same person' (422). So even in odd reflexives (like middles), you can find a conceptualization of semantic sameness. I thought this hard to swallow when I considered the small number of languages that use the reflexive in the antipassive (e.g. Australian languages, Lithuanian and Eskimo). Then I realized that the reflexive-antipassive often has the effect of stativizing the predication into a property of the subject (Lithuanian, from Lidz 1996: 79):

(1) petr- as svaido-si akmen-imis
Peter NOM throw REFL stone INSTR/PL
'Peter throws stones'.
(i.e., 'as a rule, stone throwing is a Peter-thing')

Clearly the predication and Peter are 'the same', in some sense. So once again, I go from assuming a 'just so story', where either the sameness argument has to be stipulated for these cases or these forms have to be treated as non-reflexive reflexives, to being convinced.

So I end on this happy confusion. This is a good and interesting book, frustrating in parts but equally strong on data. Either way, it is well worth reading.

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