

REVIEWS

J. Linguistics 42 (2006). doi:10.1017/S0022226706214099
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Robert D. Borsley & Bob Morris Jones, *Welsh negation and grammatical theory*. Cardiff: University of Wales Press, 2005. Pp. xiv + 279.

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As the title indicates, Robert D. Borsley & Bob Morris Jones (henceforth B&J) address the expression of negation in Welsh. They use grammatical theory to develop an empirical overview of Welsh negation, paying special attention to colloquial/informal language, and in turn explore the implications of the Welsh data for grammatical theory. The book is intended both for readers with an interest in Welsh, but with little knowledge of grammatical theory, and for specialists in grammatical theory with little or no knowledge of Welsh. Thus, B&J commit themselves to present their findings in a way that is accessible to both target audiences. This is a difficult task, and B&J deserve great credit both in the linguistic and in the Welsh community for achieving their goals in such an outstanding way.

The book consists of eleven chapters, followed by an appendix (which contains the questionnaire used to elicit the data), notes, references and an index of names and subjects. The first chapter, entitled ‘Preliminaries’, sketches a brief history of Welsh and discusses different varieties of Welsh. B&J establish a distinction between formal and informal (or literary and colloquial) Welsh, and describe the regional dialects of Welsh. The following five chapters focus on the main features of Welsh negation, including negative verbs, negative dependents, negative adverbs/quantifiers, and other forms of negation as they occur in finite and non-finite clauses. The authors sketch the general patterns without ignoring dialectal variation in informal speech, which involves phonological as well as morphological and syntactic features. The second half of the book (chapters 7–10) is more theoretical in nature. It provides a formal analysis of the Welsh data in the framework of Head-driven Phrase Structure Grammar and compares it with approaches using the Principles and Parameters framework. The text of the book concludes with discussion of some sociolinguistic and diachronic issues.

Welsh is a Verb–Subject–Object (VSO) language; that is, in finite sentences the verb precedes both the subject and the object. Non-finite clauses, on the other hand, have Subject–Verb word order. The patterns of negation in finite and non-finite clauses are quite different, particularly in informal

Welsh. In formal Welsh, the main expression of negation is a pre-verbal particle, which is obligatorily present in all negative sentences. This particle surfaces as *ni(d)* (in main clauses), *na(d)* (in subordinate clauses) or *oni* (in interrogative clauses). Informal Welsh does not have the preverbal negative particle *ni(d)*, although some verbs appear with an initial *d-* or *t-* which can be seen as a remnant of *ni(d)*. The examples in (1) and (2) illustrate negation in formal and informal Welsh main clauses, respectively.

(1) *Formal Welsh*

Nid oedd Sioned yn gweithio. ((2a), 19)
 NEG be.IMPF.3SG Sioned PROG work
 ‘Sioned was not working.’

(2) *Informal Welsh*

Doedd Gwyn ddim yn cysgu. ((36b), 26)
 NEG.be.IMPF.3SG Gwyn NEG PROG sleep
 ‘Gwyn was not sleeping.’

In (1), the preverbal particle *nid* negates the entire clause. In (2), negation has been incorporated into the verb *doedd* ‘be’ (cf. *oedd* in (1)), but the negative form of the verb is not strong enough to express semantic negation. Rather, the negative force of the sentence is carried by the postverbal particle *ddim*. While *ddim* can also be optionally added to (1), its presence in (2) is obligatory to make this a negative sentence in informal Welsh. Accordingly, both formal and informal Welsh show negative concord; that is to say, multiple expressions of negation agree to express a single semantic negation. Negative concord is also found with negative dependents, as illustrated in (3)–(5).

(3) *Formal Welsh*

Ni soniodd neb am y digwyddiad. ((16), 22)
 NEG mention.PAST.3SG no one about the event
 ‘No one mentioned the event.’

(4) *Informal Welsh*

Does neb yn yr ardd. ((45), 29)
 NEG.be.PRES.3SG no one in the garden
 ‘No one is in the garden.’

(5) *Informal Welsh*

Na’th Emrys ddim gweld dim byd. ((89c), 38)
 do.PAST.3SG Emrys NEG see NEG world
 ‘Emrys didn’t see anything.’

Unlike the verb in (4), the verb in (5) does not have a form that incorporates negation (the initial *na* in *na’th* is not a morpheme and has nothing to do with the negative particle *na(d)*). The development from obligatory preverbal negation (as in (3)) to incorporated negation that does not express semantic negation (as in (4)) and, finally, to verb forms that do not show any features of negation (as in (5)) marks an ongoing change in the system that is visible in

a range of varieties and dialects of Welsh. The patterns observed exemplify well-known diachronic changes in the expression of negation, commonly referred to as the Jespersen cycle (chapter 11).

In non-finite clauses, both formal and informal Welsh use the negative verb *peidio*:

- (6) Dw i 'n disgwyl [i Mair beidio â mynd i Aberystwyth].
 ((61b), 32)
 be.PRES.ISG I PROG expect [to Mair NEG with go to Aberystwyth]
 'I expect Mair not to go to Aberystwyth.'
- (7) Dw i 'n disgwyl [i Mair beidio â gweld neb].
 ((68b), 34)
 be.PRES.ISG I PROG expect [to Mair NEG with see no one]
 'I expect Mair not to see anyone.'
- (8) Cheisiodd neb [beidio ag ateb y cwestiwn]. ((73), 35)
 try.PAST.3SG no one [NEG with answer the question]
 'No one tried not to answer the question.'

Peidio can express negation all by itself (as in (6)), but it is also compatible with a negative dependent like *neb* 'no one' (as in (7)). The negative dependent needs to follow *peidio*, that is, it needs to be situated within the predicate headed by *peidio*. If *peidio* is preceded by the negative dependent, as in (8), a double negation reading arises. Thus, the contrast between (7) and (8) illustrates that negative concord in Welsh is clause-bound.

In a few cases, we find 'extra strong' verbs, which are compatible with negative indefinites like *neb* 'no one' or *byth* 'never', but not with the negator *ddim*. An example of such a verb is the imperative form *paid* of *peidio*.

- (9) Paid byth/ *ddim â mynd i Aberystwyth.
 ((79) & (80), 63)
 NEG.IMPV.SG never/*NEG with go to Aberystwyth
 'Don't go/Don't ever go to Aberystwyth.'

The fact that, in informal Welsh, extra strong verbs like *paid* (see (9)) co-exist with strong verbs like *peidio* (see (6)–(8)), as well as with weak negative verbs like *does* (see (4)) and forms that look like positive verbs (*na'th* in (5)), implies that the situation of negative verbs is not only complex but also has an important lexical component. In chapter 3, 'Negative verbs', B&J propose the so-called Negative Dependent Constraint, which requires weak negative verbs like *does* to have a negative dependent. The negative dependent may be a post-subject adverb, a subject or a complement of the verb. The forms that cannot be distinguished from positive verbs (such as *na'th* in (5)) are treated as non-distinctive weak negative verbs, which are subject to the Negative Dependent Constraint when they occur in negative environments. Strong and extra strong negative verbs come with their own constraints.

In chapter 4, ‘Negative dependents’, B&J discuss the status of elements like *neb* ‘no one’, *byth* ‘never’ and *dim byd* ‘nothing’. When multiple negative dependents are combined in a single clause, the sentence can have either a single or a double negation reading, as illustrated in (10).

(10) *Informal Welsh*

Dw	i	erioed	wedi	gweld	neb	yma.	((24f), 75)
be.PRES.ISG	I	never	PERF	see	no one	here	
	‘I have never seen anyone here.’					(single negation)	
	‘I have never seen no one here.’					(double negation)	

The ambiguity in (10) provides a challenge for any theory of double negation and negative concord, because it makes it difficult to determine whether expressions like *erioed* ‘never’ and *neb* ‘no one’ are inherently negative (as suggested by the double-negation reading) or constitute a special kind of negative polarity item (which is a possible analysis of the single-negation reading; see, for example, Ladusaw 1992). We also find positive uses of negative dependents, as shown in (11).

(11) Adawodd	Sioned	cyn	gweld	neb.	((68), 85)
leave.PAST.3SG	Sioned	before	see	no one	
	‘Sioned left before seeing anyone.’				

These observations are reminiscent of the data regarding negative dependents in other negative concord languages (such as, for example, Spanish, French, Polish and Greek). In order to establish the status of expressions like *neb* ‘no one’, *dim byd* ‘nothing’ and *erioed* ‘never’, B&J compare them to the forms of the negative polarity item *unrhyw*, which is similar to English *any*. Thus, the sentence in (12) corresponds to (5) above, but includes *unrhyw* rather than the negative dependent *dim byd* ‘nothing’.

(12) Na’t’h	Emrys	ddim	gweld	unrhyw	un.	((101), 90)
do.PAST.3SG	Emrys	NEG	see	any	one	
	‘Emrys didn’t see anyone.’					

Differences between negative dependents and negative polarity items occur in two contexts. First, negative dependents, unlike negative polarity items, can be used as elliptical negative answers to a question, as illustrated in (13).

(13) Pwy	welst	ti?	((9)–(14), 72f.)
who	see.PAST.2SG	you.SG?	
	‘Who did you see?’		
	Neb/*Unrhyw un.		
	‘No one./Anyone.’		

Second, negative polarity items cannot occur with weak negative verbs like *does* ‘be’. If *neb* ‘no one’ in example (4) is replaced with *unrhyw un* ‘anyone’, the resulting sentence in (14) is ungrammatical, because *does* is a weak

negative form that cannot express semantic negation in the absence of a negative dependent.

(14) *Does unrhyw un yn yr ardd. ((97), 90)
 NEG.be.PRES.3SG anyone in the garden

While this is obviously a very incomplete account of B&J's presentation, as it fails to address their observations in chapters 5 ('Negative adverbs and negative quantifiers') and 6 ('Other forms of negation'), the few data presented here already show that Welsh negation is at least as complicated as negation in a number of other languages for which this phenomenon has been studied. One of the important contributions of B&J's book is that it provides access to the Welsh data, so that other linguists can test their analyses of negation, polarity and concord against the complexities of Welsh. However, B&J go a step further, and provide their own account.

In chapters 8 ('Formal analyses 1: basic elements') and 9 ('Formal analyses 2: further matters'), B&J develop an analysis of Welsh negation in the framework of Head-driven Phrase Structure Grammar (HPSG). The Welsh data support a distinction between negative heads (which are mostly verbs, but also the preposition/aspect marker *heb*) and negative dependents. HPSG is a lexical theory that allows the distinction between weak, strong and extra strong negation to be encoded in a polarity feature on the head. Furthermore, HPSG is head-driven, in the sense that heads contain information about the non-heads with which they combine. This allows B&J to implement both the Negative Dependent Constraint and constraints on strong and extra strong verbs in terms of the relation between the polarity feature on the head and the negation feature of the dependent.

For the analysis of nominals and adverbials in negative concord structures, known as n-words (see Laka 1990), B&J adopt an ambiguity view. The negative elliptic answers (e.g. (13) above) and the contrast between n-words and negative polarity items in sentences involving weak negative verbs, as in (4) versus (14) above, support the view that negative dependents in Welsh are inherently negative. Here, B&J extend de Swart & Sag's (2002) analysis in terms of n-storage and polyadic quantification to Welsh. This derives both the single- and the double-negation reading of sentences like (10) above. It also accounts for the clause-boundedness of negative concord, illustrated in (7) and (8) above, because the resumption of n-words that leads to single-negation readings is always local.

In chapter 10, 'Principles and Parameters approaches', B&J argue that Principles and Parameters theory cannot offer a satisfactory account of the Welsh data. It remains to be seen whether their criticism extends to approaches within the Minimalist framework. The proposal developed by Zeijlstra (2004) was obviously too recent for B&J to discuss, and Zeijlstra did not have access to B&J's Welsh data, so this could be an interesting issue to explore. However, it seems likely that at least some of B&J's criticisms of the

Principles & Parameters approach carry over to its Minimalist successor, so that the advantages of the HPSG analysis would remain intact.

In sum, I consider this book to be a highly successful combination of fine-grained empirical research on a particular language and theoretical analysis in a formal linguistic framework. One can disagree with details of the account, add more data or develop extensions of the analysis. In any case, this book will have a strong impact on the development of future accounts of negation in natural language, and is likely to encourage more research on the grammar of Welsh.

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(Received 27 January 2006)

J. Linguistics 42 (2006). doi:10.1017/S002226706224095
© 2006 Cambridge University Press

Guglielmo Cinque & Richard S. Kayne (eds.), *The Oxford handbook of comparative syntax*. Oxford: Oxford University Press, 2005. Pp. xii + 977.

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Generative linguistics has long had a bad reputation of being narrowly focused on English. Indeed, some functionalist linguists refer to themselves as ‘evidence-based linguists’ (a definition that is meant to play on this stereotype), and many functionalists claim to be the only linguists who study language variation. While I think that generative grammar never really had the Anglocentric bias attributed to it, it is certainly the case that since the mid-1980s and the advent of parametric research, comparative work has become absolutely central to Chomskyan syntactic theory. One cannot seriously attempt to define the range of parameters without doing significant cross-linguistic work, nor can one refine the parameters without doing microcomparative work between dialects. The volume under review presents a broad spectrum of work that focuses on comparative syntax from a generative perspective. Although grounded in recent generative theories of

syntax, it will be of value to linguists who do not work within the Minimalist framework. There is a rich tapestry of data in this large book that will undoubtedly be of interest to typologists, descriptive linguists, and syntacticians who work in other frameworks.

The book begins with three chapters that, in addition to reporting specific research findings, address both methodological and ‘big picture’ issues. The remainder of the book consists of articles organized alphabetically by the author’s last name. In this review, I will first give a brief summary and critique of the individual articles, grouping the articles thematically rather than alphabetically. I will then offer some short remarks on the structure of this book and books of its kind.

The first article in the book, Richard Kayne’s ‘Some notes on comparative syntax, with special reference to English and French’, is an excellent overview of the conceptual, methodological and theoretical issues that arise when addressing the question of why comparative research is so important within the generative parametric perspective. From now on I will assign the first part of this article to all my graduate students because I think it should be required reading for anyone doing comparative syntax. Kayne argues for a view where parameters are associated with particular lexical entries for functional categories. He observes that both macrocomparative work (which involves comparing divergent languages) and microcomparative work (which involves comparing closely related languages) are crucial to establishing comparative syntax as true science. Macrocomparative work will shed light on the wide range of possible variation, and microcomparative work will provide us with an understanding of what is varying and what types of variation are linked together.

While the distinction between macro- and microcomparative research is a fruitful descriptive notion, the heart of the chapter is Kayne’s conjecture that all variation is ultimately controlled by microparameters, each associated with a particular functional category, and his further suggestion that perhaps EVERY functional element is the locus of some distinct parametric variation. Behind this proposal is the claim that much parametric variation has to do with the pronunciation or non-pronunciation of a wide variety of functional items. The rest of Kayne’s chapter is devoted to examining a rich set of differences between French and English in precisely these terms. Starting with the observation that French, but not English, has a nominalizing morpheme (*-aine*) that attaches to numerals, Kayne shows how a variety of word order and case phenomena in English can be explained if we assume that English has an abstract, unpronounced *-AINE*. He then catalogues a number of unpronounced functional categories in the two languages and ties these to differences in syntactic movement, which adds a rich empirical bonus to a chapter that begins with important metatheoretical considerations.

If language variation reduces to differences in parameterization, it then follows that we can investigate these mental parameters from the perspective

of language acquisition and language disorders that affect the parameters. These topics are addressed in chapter 2 (Luigi Rizzi's 'On the grammatical basis of language development: a case study') and chapter 3 (Arhonto Terzi's 'Comparative syntax and language disorders'), which come to surprisingly different conclusions.

Rizzi's contribution discusses the possibility that child language consistently exhibits patterns that are attested in some adult language. He starts with the observation that the most robust parameters (such as headedness) appear to be set very early and don't exhibit maturational effects, which led to Wexler's (1998) Very Early Parameter Setting (VEPS) hypothesis, i.e. the idea that parameter setting occurs prior to the stage of multiword production. One notable exception to the VEPS hypothesis appears to be the dropping of subjects, which is present in the acquisition of both null subject languages (NSLs) and non-NSLs, and apparently needs to be 'unlearned' for non-NSLs such as English. Rizzi dismisses a processing account of this phenomenon, and instead suggests that subject omission in non-NSLs reduces to two distinct kinds of null subjects: (i) subject topics in the left periphery of the clause; and (ii) subjects of (root) infinitives. These are distinct from the subject omissions found in rich-morphology NSLs. While topic drop involves grammatical options that are also available in adult non-NSLs (cf. topic drop in English diary registers and Chinese pro-drop), subjects of root infinitives disappear when root infinitives disappear, i.e. the maturational effects of subject omission in languages like English do not correspond to a parameter setting, but reduce to the unavailability of root infinitives in later stages of child language. Thus, the overlying theme of Rizzi's paper is that child language serves as yet another 'dialect' for the purposes of investigating parameterization in Universal Grammar.

Terzi comes to the opposite conclusion in an article whose empirical domain is the positioning of clitics in the language of Cypriot Greek speakers with Specific Language Impairment. She claims that disordered speech does not, in general, correspond to an adult pattern, as it may involve significant divergences from Universal Grammar and is not necessarily similar to ANY normal language form. This conclusion is particularly striking given that Terzi assumes that Specific Language Impairment is in essence a severe developmental delay in parameter setting. Nevertheless, she notes the important role that delayed language can play in investigating phenomena that develop very quickly in early normal language.

Clitics are also discussed in the chapters by Paola Benincà & Cecilia Poletto, Jamal Ouhalla, and Eduardo Raposo & Juan Uriagereka. In 'Clitic placement, grammaticization, and reanalysis in Berber', Ouhalla discusses variation in the patterns of cliticization across different dialects of Berber, observing that this variation follows from the degree of grammaticization towards a functional category that a lexical item has undergone. Ouhalla argues that postverbal clitics are derived by left adjunction of the clitic to

some abstract functional head, followed by subsequent left adjunction of the verb. Word orders where some functional category *F* precedes a clitic in preverbal position ($F = CL V$) are the result of left adjunction to the *F*-head, followed by subsequent inversion of *F* and *CL*. Raposo & Uriagereka ('Clitic placement in Western Iberian: a Minimalist view') compare language varieties spoken on the western Iberian peninsula (mainly Portuguese and Galician), both with each other and with the better-known languages spoken in the central and eastern parts of Spain (Spanish and Catalan). Their analysis makes use of the fusion operation of Distributed Morphology and a series of timing principles, which interact with a syntactic component where clitics adjoin to a discourse-related functional head *f*. Finally, Benincà & Poletto ('On some descriptive generalizations in Romance') compare a wide variety of Romance forms and conclude that clitics in Romance have arisen through grammaticization of various lexical items. This grammaticization amounts to a shift from feature movement with pied piping to simple feature movement, where clitics are the overt realization of moved features.

Closely related to the topic of clitic placement is the architecture of the functional categories in the verbal projection (including the left periphery), and the linked issue of the licensing of arguments. This is the topic of several articles in the book (Aboh; Amritavalli & Jayaseelan; DeGraff; Rice & Saxon; Rigau; Munaro & Pollock; and Whitman).

In 'Object shift, verb movement, and verb reduplication', Enoch Aboh discusses Object Verb (OV) and Verb Object (VO) alternations in Gungbe. He argues that VO ordering results from object shift to the specifier of the functional category *AgrO* (SpecAgrOP) and subsequent movement of the verb to a higher aspectual position. By contrast, OV orders involve movement of the object from its case position (SpecAgrOP) into the specifier of an aspectual phrase (SpecAspP) in order to satisfy the Extended Projection Principle. When there is no overt noun phrase to move to this position, or when the noun phrase is a topic, the verb reduplicates in order to license a phonetically empty subject pronoun in SpecAspP. Two things surprised me about this paper. First, in early Minimalist work on agreement phrases (see, for example, Chomsky 1995), VO/OV alternation was taken to be evidence for movement of the object from the complement position of the verb to the specifier of *AgrO*. In Aboh's approach, both VO and OV word orders are derived by overt movement to SpecAgrOP, with OV ordering resulting from further movement. Second, while Aboh presents an interesting analysis of some unique data, it is not at all clear how this paper can be considered to be comparative syntax. There is some brief mention of the related languages Gengbe and Ewegbe, but these are relatively unimportant in the development of the analysis, which focuses on phenomena entirely within one language. I suppose the reader could be expected to compare the Gungbe data to object shift phenomena in other, more familiar languages, but this is left unsaid in the paper.

The topic of Nicola Munaro & Jean-Yves Pollock's chapter, 'Qu'est-ce-que (*qu*)-est-ce-que? A case study in comparative Romance interrogative syntax', is the interaction of clitics, negation, verb movement and the left periphery of the clause in *qu'est-ce-que* 'what is it that'-constructions in French and Northern Italian dialects. The variation observed in these dialects argues for two checking positions for bare *wh*-words, and suggests that dialects vary with respect to (i) whether disjunction operators are clitics or not; (ii) the parametric value determining whether disjunction operators are spelled out at PF; (iii) the parametric value determining the height of negation; and (iv) the elements that can occupy the force layer of the clause.

The book contains two papers that address the position of negation with respect to the verb. In 'Finiteness and negation in Dravidian', R. Amritavalli & K. A. Jayaseelan investigate the nature of negation in Kannada and Malayalam and conclude that at least one instance of the negative morpheme represents finiteness in the form of the head of a Mood Phrase. They argue that in Dravidian, Tense is better analyzed as Aspect, and that finiteness is represented by the presence of Mood. Differences between Malayalam and the other Dravidian languages follow from differences in agreement morphology. Given the topic of this chapter, that is, its focus on polarity and force-like elements, such as negation and finiteness, I was disappointed that this article included no discussion of Rizzi's approach to the structure of the left periphery, which is designed to address such questions.

The second paper that deals with the relative order of verb (V) and negation (Neg), John Whitman's 'Preverbal elements in Korean and Japanese', explains a variety of Neg/V orderings in the two languages by having recourse to an antisymmetric analysis which postulates movement of a remnant verb phrase. The analysis shows that negation may be realized both in an adjunct/specifier and in a head. Whitman claims that the negation patterns in Korean and Japanese represent a typical typology for verb-final languages.

Three articles in the book deal with agreement/inflectional restrictions on argument placement. In 'Comparative Athapaskan syntax: arguments and projections', Keren Rice & Leslie Saxon compare a large number of geographically diverse Athapaskan languages, focusing on the famous *y*-/*b*-alternation. They argue for three distinct subject positions: (i) the VP-internal subject position, (ii) the specifier of a Number Phrase (SpecNumP), and (iii) the specifier of a subject agreement (AgrSP) or Discourse Phrase (depending upon the language). They also claim that two object positions are available. The positioning of subjects and objects variously depends upon animacy, person, agency and definiteness. In 'Number agreement variation in Catalan dialects', Gemma Rigau presents an analysis of object agreement in existential clauses in two dialects of Catalan. She proposes that the variation observed between these two dialects amounts to differences as to which functional category *v* (complete *v** or weak *v*) is selected and how

uninterpretable features are valued by the probe-goal relation (Chomsky 2000). Finally, Michel DeGraff argues in ‘Morphology and word order in “creolization” and beyond’ that unlike either its lexifier language (French) or its substratum (Fongbè), Haitian Creole does not exhibit verb raising. Instead, it seems to leave the verb in situ. This observation is couched within the view that creoles are simply more obvious instantiations of the ‘re-creation’ of a language each time a learner sets its parameters, and stands in stark contrast to views of creole exceptionalism.

The next major group of articles concerns the internal structure of the Determiner Phrase (DP). Jaklin Kornfilt’s ‘Agreement and its placement in Turkic non-subject relative clauses’ centers on how agreement is realized in the structure of relative clauses in various Turkic languages. Kornfilt compares three types of relative clauses, which vary in the locus of agreement morphology. Most of this article is descriptive rather than theory-oriented, although it ends with some theoretical speculations on the source of the variation (Kornfilt proposes that agreement can be either nominal or verbal and that agreement markers can be either heads or clitics).

The nature of noun class, gender and classifiers is the topic of the three remaining papers investigating DP-structure. In ‘Classifiers in four varieties of Chinese’, Lisa Cheng & Rint Sybesma consider the variation found among these varieties in terms of what kinds of information are represented in classifiers (for example, whether or not the definiteness operator ι is realized segmentally), as well as co-occurrence restrictions between classifiers and numerals. They propose three parameters to account for this variation. Andrew Simpson addresses similar topics in his article, ‘Classifiers and DP structure in Southeast Asia’, but is also interested in accounting for DP-internal variation. He suggests that the different word order patterns in Southeast Asian languages stem from variation in whether there is DP- and X° -movement within the DP, and whether this movement results in lexicalization of higher functional projections inside the DP. This latter topic is addressed both synchronically and from the perspective of grammaticization. Finally, Alain Kihm’s article, ‘Noun class, gender, and the lexicon–syntax–morphology interfaces: a comparative study of Niger-Congo and Romance languages’, takes up related issues from the perspective of Distributed Morphology. Kihm claims that classifiers are members of the functional category n (a nominalizing category akin to verbalizing v). His focus is on two empirical domains: the difference between the inflectional suffixal nature of gender systems and the agglutinative form of classifier systems, and the tight link between classifiers and numerals. The empirical facts prove to be derivable from a combination of variation in head-movement and the late-insertion property of Distributed Morphology.

The remainder of the articles in the book are comparative descriptions of the syntactic similarities and differences among some of the major language groupings of Indo-European. In an article entitled ‘Continental West

Germanic languages', Jan-Wouter Zwart provides descriptions of scrambling, verb second, word order variation in root and embedded clauses, and Complementizer Phrase extraposition, as well as more detailed descriptions of the morphology (and morphosyntax) of pronominal and verbal systems in German, Dutch and closely related languages. Zwart's article is different from other articles in the book in that it contains almost no theoretical grounding and surprisingly few pointers to the theoretical literature on the topics of discussion. Moreover, the article focuses primarily on the similarities between the languages, as opposed to the differences that are the foci of so much other work in comparative syntax (although the article does contain some interesting discussion of major differences arising in Yiddish and Afrikaans).

Steven Franks's 'The Slavic languages' provides a similar, although more theoretically-directed and variation-oriented, description of the syntax of the Slavic languages. The paper covers case and agreement, the genitive-of-negation phenomenon, numerals, voice, clitics, multiple *wh*-movement, binding, aspect, and scrambling. In 'The Scandinavian languages', Anders Holmberg & Christer Platzack compare the mainland Scandinavian languages to the insular Scandinavian languages (Icelandic, Faroese), paying particular attention to adverbial positioning, transitive expletive constructions, object shift and the internal structure of noun phrases. Finally, Maggie Tallerman's contribution, entitled 'The Celtic languages', deals with empirical issues in the Celtic languages that have been influential in generative grammar. The nature of Verb Subject Object order is surveyed along with the related issues of object shift and subject placement. Tallerman also considers clefting, copular constructions, complementarity in agreement and morphologically-marked successive cyclic movement. An obvious omission from this chapter is any discussion of construct state nominals and the internal structure of the DP.

Before moving on to a general discussion of the book's organization, I would like to make one global comment about the content of the papers in the book. One trend that emerged from a large number of papers in this volume pleasantly surprised me. This was the increased emphasis on the role of grammaticization in explaining language variation. Of course this move is to be expected if, as Kayne notes, parametric variation is limited to the lexical entries of particular functional categories. Grammaticization turns lexical morphology into functional morphology; and, accordingly, variation between languages can be viewed both in a diachronic context (as parameter changing through grammatical reanalysis) and in an acquisitional context (as parameter setting through exposure to reanalyzed forms). In my opinion, this area promises to be fruitful for generative linguistics and should be pursued further.

As can be seen from the above summaries, this book is incredibly rich in both data and theoretical insights, and worth the price for that alone. Comparative syntax is a major trend in syntactic theory, and this volume

reflects this and the importance of the trend. However, I must admit that I am also a little disappointed in the volume as a whole. My concerns mainly have to do with what seems to be a very disjointed focus to the book and what I think is a significantly impaired organization.

Kayne's article, rich as it is, is not the kind of first chapter usually found in a book like this. His contribution is really a stand-alone research article rather than the introductory chapter one might expect from one of the editors of the volume. No attempt is made to establish connections between the other papers in the book, to outline the major theoretical issues in comparative syntax beyond the metatheoretical and theoretical points that Kayne himself attempts to address, or to provide a thematic guide to the organization of the book. There is only a very brief one-page preface to the book, which essentially admits (correctly) that the book is 'heterogeneous in style, in content, and in length' (v), and concedes (again quite correctly) that there is no sense in which a book like this could possibly be complete. To me, this seems like a missed opportunity: a short chapter by the editors which at least outlined some of the major findings of the papers in the volume would have been welcome indeed. The book is very large, and while it undoubtedly contains important contributions, it is hard to see how the individual articles hang together, except in the very loose sense that they all involve comparative research. I kept feeling that the book might have been better as a series of smaller and cheaper volumes that were properly thematically organized. Perhaps the whole notion of a handbook in linguistics is a strange notion to start with, as handbooks in other disciplines typically provide surveys and summaries of extant research in particular areas rather than a forum for presenting original research like many of the articles in this volume (and in similar 'handbooks' produced by Oxford University Press's competitor Blackwell – see Carnie 2004 for related criticism). It should be noted that some of the papers in the present volume do take seriously the question of providing a survey and summary of the current research in a topic (Tallerman, Holmberg & Platzack and Franks, among others), but many seem to be mainly reporting original research – a worthy goal, but not clearly in the purview of a handbook.

In addition to the absence of a proper introductory chapter, the book suffers from the fact that it is not thematically organized: while there are clear groupings of topics, the papers appear in alphabetical order. A related point is that, almost without exception, the papers do not cite each other, which is unfortunate since many of them address interconnected issues. This is particularly striking in the case of the papers on classifiers, which all take quite different approaches to similar topics. Finally, if this were truly a handbook in the sense that I understand the term, there should be greater homogeneity with respect to the scope and structure of the papers. The articles in this volume differ considerably both in their goals (which, amongst others, range from descriptions of language families and descriptions of

particular constructions to descriptions of methodological or theoretical issues) and in the role they attribute to theory. At one extreme we have the nearly theory-free articles of Zwart and Kornfilt (who are otherwise well known for their contributions to syntactic theory); and at the other extreme, we find the highly technical analyses of Rigau and Aboh. This gives the book a disjointed feel, and one must wonder if the authors were given adequate directions about the nature of the volume as a whole.

Clearer organization, clearer direction to the authors about the scope of their papers, and an initial summary chapter would have gone some way towards making this more of a handbook and less of simply a very large collection of research articles. This said, the overall caliber of the papers in this volume is excellent, as we would expect from the authors and editors involved. One cannot dismiss significant original research even if it is packaged strangely, so no doubt many *Journal of Linguistics* readers will find a great deal to value here despite the issues that I have raised.

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(Received 22 December 2005)

J. Linguistics 42 (2006). doi:10.1017/S0022226706234091
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Peter W. Culicover & Andrzej Nowak, *Dynamical grammar: minimalism, acquisition and change* (Foundations of Syntax 2). Oxford: Oxford University Press, 2003. Pp. xxii + 324.

Reviewed by HANS VAN DE KOOT, University College London

Dynamical grammar is the second part of a two-volume work entitled 'Foundations of Syntax'. Volume 1, *Syntactic nuts: hard cases, syntactic theory, and language acquisition* (Culicover 1999), investigated the properties of language itself, with the aim of establishing the boundary conditions on the learning mechanisms responsible for language acquisition. It focused especially on aspects of (syntactic) learning that do not exemplify the regular and exceptionless properties of human language, but rather the irregular, the

marked and the downright idiosyncratic. Volume 2, *Dynamical grammar*, turns to the internal properties of the learner and investigates whether it is possible to model the process of language acquisition in terms of a complex adaptive system. While *Syntactic nuts* was aimed predominantly at syntacticians with a primary interest in language acquisition, *Dynamical grammar* should be of interest to a wider audience, as it addresses fundamental questions about the nature of grammar and its relation to the mind, including issues relating to the computational properties of human language.

The book consists of three parts. Part I, 'Foundations', which includes two chapters, outlines the dynamical perspective on language and discusses problems with the Principles and Parameters view of language acquisition. Part II, 'Simulations' (chapters 3–6), contains computational simulations of lexical learning, syntactic learning, and language change. Part III, 'Grammar', which comprises the final chapter of the book, explores the adequacy of the kind of syntactic analyses to which the dynamical system approach gives rise.

Chapter 1, 'The dynamical perspective', presents the authors' view of grammar as a dynamical system and discusses how this differs from the I-language view held by Chomsky and his followers. The key claim advanced here is that there is no such thing as a mental grammar. Rather, the fact that human languages have properties that appear to lend themselves to description in terms of a grammar is a consequence of a combination of factors. First, language expresses thought, whose representations are themselves highly compositional. As a result, so it is argued, it is only natural that the structure of thought should reveal itself in the medium through which it is expressed. Second, the mechanisms for language learning are capable of generalization, and generalizations give rise to what appear to be rules. Finally, the favoured methodology in linguistics is to disregard exceptions and counterexamples. According to the authors, a linguistic theory does not characterize the mental object of study but rather the regularities in its external behaviour.

According to the dynamical perspective, what is present in the learner prior to acquisition is the representational system in which meaning is expressed, that is, Conceptual Structure (see Jackendoff 1990), itself a highly structured and principled system, and some minimal mechanisms for learning how to relate a string of words to a representation in Conceptual Structure. This dynamical system takes shape over time and eventually comes to behave as if it embodied a set of rules, but in fact the grammar is merely an emergent property of the dynamical system.

This view of grammar as an emergent property is contrasted with the Chomskyan perspective, which adopts a 'static' view of language, according to which we are born with a mental grammar upon which we draw to speak and understand our language. On this view, language acquisition amounts to setting parameters in this mental grammar on the basis of linguistic experience.

The chapter concludes with a description of the general properties of a dynamical system. Words and sentences are represented in a mental space. A sequence of words corresponds to a trajectory through this space. An often-traveled trajectory will have a lower energy requirement than a less commonly traveled trajectory. By requiring the system to minimize its energy requirements, it is forced into self-reorganization, grouping similar trajectories into 'flows'. The sequence of internal states that the system passes through as a result of these reorganizations should reflect the way humans learn and generalize.

Chapter 2, 'Language acquisition and linguistic theory', is a critical evaluation of the Principles and Parameters model of language acquisition. The authors outline various problems and paradoxes associated with the idea that the learner must parse input in order to set parameters. This outline converges on the conclusion that the Principles and Parameters model 'seeds' the learning environment to an unwarranted extent: if the generalizations embodied in parameters can be extracted from the environment without prior knowledge, then Occam's razor requires that the relevant generalization must not be attributed to an innate property of the learner.

The following three chapters set out to test, through a series of simulations, how much and what kind of prior knowledge must be attributed to the learner in order to account for language acquisition. Chapter 3, 'The computational simulation of language acquisition: *Aqui*', investigates the limitations of a purely distributional approach to the acquisition of syntactic categories and concludes that such an approach is inadequate. The best a distributional technique can achieve is to discover semantically determined distributional regularities (unless the input to the system is seeded with the syntactic information it is meant to discover in the first place). Chapters 4, 'Computational simulation of language acquisition: *CAMiLLe*', and 5, 'Experiments with *CAMiLLe*', explore a dynamical system, *CAMiLLe* (Conservative Attentive Minimalist Language Learner), that has access to the meaning of the expressions to which it is exposed. A meaning corresponds to a representation in Conceptual Structure. Since these representations are themselves hierarchically structured and compositional, the learning mechanism has 'considerable information about the likely syntactic structure of the linguistic expression' (102). The authors argue that this information may be sufficient for successful language acquisition.

CAMiLLe has two representational systems, one for syntax and one for semantics. The system's learning task is to formulate correspondence rules that map a string of words onto a representation in Conceptual Structure. This process can be understood as the formation of couplings between trajectories in a syntactic space and trajectories in a semantic space. Self-organization has the effect that couplings that are similar (for example, those that differ in only one element in each representation) will migrate towards each other. In other words, the same organizational process that is active within the syntactic

space and is responsible for grouping trajectories into flows is also at work in the mapping system between the syntactic and the semantic space.

Parsing is viewed as the process of reducing a string to a head. For example, when the string *tall man* is coupled with the meaning MAN(TALL), this causes *tall man* to be reduced to *man*. Furthermore, the parser has no way of representing anything resembling a movement chain.

The experiments with CAMiLLe focus on categorization, structure and word order. Successful categorization seems to be hampered by the small size of the artificial data sets. The problems are mostly related to the lack of ‘complete exemplification’: for successful categorization to take place a word must be presented in sufficiently many and sufficiently varied examples with associated meaning. But since the meanings for a data set must all be supplied by hand, the samples on which CAMiLLe learns are of necessity quite small.

The experiments with structure are concerned with the acquisition of the internal structure of Determiner Phrases, argument structure and parsing. The system does not perform well on the first task, for reasons that remain unclear. By contrast, the extraction of argument structure correspondences is more successful. The discussion of parsing explains the system’s limited ability to reduce adjacent words to a head. The process of finding a correct reduction ‘is complex and involves many wrong guesses’ (165). The basic system is also unable to deal with words that it recognizes but cannot assign a meaning to. The authors conclude that CAMiLLe’s strategy of reducing a string to a head is in principle able to assign correct structure at the phrasal level.

The experiments with word order, finally, produce mixed results. Thus, the system is unable to arrive at correct generalizations in the domain of scrambling, apparently because too much information at the start of learning offers too many opportunities for spurious generalizations from which the learning system cannot recover. In a similar vein, the presence of too much diversity in form and interpretation associated with inversion phenomena appears to present an insurmountable challenge for CAMiLLe (see Elman 1993 for related discussion).

In chapter 6, ‘Language change’, the authors argue that social networks can be modeled as dynamical systems and that language ‘gaps’ (logically possible but non-existent languages) may result spontaneously from the transmission of linguistic properties in such networks (see also Culicover, Nowak & Borkowski 2003). It is suggested that the existence of Greenbergian implicational universals can be understood along these lines. An extension of the model concerns a hypothesis about the main force driving reorganisation of the dynamical system, put forward by Culicover & Nowak (2003). They argue that computational complexity associated with a particular cluster of grammatical properties results in a bias against that cluster, which will in turn lead to gaps in the set of (logically) possible languages.

Finally, chapter 7, ‘Concrete minimalism’, turns to the question of whether the dynamical model lends itself to adequate accounts of well-known

linguistic phenomena. It starts off by explaining how various design features of a minimal grammar, such as lexical categories, phrasal categories, structure and movement, can be represented in a dynamical system. This is followed by ‘concrete minimalist’ treatments of head-complement order, verb raising to the inflectional head I, verb second and inversion, clitic ordering, null arguments, *wh*-movement and scrambling. The analyses put forward are in terms of a very much simpler syntax than those in current minimalist work, allowing for syntactic representations that map onto representations in Conceptual Structure in a more transparent fashion than their counterparts in current minimalist theories.

Dynamical grammar makes an important contribution to the ongoing debate about how much innate structure must be attributed to the language learner. It explores a form of Linguistic Cognitivism that attempts to restrict innate structure to the domain of semantics (Conceptual Structure). As far as syntax is concerned, all that the learner starts out with is a minimally specified dynamical system. The exploration is backed up by actual simulations, using a simple version of the kind of dynamical system that the authors envisage. This allows the authors to evaluate the strengths and weaknesses of their proposals, and gives the reader a much better perspective on the issues at stake than an abstract presentation of the ideas on its own ever could. On the whole, this is a thought-provoking and worthwhile exercise that bears fruit in a number of ways.

Alongside proposals about the nature of language acquisition, *Dynamical grammar* also presents arguments for what one could call a ‘simpler’ syntax. Although these two aspects of the book are intertwined – the simpler syntax is much more suited to the dynamical view of language acquisition on offer – readers who find themselves unable to accept several of the arguments against the Principles and Parameters theory may still find much of value in the arguments for a simpler syntax.

I would like to single out two points for further discussion. Part I of the book moves smoothly from an argument against mental grammar (and in favour of emergence) to an argument against innately specified properties of syntax. My own view is that it is unhelpful to conflate these issues, which are logically independent. One could reject the view that a speaker-hearer’s I-language is explicitly mentally represented and still accept that Universal Grammar is a correct theory of the emergent properties of our language faculty that our genes have been selected for. Put differently, our genetic endowment does not have to take the form of a mental grammar, present at birth. Instead, it may impose constraints on the way a dynamical system ‘unfolds’ over time.

The authors devote a lot of effort to establishing that the dynamical system can learn various aspects of natural language, but very little is said about what it cannot learn. This is an important omission because a general learning mechanism may be able to learn regularities that are not in fact

found in natural languages. It could perhaps be argued that the mapping of strings to representations in Conceptual Structure, coupled with pressure to keep this mapping simple, is sufficient to rule out such unwanted consequences (after all, the properties of Conceptual Structure are themselves innately specified). But it is far from clear that this logic will be sufficient in the general case. Consider an example. A'-movement gives rise to a freezing effect: the A'-moved constituent is an island for extraction. It seems implausible to attribute this to semantic properties of the resulting structure. If it is attributed to issues of computational complexity or 'extreme twisting in the correspondence between the string and the C[onceptual] S[tructure]' (236), then why are freezing effects absent in the case of A-movement?

Overall, I found this a very enjoyable and worthwhile book. Its provocative stance forces the reader to reconsider the adequacy of a number of fundamental assumptions in linguistics, which cannot be a bad thing. Several of the results reported in this book, and particularly those about language change, deserve every linguist's careful consideration.

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(Received 18 February 2006)

J. Linguistics 42 (2006). doi:10.1017/S0022226706244098
 © 2006 Cambridge University Press

Nomi Erteschik-Shir & Tova Rapoport (eds.), *The syntax of aspect: deriving thematic and aspectual interpretation* (Oxford Studies in Theoretical Linguistics 10). Oxford: Oxford University Press, 2005. Pp. xx + 309.

Reviewed by KAREN ZAGONA, University of Washington

This volume is an important contribution to the literature on aspect, argument mapping and interpretation, and the interface between the lexicon and the syntax. It includes an introduction by the editors and twelve articles by

linguists with substantial expertise in these areas. The articles are organized into three thematically arranged sections; they are well edited and very effectively presented, with the editors' introduction, an overview of the articles in the volume, and biographical notes on the contributors.

As the title indicates, the chapters are centrally concerned with how the grammar encodes the information that leads to the aspectual construal of predicates, and how it determines the arrangement and interpretation of arguments. In their 'Introduction' (chapter 1), the editors, Nomi Erteschik-Shir & Tova Rapoport, outline the theoretical questions that gave rise to the collection, and summarize previous approaches in the literature. They give a synopsis of the studies in this collection, situating the articles with respect to these issues, and in some cases with respect to one another.

Perhaps the most fundamental question is whether the semantic specifications of particular lexical items determine structural relations for predicates, or whether lexico-syntactic structure determines semantic relations, including thematic roles and temporal classification. Previous research has discussed correlations between certain structures and interpretation, such as the occurrence of Agents as external arguments or the availability of an accomplishment reading in the presence of certain internal arguments. The two main approaches are the 'lexicon-driven' approach, on which lexical semantics determines structural relations, and the 'syntax-driven' approach, on which structural relations – in conjunction with non-relational semantic features – determine relational semantics. The syntax-driven approach can be further partitioned according to whether or not the syntactic relations at play are taken to be lexicon-internal. Most of the papers in the volume propose syntax-driven accounts. This is perhaps not surprising given that, as the editors note, the workshop from which this collection developed (held at the Ben-Gurion University of the Negev, Beer-Sheva, Israel, in June 2001) set out to explore the viability of a syntax-driven approach.

Part I, 'From lexical roots to syntax', contains three articles that focus on the question of lexical primitives and their interaction with structure. In chapter 2, 'Aspect and the syntax of argument structure', Ken Hale & Samuel Jay Keyser analyze argument structure as essentially configurational, based on the relations that hold between heads and their arguments in the complement and specifier positions. They propose that verbs are composed of a verbal 'host' and a root that originates as the complement of the host. Both structural and intrinsically semantic properties of roots are claimed to be relevant for determining the full range of lexical properties of the derived verb, including certain mappings, such as the appearance of cognate objects and the availability of transitive/middle alternations. With respect to the temporal classification of predicates, Hale & Keyser argue that stativity alternations such as *We keep the calves in the corral*/*We put the calves in the corral* are attributable to semantic (and structural) features of the complement with which the verb combines.

Chapter 3, 'How do verbs get their names? Denominal verbs, manner incorporation, and the ontology of verb roots in English', by Heidi Harley, adopts Hale & Keyser's general framework. Harley argues that there is a need for finer distinctions between types of roots, in order to account for the structural and semantic conditions under which accomplishment readings are produced. She proposes that nominal roots are of several types (objects, states or events) and can be intrinsically bounded (countable) or unbounded (mass). These distinctions lead to an account of subtypes of accomplishments. Harley's analysis offers an explanation for what appear to be exceptions (for example, the verb *push*) to the generalization that direct objects measure out events. Like Hale & Keyser, Harley accounts for syntactic regularities in lexical syntax.

In chapter 4, 'Path predicates', Nomi Erteschik-Shir & Tova Rapoport present a model in which the syntax has a crucial role in characterizing predicate types and interpretation. The lexicon contributes 'meaning components' or semantic morphemes, which combine in limited ways to produce aspectually distinct predicate types. A similarity to Hale & Keyser's approach is found in the proposal of a relationship between category and meaning. For example, the feature MANNER projects the category Noun (N), STATE projects an adjective (A), and LOCATION projects a preposition (P). Combinations of features derive aspectual types, so that Verb + Noun derives an activity, while Verb + Adjective derives a 'change' predicate (which is Erteschik-Shir & Rapoport's umbrella term for achievements, inchoatives and unaccusatives/ergatives). Complex predicates such as causatives are produced in the syntax, based on the assumption that an item in a lexical array is not always removed from the array when it is first accessed and that, consequently, multiple copies can be merged. Support for the framework is given in the analysis of 'path' predicates, such as *advance* and *march*, which display a cluster of syntactic and semantic properties that are accounted for on the assumption that the notion of path corresponds to a feature for plurality.

Part II, entitled 'Event structure and feature projections', includes studies of thematic relations and event type. In chapter 5, 'Tense, person, and transitivity', Jacqueline Guéron argues that certain temporal and thematic generalizations follow from differences in how events are analyzed at different levels of clause structure. Guéron proposes that spatial event analysis is situated within the verb phrase, and that temporal event analysis is situated outside the verb phrase. Guéron argues that this accounts for differences in the behavior and interpretation of certain thematic relations. She proposes that at each level of structure, an external argument is selected that 'organizes' the predicate spatially or temporally. This provides an innovative way of accounting for thematic roles such as Benefactive, which are not easily described as event participants. Guéron suggests that arguments can be merged inside or outside the maximal projection of the light verb *v* and that their point of merger determines the type of semantic relation that is available.

In chapter 6, 'Complex aspectual structure in Hindi/Urdu', Miriam Butt & Gillian Ramchand examine Verb-Verb complex predicates that have been analyzed as a main verb combined with a light verb which adds aspectual information. They argue that some of these verbal structures instantiate the components of the verbal nucleus: v , V and the head of a result phrase R . The correlation between the morphosyntactic properties of the components and their aspectual properties provides evidence for lexical decomposition of verbs into cause/process/result heads. One of the interesting insights of this article is that in the languages which have the verbal complexes under discussion, these exist side by side with auxiliary-main verb and main verb-infinitive sequences, from which they can be distinguished both semantically and morphosyntactically. This implies that in at least some cases the formation of verbal complexes may be external to the lexicon.

The next two articles offer different types of evidence (using different approaches) that 'Cause' arguments are licensed by elements other than the $v+V$ projections. In chapter 7, 'The aspect of agency', Edit Doron characterizes aspect as involving either a temporal or a thematic structure imposed by a verb. Doron argues that Semitic verbs encode thematic aspect rather than temporal aspect. Thematic aspect is based on the primitive semantic feature of 'action' and causality, while temporal aspect is concerned with concepts of change and culmination. Doron proposes an account of three morphologically distinct types of verbs. She argues that verb stems consist of two parts, the root and the 'template', and that the latter can vary in its semantics and in its relationship to the root. While v determines the thematic role of Agent, the semantics of the template determine whether the Agent role is construed as 'Actor' or 'Cause'. Doron's analysis defends a lexically-driven approach to compositional meanings, with the meaning of the verb stem deriving from the meaning of its parts.

Chapter 8, 'Agents and causes in Malagasy and Tagalog', by Lisa Travis, argues that the presence of a Cause argument in these languages is determined by an aspectual morpheme that encodes telicity. Travis analyzes Tagalog *-pag* as a causative morpheme in v , and proposes that *-pag* deletes when its specifier is overtly occupied. In addition to *-pag*, Tagalog has a morpheme which forces a telic interpretation of an event and imposes a non-volitional cause interpretation on the external argument (if one is present). Travis argues that this second morpheme is generated in a position lower than v . If no external argument is present, a Cause argument is added. Travis' chapter, like those of Guéron and Doron, provides interesting evidence for a more complex, more compositional theory of thematic relations.

Chapter 9, 'Event structure and morphosyntax in Navajo', by Carlota Smith, discusses the typology of events in Navajo and argues for a Discourse Representation Theory (DRT) account, based on the interpretation of the surface structure of sentences. Following an introductory summary of the verb template, Smith proposes that three types of information are encoded in

the Navajo verb: Event structure (which specifies the situation type), Qualia structure (which identifies primitives such as Figure, Motion, Ground, Path, Manner and Cause) and Argument structure. She goes on to analyze how aspectual morphemes contribute meaning to the verbal base and can affect the three types of information encoded in the verb. The inceptive morpheme, for example, focuses on the beginning of an event, affecting both Event structure and Qualia structure. Aspectual morphemes also combine in restricted ways. Smith compares two ways of handling the facts: a syntax-driven approach in the spirit of Hale & Keyser's article and the DRT approach. She argues that the latter is more satisfactory in its ability to capture the whole range of temporal relations, that is, both the word-internal and word-external relations that are necessary for characterizing the temporal properties of predicates.

Part III, 'Lexical restrictions on syntax', contains four papers that address differences between lexical items or classes of items. Two of these discuss the conditions under which arguments can be covert. In chapter 10, 'Constructions, lexical semantics, and the correspondence principle: accounting for generalizations and subregularities in the realization of arguments', Adele Goldberg argues that the concept of 'construction' is central to the explanation of certain exceptions to the normal pattern in argument mapping. Constructions are described as learned pairings of form and function that diverge from predictable or principled pairings. It is suggested that these sub-regularities are motivated by non-syntactic principles, such as discourse demands. Goldberg proposes that arguments have obligatorily overt mappings if they are 'profiled', that is, particularly discourse-prominent. A correspondence principle aligns lexical semantic information with discourse information.

In chapter 11, 'Unspecified arguments in episodic and habitual sentences', Anita Mittwoch argues that context is crucial in certain cases of missing complements. In episodic sentences, intransitives are typically atelic, and transitives are telic. Habitual sentences allow missing arguments in a number of circumstances where the corresponding episodic event sentences disallow them. Mittwoch discusses several factors that allow arguments to be omitted, such as the ascription of dispositional properties (as in *Fido bites* or *Wool scratches*).

Chapter 12, 'Resultatives under the "event-argument homomorphism" model of telicity', by Stephen Wechsler, addresses the question of how a resultative phrase makes a predicate telic. Wechsler argues for an analysis of resultatives that is based on the event-argument homomorphism model of Krifka (1998) rather than on the state model of telicity. On this approach, resultatives involve an abstract 'path' argument which corresponds to degrees along the scale denoted by the resultative predicate, thus drawing a parallel between resultatives and locative paths. Wechsler shows that this accounts for previously noted lexical variation in the acceptability of

resultatives (cf. *wipe the table dry/*wet*), variation in category, and variation concerning the argument status of the direct object.

Finally, in chapter 13, ‘Change-of-state verbs: implications for theories of argument projection’, Malka Rappaport Hovav & Beth Levin discuss two claims about argument mapping that have appeared in recent literature: (i) the idea that aspectual interpretation drives argument mapping, and (ii) the proposal that argument expression is free rather than lexically determined. Rappaport Hovav & Levin examine change-of-state verbs, arguing that lexical properties are important for constraining or determining the options for argument mapping. The evidence they provide supports the conclusion that non-aspectual factors drive the projection of arguments.

Taken as a whole, this volume offers important insights into the structural and semantic underpinnings of the temporal and thematic properties of predicates, which take into account and are founded on a wealth of data from a range of languages.

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(Received 30 January 2006)

J. Linguistics 42 (2006). doi:10.1017/S0022226706254094

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Elly van Gelderen, *Grammaticalization as economy* (Linguistik Aktuell/Linguistics Today 71). Amsterdam & Philadelphia: John Benjamins, 2004. Pp. xiv + 320.

Reviewed by ANNA ROUSSOU, University of Patras

The formal expression of grammaticalisation has been the focus of attention in recent formal diachronic approaches. In this book, Elly van Gelderen argues that grammaticalisation is the output of two economy conditions: (i) Late Merge, and (ii) Head over Specifier. According to (i), elements are merged in the tree as late as possible, predicting that grammaticalisation can only be upwards (see also Roberts & Roussou 2003). According to (ii), re-analysis of phrases (specifiers) to heads is to be preferred, presumably because this ensures some form of morphosyntactic simplification, since heads are ‘lighter’ than phrases. Specifiers may be reintroduced, but as the author argues, this can be viewed as the result of a conflict between economy and

innovation. In other cases, simplification may be blocked under the pressure of prescriptive rules.

The book makes a very straightforward theoretical claim about the formal nature of grammaticalisation. The empirical basis is drawn primarily from the history of English. This approach has clear merits: first, the discussion of the data is very thorough and detailed; second, it provides an in-depth analysis of a number of constructions, supported by further comparative evidence when necessary. As a result, each case can be viewed on its own, enabling insights into diachronic syntax that go beyond grammaticalisation.

The book is organised into four parts, preceded by a useful list of abbreviations, and followed by a detailed index. Part I, which comprises the first two chapters, is introductory. It outlines the essence of the two economy principles and introduces the Layer Parameter (4), according to which languages tend to expand either the Inflection Phrase (IP) and the Complementiser Phrase (CP), i.e. they are 'IP/CP-active', or the Verb Phrase (VP), in which case they are 'VP-active'. Given Late Merge, one expects to find that grammaticalisation is responsible for the change from VP-oriented to IP/CP-oriented grammars. The empirical data in this chapter include cases of specifier-to-head reanalysis (pronouns, determiner-like elements, negative expressions) and some instances of Late Merge, which are discussed in more detail in the following parts.

Part II (chapters 3–6) discusses the CP layer, claiming that (embedded) CPs arise mainly out of paratactic constructions with a demonstrative in the first clause. The demonstrative is then reanalysed as part of the second clause, and, being a phrase, it is taken to occupy the CP specifier position (SpecCP) of the new embedded clause. Under the 'Head Preference' condition, it later becomes a C head. This is the case of English *that*, in both complement and relative clauses. CP was originally a single projection, and only later (around the thirteenth century) split into two functional categories – Fin and Force in Rizzi's (1997) framework – between which more than one Topic Phrase may be projected. Thus, *that* was first a Fin element (until around 1500), but then came to occupy Force in Modern English. The introduction of *wh*-elements in relative clauses, which was possibly due to language contact with French and Latin, created a conflict in the system, resulting in a preference for *that*-relatives. Finally, the element *for* first appeared as the head of a Prepositional Phrase in SpecCP, and then was reanalysed as C. The sequence *for that* suggests that, more precisely, *for* was reanalysed as Force in finite complements (but was in Fin in non-finite complements). This reanalysis goes along with (or presupposes) the presence of a split CP.

Part III (chapters 7–11) is perhaps the most interesting and original part of the book. Assuming that I splits into Tense (T), Mood (M) and outer Aspect (Asp), and that inner aspect (E) projects between the light verb *v* and V, the author provides a detailed account of deontic modals and epistemics in

Modern English. The proposed analysis of deontic modals as Asp heads and of epistemics as M heads predicts that deontics, unlike epistemics, are incompatible with +/– perfective auxiliaries (cf. *John must have left*, which has only an epistemic reading). In the same context, it is argued that *saw* followed by a bare infinitive, as in *John saw him cross the road*, is an evidential in Asp, which explains its incompatibility with other aspectual elements. The perfective reading associated with the bare infinitive derives from the interpretation of the perception verb as stative. If the verb is eventive, then the infinitive can be imperfective (cf. *Seeing him cross the road...*). We could then treat the bare infinitive as unspecified (or underspecified) regarding perfectivity, although this is not explicitly stated as such in the text. The absence of the relevant constructions in Old English is expected since modals were main verbs in this period – a situation which started changing in Middle English.

Van Gelderen next considers the effects of the so-called Tense Aspect Parameter. From a setting where (inner) aspect was unmarked, we move to a system (around the nineteenth century) where Tense ('Moment') becomes unmarked, giving rise to the formation of the present progressive (*be* + *V-ing*) for the denotation of 'Interval'. A precondition for this change was the replacement of the inner aspectual head E by (outer) Asp (auxiliaries), triggered by the loss of the Old English aspectual prefix *ge-* and related markers, which occurred around the fifteenth century. Interestingly, Modern English has reintroduced E through the use of particles, as, for example, in phrasal verb constructions (e.g. *take off*). Finally, the infinitival marker *to* in M shows signs of becoming C (which is consistent with Late Merge), as evidenced by its doubling in the presence of negation in some dialects, cf. (1).

(1) % ... for Thatcher to not to do this (37), 239)

Part IV, which comprises the last two chapters of the book, provides a more general discussion in the context of parameters. It argues that Old English was a partial Pronominal Argument Language (PAL), as well as 'VP-active' according to the Layer Parameter. The development of the left periphery in Middle and Modern English, brought about by more instances of Late Merge, made the language more analytic – a situation that was further supported by the loss of pronominal marking. The need for innovation was responsible for the creation of new specifiers.

As is evident from the above brief presentation, Late Merge appears to have the effect of creating new functional structure: higher functional positions – arguably unavailable before – are provided in the tree. As such, economy eliminates steps of Merge, but at the expense of introducing new positions. One may think that this is a desirable result, since it keeps the system at an equilibrium. What remains rather unclear in the discussion throughout is to what extent Late Merge presupposes Movement (internal Merge in current terms) and its loss – an issue which is addressed in Roberts & Roussou (2003).

A second observation has to do with the link between the Layer Parameter and the availability of pronominal marking. Although intuitively this approach may sound appealing because it captures the usual trade-off between syntax and morphology, that is, the fact that more internal/morphological marking correlates with fewer syntactic positions (and vice versa), the way it is formulated leaves certain questions open. In particular, the role of morphology is somewhat downplayed in the analyses of the various constructions in parts II and III. Moreover, while Italian, Spanish and Greek (to mention only a few) are categorised as CP-active languages, these languages also have a rather rich pronominal system, which includes subject agreement affixes and object clitics.

Another point has to do with the impact that prescriptive rules may have upon the grammar, which van Gelderen uses to explain why *whether* failed to stabilise as a head (92–96). Although one would expect prescriptive rules to have an effect on the language use of educated speakers, this is not expected to be the case with less educated speakers. Note that an alternative, grammar-internal explanation may be available: it is possible that the morphophonemic structure of *whether* made reduction to a head more difficult. On the other hand, grammaticalisation can still have played a role since *whether* has restricted its function to that of a *wh*-complementiser. Restriction of function, then, can be viewed as another instance of simplification, as argued by Roberts & Roussou (2003).

On the more technical side, there are some disputable issues. The first concerns the treatment of *that* as a Force rather than a Fin head in Modern English (105–107). Note that this account cannot be quite right for Standard British English, given the interaction of *that* with subject extraction, as in *Who do you think (*that) left?* (see Rizzi 1997). Perhaps reanalysis of *that* as a pure Force head has taken place in those dialects which appear to violate the *that*-trace effect. A second issue has to do with the role of *to* in (exhaustive) control complements, which are argued to be mono-clausal. The relevant structure is given in labelled bracketing below.

(2) [TP T [MP [M to] [AspP [Asp {manage, begin, go}] [VP]]]] (149)

According to van Gelderen, *manage* (or *begin* or *go*) moves to M to pick up *to* and then moves to T. The problems are threefold. First, if V incorporates into *to*, the two should form a unit not interruptable by other material, contrary to fact. Second, this incorporation requires right-adjunction of V to *to* in order to yield the correct order, suggesting that right-adjunction be allowed (*contra* Kayne 1994). Third, the author appears to accept implicitly that main verbs raise to T in English, contrary to most standard assumptions in the literature.

Note that the issue of right- versus left-adjunction also arises in relation to Old English *ge-*, as in the following structure:

(3) [VP V [EP [E *ge-*] [VP V]]] (210)

In order to get *ge-V*, the verb would have to right adjoin to E, which raises once again the question whether right-adjunction is permitted in this system, especially as van Gelderen invokes an equivalent structure for Dutch (216), but at the same time assumes Kayne's (1994) analysis for SOV languages.

A few minor points arise in some other places. For example, the ungrammaticality of the example in (4) is accounted for in terms of the selectional restrictions of a deontic modal for an eventive predicate.

(4) *An orange must/may be healthy. ((13), 162)

However, the problem is surely more complex than that, since an animate subject renders the sentence grammatical, as seen in (5), where it is a soldier's obligation to remain/stay healthy.

(5) A soldier must be healthy.

Finally, a note on the change involving subjunctives and infinitives, which is briefly discussed in the final chapter of the book. On the one hand, the change from subjunctive to indicative does not have to correlate with grammaticalisation. In the case of Hellenistic Greek, for example, the two inflectional paradigms collapsed due to independent morphophonological changes in the grammar. On the other hand, the Greek infinitives discussed in Joseph (1983) (incidentally, the entry 'Joseph' is missing from the list of references) were neither 'inflected' nor replaced by 'inflected infinitivals' (270), but simply replaced by finite, ultimately indicative, forms, which are introduced by different C-type elements. In the context of the proposed analysis, these developments can in fact be seen as consistent with Late Merge without resorting to a mono-clausal analysis: upon the loss of infinitival morphology, Late Merge favoured the projection of a C position which was lexicalised by either *oti* 'that' or the modal particle *na*, depending on the selecting predicate.

To conclude, the book makes a clear theoretical claim. As such, it is an important contribution to the study of grammaticalisation in particular and of diachronic syntax in general. It will also be of great interest to anyone concerned with language change and syntactic theory.

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(Received 3 February 2006)

J. Linguistics 42 (2006). doi:10.1017/S0022226706264090
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Bruce Hayes, Robert Kirchner & Donca Steriade (eds.), *Phonetically based phonology*. Cambridge: Cambridge University Press, 2004. Pp. viii + 375.

Reviewed by MARC VAN OOSTENDORP, Meertens Institute

The problem presented by Chomsky & Halle's (1968) 'overly formal' (400) treatment of phonological features as devoid of intrinsic content is still awaiting a fully satisfactory solution. The book under review provides an overview of work done to solve this problem under one very specific set of assumptions, combining recent insights into phonetics with phonological Optimality Theory (OT). More specifically, the assumption shared by nearly all articles in this book is that language-acquiring children have some implicit knowledge of phonetics, which renders much of abstract phonology superfluous. The contributors discuss various implications of this stance for our views on classical problems in phonological theory, such as sonority, place assimilation, vowel harmony, contour tones, vowel reduction, contrast, syllable weight, lenition and the Obligatory Contour Principle (OCP). The ten articles are preceded by an introductory chapter, 'Introduction: the phonetic bases of phonological Markedness', in which Bruce Hayes & Donca Steriade present a clear and lucid outline of the basic philosophy behind the enterprise of phonetically based phonology (PBP).

I consider *Phonetically based phonology* a very important book because if the field of phonology is not to fragment, there must be serious debate about the differences in fundamental assumptions, and this book contributes to this debate by representing a particular view of phonology in an intelligent and coherent manner – even if it does not pay a lot of attention to alternative frameworks. Since space does not permit me to consider each article in detail (readers who want to have an overview of the various topics discussed are advised to consult the website of the first-named editor), I will concentrate here on two topics which are essential for evaluating the framework of PBP as a whole: (i) a comparison between PBP and the classical assumptions of OT; and (ii) the relation of PBP to other models of the interaction between phonology and phonetics.

The central difference between PBP and classical OT is the source of the constraint set. Whereas Prince & Smolensky (1993) assume that all constraints are universal and innate, PBP subscribes to neither of these assumptions and assumes that speakers use both the available evidence and their implicit phonetic knowledge, i.e. 'the speakers' partial understanding of the physical conditions under which speech is produced and perceived' (I), to derive constraints. Phonetic knowledge is potentially universal, but not necessarily completely innate. Although none of the articles is very explicit

on this, the assumption is that ‘Universal Grammar (UG) [is] primarily ... a set of abstract analytical predispositions that allow learners to induce grammars from the raw facts of speech’ (Hayes 1999: 244). One important reason for taking this step is that it is believed by the proponents of PBP that the cognitive universals in UG are inherently arbitrary and that we should have as few of them as possible. This in itself is nothing but an application of Occam’s razor, but it should be borne in mind that the phonetic knowledge that PBP posits will also come with a substantial cognitive cost – for example, when it is assumed that certain putatively universal constraint rankings ‘reflect the speaker’s knowledge that ... a syllable should be able to host a tone with a lower complexity before it can host a tone with higher complexity’ (159). It is difficult to make a judgement on the relative costs and merits of innate constraints versus an innate capacity to make acute phonetic observations, but it seems unlikely that PBP represents the ‘null hypothesis’ in any reasonable sense of the term.

Another relatively new development of PBP as compared to classical OT and other earlier generative approaches is the central place accorded to perceptual factors. While earlier approaches to the phonetics-phonology interface, such as Chomsky & Halle (1968), concentrated almost exclusively on articulatory phonetics, in this volume, even articles that deal with phenomena seemingly amenable to an articulatory analysis include a perceptual component. For example, in ‘The typology of rounding harmony’, Abigail Kaun shows how an understanding of perception can provide a new perspective on Turkic vowel harmony. She attributes the behaviour of high vowels, which are good targets for harmony but only rarely triggers, to the fact that rounding differences are particularly easy to perceive on high vowels, proposing that vocalic properties spread from positions where they stand a lesser chance of being detected to positions where they may be perceived more readily.

The attention paid by PBP to perceptual factors can be viewed as a positive development: not only is there no a priori reason to restrict our interest in phonetics to articulation, a more extensive phonetic theory is in fact required, if a reduction of phonological explanation to phonetic explanation is to be viable in cases where both phonological and phonetic explanations exist and economy considerations suggest dispensing with one. On the other hand, taking into account perceptual phonetic factors may lead to a doubling of explanations. Consider, for example, Katherine Crosswhite’s discussion of the minimal vowel set /i u a/ in her contribution entitled ‘Vowel reduction’. Several phonetic reasons can be (and have been) given for the special status of these three vowels: (i) ‘the corner vowels /i u a/ are special in that they are maximally acoustically distinct’ (195); (ii) they ‘are also special in terms of their production: they all show quantal effects’ (195); (iii) they ‘are distinguished by having not only articulatory stability, but also by the fact that they do not share this characteristic with adjacent qualities’ (195);

(iv) they ‘are perceptually special by virtue of having spectral prominences caused by convergences: either proximity of two formants ... or proximity of the first formant and the fundamental frequency’ (196). Crosswhite concludes that the status of the corner vowels ‘does not seem to be linked to any single characteristic, but perhaps to the amalgamation of several’ (196). This doubling of explanations is not particularly convincing, given that it is precisely for economy reasons that we try to reduce phonology to phonetics in the first place. Moreover, the question arises why other vowel sets, such as /i u ə/, which shares some of the characteristics of /i u a/, do not seem to play a special role in language typology, as Crosswhite herself points out.

Jongho Jun’s analysis of ‘Place assimilation’ is another example where reference to phonetics does not necessarily ensure a more constrained theory. Jun bases his analysis on the following two constraint schemas:

- (i) (a) PRESERVE_{*n*}(place): Preserve at least *n* percent of the perceptual cues for place of articulation. ((14), 71)
- (b) WEAKENING_{*m*}: Do not produce an articulatory gesture whose effort cost is at least *m*. ((13), 70)

However, these definitions are not self-evident, since there is no a priori way of defining how much ‘percent’ of perceptual cues are preserved in some input-output pair, nor is there a well-established way of measuring effort costs. It seems likely that the relevant measurements could be done in many different ways, which would give different outcomes and therefore different scales and different predictions.

The inclusion of Stefan Frisch’s article, ‘Language processing and segmental OCP effects’, which is based on psycholinguistic work on processing rather than on articulation and perception, raises the question of how exactly we define phonetics. In this context, it is interesting to note that Hayes & Steriade (4f.) distinguish between ‘inductive’ and ‘deductive’ approaches to markedness:

Most attempts to discover markedness principles in phonology have proceeded, until recently, in inductive fashion: phonologists accumulate factual observations about languages and, in due course, a cluster of such observations coheres into a law ... [I]n most cases, the laws originate as generalisations over known languages, not as principles explaining why these laws should be expected to hold. A set of such laws, when they survive peer review, forms a proposed theory of markedness ... The work reported in this volume proceeds deductively ... by asking at the outset variants of the following questions: Are there general properties distinguishing marked from unmarked phonological structures, and, if so, what are they? ... Deductive research on phonological markedness starts from the assumption that markedness laws obtain across languages not because they reflect structural properties of the language faculty, irreducible

to non-linguistic factors, but rather because they stem from speakers' shared knowledge of the factors that affect speech communication by impeding articulation, perception, or lexical access.

It seems too strong to equate the distinction between earlier approaches and PBP with a distinction between inductive and deductive research. In the best generative work on markedness – I assume that Hayes & Steriade are referring here to a variety of generative traditions, including more formalist versions of OT – one indeed generalises over known languages, but the generalisations are theoretically inspired, taking the form of a model of segmental, syllabic or metrical organisation, which is then taken as a set of axioms from which new hypotheses are deduced. Inversely, phonetically inspired work can hardly live on deductive principles alone: in almost every paper in this volume, the author tries to understand some (inductively established) generalisation and to find an articulatory or perceptual explanation for it in a post hoc fashion.

In fact, it is debatable how deductive the PBP approach really is. In order to be deductive, we need to have a set of axioms from which hypotheses can be derived, and it is not clear that such a set of axioms is available within phonetics. In any case, no such set is presented in any of the contributions. On the contrary, phonetics is used at various places in this volume as a tool to derive something that has been established beforehand. For example, in 'A review of perceptual cues and cue robustness', Richard Wright provides a long list of perceptual cues and factors for cue robustness in order to derive the effects of Sonority Sequencing – a classical principle of generative phonology, arrived at by induction. While such rather ad hoc deduction may be an inevitable aspect of any scientific enterprise, it calls into question the claim that PBP is particularly deductive, with its implicit suggestion that deduction is superior to induction.

A final point to note in comparing PBP to other generative theories concerns the relation between formalism and substance. One particularly relevant contribution in this respect is Matthew Gordon's article, which provides an overview of various phonetic factors that contribute to 'Syllable weight', such as length, vocalic height and sonorancy of coda consonants. As the discussion shows, in actual phonological systems there seems to be an 'upper limit of formal complexity tolerated by weight distinctions' (289), leading Gordon to comment:

I thus offer the following definition of complexity as a working hypothesis: a weight distinction is too complex if it refers to more than one place predicate. (289f.)

This notion of complexity is purely formal and therefore purely phonological in Gordon's view: even though there is no articulatory or perceptual reason to avoid this type of complexity, UG imposes restrictions on syllable weight. But notice that this conclusion is reached by means of induction alone.

In recent years, the relation between phonology and phonetics has been the object of many studies (although perhaps of too little debate), but most of the contributions in this book do not refer to other models. Even authors who pursue a similar agenda, such as Paul Boersma (whose separate grammars for perception and articulation distinguish his approach from PBP, which assumes a single grammar where both perceptual and articulatory constraints are interspersed), Björn Lindblom, John Ohala, Janet Pierrehumbert and Joan Bybee are not mentioned in this book. Voices in the debate which defend opposing views, such as McCarthy (2002), who notes several problems with Edward Flemming's particular implementation of PBP (none of which are acknowledged in Flemming's chapter on 'Contrast and perceptual distinctiveness'), or Hale & Reiss (2000), are similarly missing. The absence of such cross-references is worrying because it leaves the field in danger of falling apart into a number of schools, each with its own methodology and discourse.

It has to be mentioned that one article in this collection presents a view which is very different from that of all the other contributors. In 'The evolution of metathesis', Juliette Blevins & Andrew Garrett offer a four-way classification of phonological theories: (i) synchronic and nonfunctionalist (e.g. classical OT); (ii) synchronic and functionalist (e.g. PBP); (iii) diachronic and functionalist (e.g. Grammont 1950); and (iv) diachronic and nonfunctionalist, which describes the authors' own approach. Blevins & Garrett do not share PBP's assumption that speakers need to have phonetic knowledge. In their view, languages change because speakers interpret the phonetic signal that they hear in a different way. Phonetically natural misapprehensions are more likely to occur than unnatural ones, and this is the way in which natural patterns will tend to creep into the linguistic structure. Thus, there is no longer any need for an independent phonetic markedness component in the grammar: phonetics is external to the speakers' heads.

As explained in the editors' introduction, Blevins & Garrett's view of language change sits uneasily with Hayes & Steriade's belief that certain language changes are teleological, that is, moving in the direction of a less marked structure. But if indeed 'most of the evidence that could bear on either side's position remains to be gathered or considered, and [if] further attention to this debate could lead to research progress' (27), it is regrettable that the authors in this volume have not paid more attention to differences between their own framework and other approaches, past or present. For instance, one might wonder what 'goal' the language system is trying to achieve. Much here depends on our conception of the function of language. Underlying almost all work within PBP is the idea that the primary, and perhaps the only, function of language is to transmit information. Seen from this perspective, it is important for the speaker to make her signal as clear as possible without making too much effort. However, no argument is given for this assumption that efficient communication is the central, or indeed the

only relevant, function of language, and there is no discussion of any of the other functions human language may have. Since Jakobson (1960), it has usually been assumed that language fulfils emotive, conative, referential, phatic, poetic and metalingual functions. While it seems clear that some of these functions are more important than others, it is far from clear what the central function of language is and, hence, it is somewhat disconcerting that a theory should centre around the referential function of language without giving any arguments for its special status (see Hale & Reiss 2000 for similar criticism).

Given the implicit assumptions of PBP, the function of individual objects proposed in the model is not always transparent. One case in point concerns Dispersion Theory, which is presented in a very lucid way in Flemming's article. Within Dispersion Theory, it is assumed that vowels are part of a vowel system, and that vowel systems are organised in such a way that vowels are maximally different from one another. One interesting consequence of this is that the theory incorporates certain structuralist insights which were not available in earlier stages of generative grammar (cf. Scheer 2004). Thus, Saussure's claim that every element of a linguistic system exists only by virtue of the fact that it is in opposition to other elements translates into the Dispersion Theory claim that the phonetic distances corresponding to the differences between forms are optimised in OT tableaux. It seems to me that it follows from this that something like a 'vowel system' is a concrete object in the theory. Since PBP subscribes to the generative idea that a grammar is a mental object, a speaker must have a vowel system in her mind, independent of the concrete words in the lexicon. However, what is less clear is what the functional need for such an object would be. Why would the language user come equipped with a mental map of a vowel triangle on which dispersion is calculated, and where the vowels in individual words need to be related to the vowels in the triangle? In the standard generative view, a vowel system is an epiphenomenon at best; and from a functional point of view, it would make more sense to disperse words rather than individual sounds (McCarthy 2002). Vowel systems are abstract objects for which there is no obvious functional need. Issues such as these warrant more discussion in this book.

To conclude this review, the issue of how phonetics and phonology are related is a very important one, and it is potentially beneficial for both fields of study to consider their interaction more closely. This is one of the goals of PBP, which sets out one specific proposal and applies it in a generally coherent way to a number of phenomena that are crucial for any phonological model. Many of the assumptions underlying work in PBP are not shared by other phonologists. Therefore, it would have been helpful if these assumptions had been laid out more explicitly and if they had been compared more carefully to other possible sets of assumptions. Phonology has a considerable tradition upon which to draw. It is not helpful to ignore this tradition or

dismiss it as mere inductivism. Of course such comparison may define a programme for future work, both for the authors and for the readers of this thought-provoking volume.

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(Received 19 September 2005)

J. Linguistics 42 (2006). doi:10.1017/S0022226706274097

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Raymond Hickey (ed.), *Legacies of colonial English: studies in transported dialects* (Studies in English Language). Cambridge: Cambridge University Press, 2004. Pp. xx + 713.

Reviewed by CLAIRE COWIE, University of Edinburgh

On first appearances, this book establishes a sorely needed historical basis for the study of extraterritorial Englishes (ETEs). The organisation of this 713-page volume by geography (there are sections on the New World, the Southern hemisphere and Asia) follows the pattern of a number of textbooks on Englishes outside of Britain (Trudgill & Hannah 2002, Melchers & Shaw 2003), and in this way promises to be a companion to such works. The book aims, according to its editor, to describe ‘the development of English at various overseas locations during the main period of colonialism, between the early seventeenth and the late nineteenth centuries, and up to the present

day' (xix). Englishes arising from native speaker settlement, we are told, will be treated in addition to 'forms of English which arose from the functional need for a means of communication in societies with many background languages but without a significant tradition of settler English' (xix). Whether the variety in question is a continuation of the language of settlers or not, contributors are said to address the following common questions:

- (i) What input varieties were operational in individual colonies?
- (ii) What evidence is there for the continuity of dialect features, and what can we deduce about their 'realignment' and their 'alteration'?
- (iii) To what extent is the form of the emerging English affected by interaction with 'background languages'?

This inclusive approach promises to avoid the pitfalls of a priori distinctions between native and non-native varieties, 'contact varieties' and creoles. But the main difficulty of the book lies in the highly varied nature of the contributions, which certainly do not address all of the above questions in equal measure. Some, offering fresh data and analyses, are clearly the 'studies' of the subtitle; others are overviews of the existing literature on input, and others are simply reference guides to particular ETes.

Part I (chapters 1–3), entitled 'Out of Britain', presents the input dialects from England, Scotland and Ireland for the relevant historical period. Raymond Hickey's chapter on England, 'Dialects of English and their transportation', clearly cannot give a full account of English dialects over a 300-year period, so it concentrates on features that have not been transported, or that have been 'lost at source'. Chapter 2 on Scots, Caroline Macafee's 'Scots and Scottish English', by contrast, is a reference guide on the features of Standard Scots English and Ulster Scots, with occasional references to situations in which features of these varieties resurface, for example, rhotacism in New Zealand's Southland dialect. Chapter 3, 'Development and diffusion of Irish English' by Hickey, is more of a social history of Irish English, from its origins in the spread of English to Ireland, to its subsequent diffusion from Ireland to the Caribbean, parts of Britain (such as Merseyside), the U.S. and Canada.

About half of the contributions in part II (chapters 4–12), 'The New World', are 'studies', while the other half are better described as overviews of the most influential work on the emergence of North American varieties. In 'Solving Kurath's puzzle: establishing the antecedents of the American midland dialect region', Michael Montgomery reflects on the twentieth-century dialectology of the Midland region and suggests that Hans Kurath's characterisation of the 'Scotch-Irish' character of this speech region was a deduction from settlement history rather than from tangible linguistic evidence (314). Montgomery's own work (inspired by research on African American Vernacular English) relies on syntactic patterns to reconstruct the relationship, most notably subject type constraints on present tense *-s*, and double

modals. According to Edgar Schneider's 'The English dialect heritage of the Southern United States', Montgomery's work on present tense *-s* and Ulster Scots in Appalachia constitutes some of the best evidence so far for 'a direct transatlantic dialect legacy' (290). Schneider's treatment of the (white) English of the South attempts to reconcile some apparently diverse theories, from the 'colonial lag' view associated with Kurath (the conservative transported dialect simply reflects older forms of the input dialect) to Bailey's (1997) view that the forms of the South are more recent innovations. Some unique features of the South can be traced to British varieties, but British input is often not discernible, as 'British sources tend to be limited in weight and applicability' (300). Many Southern features simply fit into patterns of what Schneider calls international non-standard English.

Walt Wolfram & Natalie Schilling-Estes' paper, 'Remnant dialects in the coastal United States', brings together work which has made a significant contribution to the study of the transportation of dialects. By comparing grammatical and phonological variables across 'remnant' communities on the Atlantic coast, the authors are able to differentiate between (i) 'relic' features, such as *a*-prefixing (*she was a-walking*), which establish a direct link with a British dialect; (ii) traits that can be traced to regional patterns of distribution in the dialects of England, for example, subject type constraints on present tense *-s* associated with Northern English and Ulster Scots; (iii) features that were part of an early American regional dialect that diffused; and (iv) innovative features in remnant communities.

Sandra Clarke's findings in 'The legacy of British and Irish English in Newfoundland' contrast with findings for other isolated remnant communities in that there is little evidence of focusing or levelling. The two communities on the island (south-west British and Irish) have maintained quite separate identities. The two varieties do not straightforwardly mirror the source varieties, apart from some relic features.

Merja Kytö's chapter, 'The emergence of American English: evidence from seventeenth-century records in New England', offers insights into the methodological problems that have dogged the study of the origins of North American English. She aims to test established connections, like that between East Anglia and New England, using the 715,000-word Early American English Corpus, which includes documents such as the records of the Salem witchcraft trials from 1692. Kytö finds few illustrations of expected *r*-lessness in spelling and few examples of uninflected *do*. The latter are unlikely to be captured in writing, and we are left with only glimpses of transported dialect. In 'English dialect input to the Caribbean', Raymond Hickey runs into similar difficulties when he searches for corpus evidence of Irish input to Barbadian English. For instance, the *does + be* habitual construction is not attested in Irish English before the nineteenth century.

Laura Wright uses the Court Minute books of Bridewell and Bethlem to establish the levels of variants of the third-person singular present tense in

‘The language of transported Londoners: third-person-singular present-tense markers in depositions from Virginia and the Bermudas, 1607–1624’, that is, the language of prisoners sent to Virginia in the early seventeenth century. In ‘Back to the present: verbal *-s* in the (African American) English diaspora’, Shana Poplack & Sali Tagliamonte reprise their work on the diaspora of African American Vernacular English. Revisiting the Samaná data, they show that the factors influencing the distribution of verbal *-s* (subject type, habitual aspect, and phonological conditioning) pattern similarly to those influencing its distribution in a corpus of Devon English. The authors are unique (in this volume) in their use of multivariate analysis and are notably more optimistic than their co-contributors about the possibilities of reconstruction. J. K. Chambers’ contribution, ‘“Canadian dainty”: the rise and decline of Britishisms in Canada’, demonstrates how Britishisms in Canadian English (e.g. the pronunciation of *leisure* to rhyme with *measure*) have waned in the twentieth century, against a detailed backdrop of the loyalist origins of the Canadian accent and changing attitudes to Britain.

Part III (chapters 13–18), ‘The Southern hemisphere’, presents studies of the more recently evolved Southern hemisphere varieties, which tend to differ from studies of the New World in that they are usually able to establish extensive phonological similarities to the varieties’ British source, the South-East. This is true of Roger Lass’s account of ‘South African English’. Lass does not speculate, however, on the manner in which this variety has developed, believing that the shapes of Southern hemisphere Englishes can only be explained ‘in the light of the times and circumstances of settlement’ (364). Vital material on the language of South African settlers from 1820 to 1870 remains to be collected (383).

This makes it easy to see why Elizabeth Gordon & Peter Trudgill’s contribution, ‘English input to New Zealand’, is so frequently referred to by other authors. Gordon & Trudgill have discovered sound recordings from the 1940s of elderly New Zealanders, born at a time when the now fairly homogeneous New Zealand variety was still in flux. Individual speakers show striking combinations of features which have not yet been levelled. The features which ultimately ‘win’ are those that are sociolinguistically and linguistically unmarked, although for any details on this process we are referred to other publications by Gordon, Trudgill and colleagues. Interestingly, many well-known features of New Zealand English are not a part of these dialect mixes – the centralisation of the vowel in *kit* and the raising of the vowel in *dress* and *trap* are independent later developments. Scott Kiesling’s ‘English input to Australia’ is more of a social history than an exercise in historical linguistics, but it provides background which enables us to see the ways in which Cockney variants acquired prestige and came to dominate Irish variants, despite considerable numbers of Irish immigrants.

Tristan da Cunha and the Falkland Islands of the South Atlantic are the Southern hemisphere equivalent of remnant dialect communities. In ‘English

on the Falklands', Andrea Sudbury is able to trace the population of the Falkland Islands to Scotland and the British South-West. She notes that there has been little focusing among the population and that there is considerable intra-speaker variation, which she attributes to a transient population and lack of a unique regional identity. Despite Daniel Schreier's claims that Tristan da Cunha is the ideal situation to test out theories of dialect development, we learn little in his contribution, 'English transported to the South Atlantic Ocean: Tristan da Cunha', about how the speech of the islanders has retained British, American, St Helena creole and untraceable non-standard features from the settlers who made up its population (scarcely over 30) in the nineteenth century.

Suzanne Romaine's article, 'English input to the English-lexicon pidgins and creoles of the Pacific', differs dramatically from Hickey's treatment of the Caribbean. Romaine concentrates on jargon varieties as input, mostly through a close examination of individual lexical items. These detailed etymologies tell important stories about the Pacific, but the relation of these items to Britain, and also Australia, is very indirect and convoluted.

Part IV (chapters 19–21), 'English in Asia', consisting of three pieces by Hickey ('Englishes in Asia and Africa: origin and structure', 'South Asian Englishes' and 'South-east Asian Englishes'), announces its exclusive concern with varieties that emerge from 'non-continuous' contact with native speakers, i.e. 'new Englishes' or 'World Englishes'. These sit uneasily in other parts of the book: Lass explicitly deals with the South African English spoken by settlers and their descendants, but part III also contains some attempts to treat Aboriginal Englishes and Maori English. I have applauded Hickey for not excluding varieties on the basis of distinctions between native and non-native varieties, contact varieties and creoles, not least because so little is known about the historical development of 'World Englishes'. However, the extant scholarship of these varieties is markedly different from the research tradition of North American and Australasian varieties: it is about synchronic description rather than diachronic emergence. Given that the volume does not offer new historically informed scholarship in the area, all Hickey can do is summarise this wealth of description. Apart from an updated bibliography, we are left with something that does little more than, say, Kachru (1994) for Indian English or Platt, Weber & Ho (1984) for Asian and African Englishes. The material that should be in a volume like this has yet to be published. We are told that educational systems have been instrumental in the emergence of these Asian English varieties, yet the past and present form of these systems is poorly understood. Regional differences do not receive the level of attention that they do for other ETEs. Many Asian countries are experiencing a tension between the traditionally British orientation of their standard forms and their speakers' current orientation to American norms.

The book's appendix, containing a 'Checklist of non-standard features', a timeline and a set of maps, all compiled by Hickey, reinforces the marketing of this book as a reference volume. But does Cambridge University Press need to publish reference volumes on ETEs beyond the dedicated volumes of the *Cambridge History of the English Language* (cf. Burchfield 1994 and Algeo 2001)? Extracting a sense of the current state of the field from this book is a painful process, although some dedicated ploughing at least guarantees exposure to a very diverse range of reconstructive methodologies. A slimmer volume showcasing some of the state-of-the-art might have been more effective.

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(Received 5 November 2005)

J. Linguistics 42 (2006). doi:10.1017/S0022226706284093
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Sharon Inkelas & Cheryl Zoll, *Reduplication: doubling in morphology*. Cambridge: Cambridge University Press, 2005. Pp. x + 254.

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Reduplication is universally understood as the repetition of segmental material in some particular morphological environment. Sharon Inkelas & Cheryl Zoll's book, *Reduplication: doubling in morphology*, argues for a view of reduplication as a purely morphological process, which is a radical departure from past and recent analyses of reduplication. For Inkelas & Zoll (henceforth I&Z), reduplication results from the repetition of abstract morphemes rather than the phonological copying of a string of segments.

An immediate consequence of the adoption of a morpheme-based view of reduplication is the prediction that there should be strong evidence for morphological constituency within reduplication. While total reduplication, which appears to be the most common pattern of reduplication (Rubino 2005), provides evidence for morphological constituency, partial reduplication patterns, which abound cross-linguistically, argue against morphological (and phonological) constituency playing an important role in reduplication. Thus, partial reduplication may undercopy a morphological constituent (as seen in (1)–(2)) or copy across morphological constituents (as seen in (3)–(4)).

Undercopying in a morphological constituent

- (1) Ilokano (Hayes & Abad 1989: 357), light syllable prefixing
- | | | |
|-----|----------------|-----------------------------|
| (a) | liŋpét | si-li- liŋpét |
| | ‘perspiration’ | ‘covered with perspiration’ |
| (b) | ɟyáket | si-ɟyá- ɟyáket |
| | ‘jacket’ | ‘wearing a jacket’ |
- (2) Temiar (Benjamin 1976: 169), C(C) reduplication
- | | | |
|-----|-------------------------|---------------------------|
| (a) | kōw | kw- kōw |
| | call.ACTIVE PERFECTIVE | call.ACTIVE CONTINUATIVE |
| (b) | slɔg | s-g-lɔg |
| | sleep.ACTIVE PERFECTIVE | sleep.ACTIVE CONTINUATIVE |

Copying across morphological constituents

- (3) Arrernte (Breen & Pensalfini 1999: 6), habitative reduplication
- | | | |
|-----|--------------------|-----------------------------|
| (a) | at ^w er | at ^w er-er-er-er |
| | ‘fight’ | ‘weapon’ |
| (b) | eɬ | eɬ-er-eɬ-er |
| | ‘poke’ | ‘instrument for poking’ |
- (4) English (Alderete et al. 1999: 355), *shm*-reduplication
- | | | |
|-----|-------------|----------------------------|
| (a) | table | table-shm-able |
| (b) | resolutions | resolutions-shm-esolutions |

The data in (1) show a common CV prefixing pattern, while (2) illustrate the widespread C(C) pattern of reduplication. One immediate conclusion that can be drawn from the examples in (1)–(2) is that it is not unusual to reduplicate less than a morphological constituent. A second conclusion is that the phonological constituency of the non-reduplicated form is also commonly deviated from.

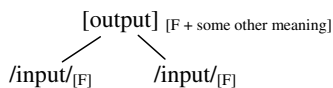
The data in (3)–(4) show cases where the segmental material that occurs in reduplication is taken from two distinct morphemes. Example (3) shows a case where part of a root and suffix are used for reduplication, and (4) shows a prespecified consonant cluster using the entire root except for the first consonant for reduplication. The conclusion from these observations

is that reduplication commonly uses phonological material from more than a single morpheme in a way that does not respect morphological constituency.

Under a morpheme-based view of reduplication, the data in (1)–(4) are problematic and require additional mechanisms to derive the observed partial reduplication patterns. Note that phonological copying, which is assumed by almost all other models of reduplication, does not have this liability because there is no claim made about morphological constituency effects. Part of the phonological copying approach is the assumption that the region of copying must be independently specified in some manner. This is not an addition or modification to the phonological copying approach, but a core aspect of the approach, which enables it to capture the data in (1)–(4) without further stipulation. The important question is whether the additional mechanisms required in a morpheme-based view of reduplication can be justified by the fact that a morpheme-based view adds something to our understanding of reduplication which is not provided by a phonological copying approach. To pursue an answer to this question, let us consider the details of I&Z’s theory.

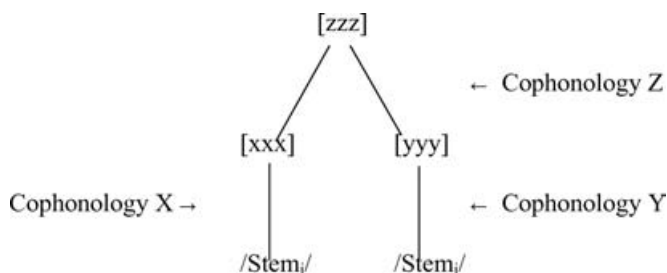
I&Z propose the morpheme-based Morphological Doubling Theory (MDT) to account for reduplication. The main claim of MDT is that reduplication results from morpho-semantic doubling of morphemes, as illustrated in (5), where [F] = semantic feature bundle.

(5) *Morpho-semantic doubling* (7)



The claim of the model in (5) is that within the morphological component, there are structures which call for inputs that are identical only in the make-up of semantic feature bundles. In other words, abstract morphemes are repeated in the morpho-semantic structure, which, in the most common cases, produces repetition of phonological material because the same stem is inserted for the identical abstract morphemes. Note that the morphological doubling mechanism proposed by I&Z is able to double more than a single morpheme because doubling targets a node in the morpho-semantic structure and will thus result in the repetition of any dependents of this node.

Given the structure in (5), MDT can only produce reduplication patterns that show morphological constituency effects, which is obviously inadequate considering the data in (1)–(4). I&Z thus propose that there is an additional morpho-phonological level of structure in reduplicated constructions, which is presented in (6).

(6) *Morpho-phonology of MDT* (19)

The crucial feature of the morpho-phonological structure in (6) is the use of specific cophonologies or sets of ranked constraints, which are associated with each node and characterized in the framework of Optimality Theory (OT). These OT cophonologies are posited to account for morpheme- and reduplication-specific phonological behavior. One of MDT's main claims is that the two levels of structure provided by (5) and (6) (or, in I&Z's terms, 'the morphology-phonology layering') are sufficient for capturing the interaction of phonology and reduplication. Thus, I&Z state that '[o]nce the role of morphology-phonology layering is recognized in reduplication, the need for special mechanisms like B[ase]R[eduplicant]-Faith vanishes' (173). The authors provide general arguments in support of cophonologies based on morpheme-specific phonological processes. If the content of the cophonologies themselves were sufficient to account for reduplication data, then we could indeed conclude that the morpheme-based view is vindicated.

There are two relevant issues in the evaluation of whether MDT is an adequate model of reduplication. The first is anomalous rule interaction (e.g. underapplication, overapplication and backcopying), which is a well-known phenomenon (first demonstrated by Wilbur 1973) and which provides the basic argument for McCarthy & Prince's (1995) Correspondence Theory model of reduplication. The second issue is how reduplication patterns which do not follow from morphological constituency (such as those in (1)–(4)) are to be accounted for. Fortunately, I&Z acknowledge the importance of explaining these phenomena in reduplication and devote much of the book to analyses which address these issues.

For I&Z, the source of anomalous rule interaction with reduplication is opacity – that is, the environment that triggers or blocks the application of a rule is not surface-apparent. An example of an opacity effect is provided by Javanese (136–150).

(7) /a/-raising in Javanese (/medja/ is the underlying form)

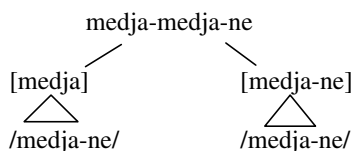
- (a) medjɔ /a/-raising occurs
 'table'
- (b) medja-ne /a/-raising blocked by suffix
 'his/her table'

- (c) medjɔ-medjɔ /a/-raising occurs in reduplication
'tables'
- (d) medja-medja-ne /a/-raising underapplies
'his/her tables'

While /a/ raises to [ɔ] if it is the stem-final vowel (as seen in (7a)), suffixation bleeds the application of /a/-raising (as seen in (7b)). The reduplicated form in (7c) also shows normal application of /a/-raising on the assumption that both repeated regions are treated as separate stems. However, the reduplicated form in (7d) shows an opaque underapplication of /a/-raising. Unlike the second stem, the first stem in the reduplicated form is not suffixed at the surface, so we should expect /a/-raising to occur (producing *[medjɔ-medja-ne]), but it does not.

I&Z argue that the underapplication of /a/-raising in forms like (7d) is due to the fact that the first repeated stem is actually suffixed, i.e. the reduplicated form has the morpho-phonological structure in (8).

(8) *Opacity by truncation* (138)



Accordingly, /a/-raising is bled by suffixation in both stems. The specific cophonologies for each stem contain the same /a/-raising process, but they differ in that only the cophonology for the first stem contains a truncation process which deletes the suffix after /a/-raising is blocked.

The strong connection that I&Z make between opacity and reduplication raises questions about the OT implementation of MDT and the handling of opacity effects in OT. MDT makes available a morpho-phonological layering of cophonologies, which will provide a limited derivational mechanism to account for some types of opacity. If opacity in reduplication occurred only across different cophonologies but not within a single cophonology, it could be reasonably concluded that level ordering is the only mechanism necessary to account for opacity in reduplication. This would be a pleasant surprise, considering the present view (Odden, to appear) that in addition to level ordering, multiple distinct theoretical devices, such as two-level constraint evaluation, conjoined constraints, output-output correspondence and Sympathy Theory, are required to account for opacity effects in OT.

But even MDT does not maintain that morpho-phonological layering (level ordering) is sufficient to account for opaque effects in reduplication. This point can be demonstrated by considering the cophonology for the first stem in the Javanese example (7d)/(8). There is opacity within this

cophonology in that the output of the cophonology does not contain the necessary environment to block /a/-raising. Consider the simplified tableau in (9).

(9) *Cophonology for first stem in (7d)/(8)*

	/medja-ne/	/a/-raising	Truncation	Faith
a.	medja-ne		*!	
b. !!!	medjə			*
c.	medja	*!		*

From general ranking logic, the constraints that cause /a/-raising and truncation of the suffix must both be ranked above the appropriate Faithfulness constraints within this particular cophonology. (9a) is ruled out because it does not truncate the suffix, and (9c) is ruled out because it has a violation of /a/-raising. The problem in (9) is that candidate (9b), which shows /a/-raising, is the most harmonic form because it satisfies both the /a/-raising and truncation constraints.

While this is not an empirical problem in that, amongst others, the devices of two-level constraint evaluation or Sympathy could be invoked (see Odden, to appear) to ensure that candidate (9c) is the most harmonic, it shows that level ordering and cophonologies are not sufficient in themselves to account for phonology reduplication interaction which involves truncation. Truncation produces a non-surface-apparent generalization which requires another theoretical device to be invoked in order for an OT cophonology to account for the data.

I&Z also use truncation to account for reduplication patterns that do not follow from morphological constituency. Reduplication patterns such as those presented in (1)–(4) are said to require the truncation of part of one (or possibly both) of the stems. The authors defend this truncation approach by providing examples of truncation outside of reduplication (91), but they do not address the question of whether all of the types of truncation that are required to account for reduplicative templates can be found outside of reduplication. If we consider the non-reduplicative truncation examples offered by I&Z (and given on the two websites listed as sources for some of these forms (91 fn. 14)), there appears to be a mismatch between non-reduplicative and reduplicative truncations. Only the truncation patterns for light syllable prefixing (cf. (1)) have attested non-reduplicative forms. All other patterns, i.e. C(C) reduplication (cf. (2) above), VC(C) reduplication and the copying across morphological constituents (cf. (3)–(4) above), do not appear to be attested outside of reduplication; and my intuition is that these are not accidental gaps. The conclusion for MDT is that there are ‘reduplication-specific’ truncations. This weakens any claim made by I&Z that MDT is a better theory of reduplication than others because it eliminates any reduplication-specific device, such as the Base Reduplicant-Faith

(BR-Faith) constraint. Although MDT does not use BR-Faith, it must invoke special reduplication-specific cophologies which will produce the necessary truncation patterns in order for MDT to be empirically adequate.

To summarize, MDT is an empirically adequate morpheme-based theory of reduplication. The key to MDT's empirical success is the positing of truncation processes within morpheme-specific cophologies. Truncations in specific cophologies provide both the opacity effects that underlie the anomalous rule interaction with reduplication, and the reduplication patterns that do not follow from morphological constituency. Unfortunately, the truncation approach to both of these core aspects of reduplication does not add any insight into the nature of reduplication. Since the cophologies that produce the truncation effects are based in OT, insights provided by MDT into reduplication are limited to the insights into opacity provided by OT. The MDT analysis also raises the question of why certain truncations are found only in reduplication. There does not appear to be any fruitful answer here because MDT claims that there is nothing special about reduplication and asserts that any phenomena found in reduplication should also be found outside of reduplication, which appears to be false.

We could possibly overlook these conceptual issues in MDT if there were no other viable models of reduplication. I&Z's arguments against Base-Reduplicant Faithfulness models of reduplication are carefully thought out, and it would be difficult to defend such models. However, the authors neglect to consider derivational models of reduplication, which is a considerable weakness in their argumentation for MDT. While recent proposals for a purely derivational theory of reduplication, such as Raimy (2000a, b), share the view with MDT that there is abundant opacity in reduplication, opacity is clearly accounted for and predicted by a derivational analysis – in contrast to the MDT approach, which suffers from the well-documented issues that OT has with accounting for opacity effects. The importance of the intracophonology opacity of the truncation and /a/-raising interaction cannot be understated because it calls into question I&Z's claim that 'MDT not only describes this opacity [Javanese examples, ER] effortlessly; it correctly predicts whether the opacity will take the form of underapplication or overapplication' (149). Yes, MDT describes the opacity well; but the OT cophology does not account for it 'effortlessly' and does not make the correct predictions. As discussed above, a tableau like (9), which does not invoke any of the additional mechanisms required by OT to handle opacity, gives rise to the incorrect prediction that there should be normal application of /a/-raising in Javanese.

The strong claim made by I&Z that the fundamental basis of reduplication is a morpho-semantic operation predicts that there should be semantic neighborhood speech errors found in reduplication. A potential example of this, which would offer evidence in favor of MDT, would involve slips of the tongue like *doctor-nurse* or *nurse-doctor* for the English contrastive

reduplicated form *doctor-doctor*. A fundamental question that is not fully pursued by I&Z is whether morpho-semantic copying in reduplication is the same as or different from syntactic copying. This omission may be due to the fact that the MDT is presented within the morphological framework of Sign Based Morphology, which I&Z claim 'is compatible both with item-based and with realizational morphology ... with Optimality Theory and rule-based theories of phonology' (7). While this view of morphology allows for easier exposition of MDT, I would be very surprised if I&Z's assumptions about the nature of morphology did not have any effect on the workability of MDT in general.

To conclude this review, let me point out that there are many places where I agree with the proposals made by I&Z. Many of their arguments against the use of BR-Faith by Correspondence Theory analyses of reduplication should be noted and carefully considered; and I completely concur with I&Z's arguments that models of reduplication that utilize 'coerced identity' (e.g. Wilbur 1973, Correspondence Theory) are unnecessary and unjustified. An important contribution of this book is I&Z's identification of the role of opacity in reduplicated forms. As with other areas of phonology, the understanding of opacity is crucially important. The opaque interactions found in reduplication provide another avenue to investigate the nature of opacity. Finally, I&Z should also be applauded for their discussion and analysis of both classic and less well-known reduplication patterns. Calling the field's attention to some of these sources of data and the importance of specific patterns is invaluable.

The question that must be considered by the reader in evaluating MDT is whether the fact that additional analytical devices are required to account for opacity in reduplication constitutes 'an unconscionable opening of the theoretical floodgates, or is simply a minor but necessary adjustment to the theory' (Odden, to appear, page 2). If I&Z are correct in their claim that there is a strong connection between reduplication and opacity, derivational analyses of reduplication, which do not have to grapple with this question, may lead to a new understanding of reduplication.

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(Received 31 October 2005)

J. Linguistics 42 (2006). doi:10.1017/S002222670629409X

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Sverker Johansson, *Origins of language: constraints on hypotheses* (Converging Evidence in Language and Communication Research 5). Amsterdam & Philadelphia: John Benjamins, 2005. Pp. xi + 345.

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I hadn't intended to read this book quite so soon. I picked it up and read the first few pages over a sandwich one lunchtime and just couldn't put it down. So it might have the odd hummus stain on it now, but I'll still be referring to it for many years to come. Despite the somewhat unpromising subtitle, this is no dry tome, but instead a brilliantly readable, amazingly succinct yet comprehensive overview of what is scientifically plausible – and utterly implausible – in terms of hypotheses about the evolution of language. Who is it for? Well, it's quite likely to be for you, especially if you've wondered about the burgeoning field of evolutionary linguistics, but haven't been following the plotlines over the past ten years or so. It could easily be used as a textbook for a course in language evolution (in fact, the author intends to do just that himself) and would in many respects make a better text than my current choice, the collection of papers in Christiansen & Kirby (2003).

Generally, one could caricature the situation in language evolution studies as follows: linguists make hypotheses about evolution which might solve their linguistic problems, but are absolutely impossible in evolutionary terms; everyone else makes hypotheses about language evolution which may (or may not) be feasible from an evolutionary perspective, but which leave just about everything linguistic still to be accounted for. Johansson is exactly what both sides need: a fully-paid-up scientist (an academic physicist) who's also got two degrees in linguistics (and in fact a few more in some unrelated fields). The book, then, is 'mainly aimed at linguists' (x) and presents 'in a coherent manner the relevant evidence and theories from all the disciplines

involved, with ample references to primary sources' (x). In all this, it succeeds admirably.

The book consists of a short introduction (chapter 1) followed by ten main chapters and a short conclusion (chapter 12). Almost a quarter of the total number of pages is devoted to those 'ample references' – what's more, the author has actually read most of them; we can tell where he hasn't, because he sweetly notes that such-and-such is 'cited in' something else in such cases. Each chapter ends with a wonderfully useful summary of the main points and has a short list of further readings. The index is disappointing, though, and could certainly be fuller.

The first eight chapters are explicitly presented as background material, that is, all we need to know in order to be able to evaluate hypotheses about language evolution. So, a short chapter 2 asks 'What is language?' and looks at its main properties – in other words, what we need to account for. Chapter 3, 'The theory of evolution', forms a really nice introduction to the topic, and reminds us of some important features, such as the fact that evolution doesn't plan ahead, that it can't work for 'the good of the species', and that there's no ultimate goal. Of course, most non-biologists know these things, but that doesn't stop them violating some basic scientific principles in their writings. Chapter 4, 'Human origins and evolution', is an excellent survey of the main facts (and controversies), steering a clear path through the minefield of paleoanthropological disagreements. Chapter 5, 'Anatomical and neurological prerequisites', tells us what had to evolve in terms of equipment for speech production and perception, and then dives fearlessly into the human brain, where, as the author notes, 'causal link[s] between language and brain growth have been proposed in both directions' (95). Next come two (thankfully) short chapters: chapter 6, 'Animal communication in the wild', and chapter 7, 'Can non-humans be taught language?' (more below on why I was especially grateful for this chapter to be quite short). Chapter 8, 'Language, mind, and self', again a short chapter, is on the difficult topic of the mind.

With the background out of the way, we come to the real heart of the book. Chapter 9, 'Hypotheses of language origins', concentrates on a number of dimensions concerning language evolution (162f.): adaptation vs. exaptation (the 'hijacking' of a feature for a different usage than the one that it evolved for) or spandrel (an accidental by-product); early in human pre-history or recent evolution (i.e. within the past 100,000 years); gradual evolution vs. sudden saltation; speech first vs. gestures first; and finally, 'innate and genetically determined vs. learned and culturally determined' (163). Here, some important points are presented. Johansson argues that, since various adaptations for speech are found in Neanderthals, we can infer that they are present in the common ancestor to homo sapiens and Neanderthals, some half a million years ago. Although we can't know whether full language was present at this time, it is certain that some form of protolanguage must

have existed. And gradual evolution is the only plausible kind, given ‘the poverty of the genes’: our TOTAL number of genes is no more than 25,000, and there’s no way that a large number of these suddenly jumped into place to form language out of thin air.

Chapter 10, ‘Why did language evolve?’, crucially reminds linguists that the evolution of complex features (such as language) must have been driven by selective pressures, and (again) that we must avoid teleology – language is damn useful, but that can’t be why it evolved. Here, Johansson carefully evaluates the major proposals, which link language to hunting, tool making, sexual selection, teaching children, and (last but not least) social relations. He concludes, reasonably in my view, that ‘[a]ll in all, the basic socio-political hypothesis appears quite plausible as a basis for the selective advantage of language’ (214).

The final main chapter is chapter 11, ‘Proto-language’. Given that language must have evolved via a number of intermediate stages, we need to know what was likely to have happened in the earliest stages, and what the intervening stages looked like before full language emerged. I did wish this chapter had been longer, because this is the stuff that linguists are probably most interested in, and also what we’re most likely to want our students to know about. However, the relative weighting is deliberate. Johansson states:

I have chosen to place less emphasis ... on evidence from linguistics proper, and more on fields with which the linguist may be less familiar, notably evolutionary biology, primatology, and paleoanthropology. (x)

The whole book is as scientifically rigorous as anyone could wish for, yet remains compelling reading. By now it will also be clear that the chapters have properly useful titles, rather than the hideously twee chapter headings of the pop-sci world, which, for some reason, are pervasive in books on language evolution. (Why? Do they think that being serious will scare readers off?) Johansson’s dry wit often unobtrusively raises a smile, for instance, in a footnote on page 131, where he remarks (accurately!) that the chimpanzee Nim Chimpsky is ‘distantly related to a certain famous linguist’.

Note that quite intentionally, this book doesn’t offer anything really novel (not unlike evolution itself!). Johansson doesn’t treat us to any pet theories of his own, such as language evolving in order to replace grooming, or language evolving first as gesture, or as mime, or from aquatic apes (though he does discuss all these and more). This is a bonus, not a deficit. I think Johansson would agree with me that we don’t actually need anything radically new in order to have a plausible picture of language evolution. What we do need is a way of pulling together the best from the totality of facts and good hypotheses which we already have from all the relevant disciplines. And broadly, this is what we get here. However, the author isn’t shy about

making his views clear when he feels strongly. For instance, he is very critical of Lightfoot's view that language is not an adaptation (165), and he swiftly dismisses the idea (234) that protolanguage might have been 'holistic', i.e. that its earliest utterances were unanalysable wholes, from which words were subsequently 'segmented' (a poor terminological choice, but not Johansson's).

So do I have any quibbles, and what would I change? I did feel it was a shame not to have more on the linguistic arguments. As noted, chapter 11 is the major locus of these, and Johansson does pack a fair bit in, but it would be good to spend more time unpicking hypotheses concerning the actual form of protolanguage at various stages.

Most of my disagreements with Johansson came in chapter 7, which discusses the issue of whether other species can be taught language. In my view, much of this debate is a non-issue for studies of language evolution, though it may indeed help us to figure out what aspects of language are phylogenetically ancient in the hominid line: if chimpanzees have some feature, it's likely to predate the split in our ancestry, which occurred five to six million years ago; but if parrots have the same feature, it's likely to be a case of convergent evolution. However, Johansson is surprisingly uncritical throughout the book when it comes to ape language experiments. Despite it being well known that Washoe was actually not taught (and did not learn) anything like genuine American Sign Language, he comments (130) that 'Washoe was taught sign language from an early age' and that Washoe's son 'appeared to acquire sign language from her'. But learning a few signs (or something approaching signs) is not at all the same thing as learning a true, signed language. Johansson also suggests that chimpanzees 'can be taught rudimentary syntax' (240), and again I would counter that learning or making up a few basic ordering principles does not equate to rudimentary syntax. On page 134, he notes that the bonobo Kanzi responds correctly to commands like 'Go get the carrot that's in the microwave', and thus must be successfully parsing a recursive structure. This is fallacious: all Kanzi needs to understand is 'get carrot microwave' – which doubtless shows that Kanzi is one smart critter, but certainly doesn't prove a comprehension of relative clauses (see also Kirby 2000). Furthermore, despite supporting the idea that a non-human can possess the recursion feature, Johansson (235, 242) actually seems to defend the highly contentious claim by Hauser, Chomsky & Fitch (2002) that recursion is the *ONLY* component of the language faculty which is 'narrowly language-specific'. One can't back both of these two horses at the same time.

But I don't want to end on a negative note. It's amazing to find any book or paper on language evolution which contains only a few matters that one disagrees with, and it's just about unique to find that (almost) an entire book is actually sensible. So note the title: *Origins of language*. And there's your bedtime reading sorted.

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J. Linguistics **42** (2006). doi:10.1017/S0022226706304094
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Christopher Potts, *The logic of conventional implicatures* (Oxford Studies in Theoretical Linguistics 7). Oxford: Oxford University Press, 2005. Pp. xii + 246.

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Paul Grice warned that ‘the nature of conventional implicature needs to be examined before any free use of it, for explanatory purposes, can be indulged in’ (1978/1989: 46). Christopher Potts heeds this warning, brilliantly and boldly. Starting with a definition drawn from Grice’s few brief remarks on the subject, he distinguishes conventional implicature from other phenomena with which it might be confused, identifies a variety of common but little-studied kinds of expressions that give rise to it, and develops a formal, multi-dimensional semantic framework for systematically capturing its distinctive character. The book is a virtuosic blend of astute descriptive observations and technically sophisticated formulations. Fortunately for the technically unsophisticated reader, the descriptive observations can be appreciated on their own.

Here is a quick summary of the book. Following a brief introductory chapter (so titled), chapter 2 makes ‘A preliminary case for conventional implicatures’ by offering a four-part definition, distinguishing conventional implicature (CI) from conversational implicature and presupposition, identifying the main kinds of linguistic phenomena that fit this definition, and motivating the book’s distinctive multidimensional semantic framework. Chapter 3, ‘A logic for conventional implicatures’, develops a rigorous ‘description logic’ for representing CI meanings along with ‘at-issue’ meanings. The next two chapters illustrate and discuss the two main kinds of expressions with CI meanings. Chapter 4 focuses on ‘Supplements’, including nonrestrictive relative clauses, *as*-parentheticals, Noun Phrase appositives, and several sorts of adverbials; and chapter 5 considers ‘Expressive content’, including expressive attributive adjectives, epithets, and Japanese honorifics.

Chapter 6, ‘The supplement relation: a syntactic alternative’, compares Potts’s approach to supplements with an alternative syntactic approach, and argues that there is no need to complicate the syntax in order to distinguish supplement structures from coordination – the difference can be captured with multidimensional semantics. The seventh and last chapter, ‘A look outside Grice’s definition’, briefly considers what sorts of linguistic phenomena do, or in principle could, arise when one or another of the four conditions in the definition of conventional implicature is not satisfied. The most important case of this involves words like *although*, *but*, *therefore*, and *yet* – words of the very sort that traditionally, thanks to Grice (1975/1989) and originally to Frege (1892/1997, 1918/1997), have been thought to trigger conventional implicatures (although not by Potts).

Potts extracts a four-part definition from several brief remarks made by Grice in the course of distinguishing conversational from conventional implicature. Two of Potts’s four conditions are straightforward and closely connected: conventional implicatures arise from conventional meanings of words (hence are not ‘calculable’ from conversational maxims), and they are not cancelable. Moreover, conventional implicatures are SPEAKER-ORIENTED commitments and, unlike presuppositions, are ‘logically and compositionally INDEPENDENT of what is said (in [Grice’s] favored sense), i.e., independent of the at-issue entailments’ (11). These last two features of conventional implicatures, speaker orientation and independence, are the more interesting and controversial ones (note that Potts’s independence condition replaces Grice’s stronger condition (1975/1989: 25) that the falsity of a conventional implicature does not affect the truth of the entire utterance).

The case of supplements clearly illustrates these two features. Supplements are so called because they do not affect the content or truth-value of the material with which they combine. Syntactically, they belong to the same tree structure, but they do not have semantic effects on the proposition expressed by the main clause. They include nonrestrictive relative clauses, *as*-parentheticals, and appositives, as illustrated in (1)–(3).

- (1) Condi, who used to be provost at Stanford, is implacable.
- (2) Cheney is, as Maureen Dowd has dubbed him, the Grim Peeper.
- (3) Libby, the former aide to Cheney, can’t be compared to Liddy.

Each of these sentences expresses two propositions, not one conjunctive proposition. For example, (1) expresses both the ‘at-issue’ proposition that Condi is implacable and the supplementary proposition that she used to be provost at Stanford. Calling the first one ‘at-issue’ is a bit misleading, since it is easy to imagine cases in which the content of the supplement, such as a nonrestrictive relative clause, is more controversial than that of the main clause. So Potts could just as well have called the proposition that Condi is implacable the ‘main’ proposition, meaning merely that it is the content of the main clause.

Supplements are used to make ‘speaker-oriented comments on a semantic core’ (II), and the notational devices that Potts introduces in chapter 3 formally characterize how applying a supplement to material in the main clause yields a supplementary proposition. His apparatus captures the fact that the truth-values of the main and the supplementary propositions are independent of each other. This is unlike the relationship between a classical semantic presupposition and the main proposition expressed by a sentence, whose truth or falsity depends on the truth of the presupposition.

Now, in what way is the supplementary proposition a SPEAKER-ORIENTED commitment? After all, a speaker who assertively utters (1), for example, commits himself to the truth of both propositions. The feature of being speaker-oriented emerges when the sentence is embedded in an indirect speech report, as in (4).

(4) John said that Condi, who used to be provost at Stanford, is implacable.

The content of the supplement in (4) is understood as the speaker’s own comment, not as part of what he is saying John said. Note, however, that this content can be attributed by including an explicit indication:

(5) John said that Condi, who, he said, used to be provost at Stanford, is implacable.

But this just highlights the fact that supplements are semantically independent. In this respect, supplements differ from expressions that have been traditionally regarded as sources of conventional implicature, viz. words like *although*, *but*, *therefore*, and *yet*. For example, in the reporting of (6) with (7), the additional proposition generated by *but* can be part of what John is said to have said rather than the content of the reporter’s side comment.

(6) John: Condi is smart but conservative.

(7) John said that Condi is smart but conservative.

This was one of the factors that led me to conclude that conventional implicature is a myth (Bach 1999). The propositions yielded by *but* and the other traditional candidates, as well as those yielded by supplements, are as much asserted as propositions expressed by main clauses.

This raises the question of whether being speaker-oriented makes the contents of supplements merely implicated. That is, even if they can’t be embedded under *say*, aren’t they still asserted? Indeed, Potts acknowledges that the presence of IMPLICATURE in the term CONVENTIONAL IMPLICATURE is ‘unfortunate’ (9), presumably because the difference between asserting and implicating is beside the point. The point is that sentences can express more than one proposition (not to be confused with expressing a conjunctive proposition), and that in this way the content of such a sentence is multi-dimensional, one dimension of which is speaker-oriented, in the way described above.

Utterance modifiers, such as *frankly*, *by the way*, and *in case you're interested*, are not on a par with other supplements. They are not used to make side comments on any part of the content of the main clause (Potts's 'semantic core'); rather, they are used to comment on some aspect of one's act of uttering the main clause (for explanation and illustration see Bach 1999: section 5).

After explaining how supplements work, Potts turns to words with expressive contents. Here he includes epithets, certain attributive adjectives, and honorifics. One interesting fact about these is that they generally are not used to distinguish one thing from another, for example, that jerk Jones as opposed to some other Jones. Indeed, as Potts observes, many expressive adjectives cannot occur in predicative position (168). For example, we can say *that damn Kaplan* but not *Kaplan is damn*. And those adjectives that can occur as predicates are then not used expressively – compare *that dirty bastard* with *that bastard is dirty*. Potts makes many other astute observations about the behavior of expressives of different sorts. These observations illustrate how they are speaker-oriented; but, as I will suggest, Potts does not pinpoint the fact that expressives are speaker-oriented in a more radical way than supplements.

Supplements are speaker-oriented in that, when occurring in indirect quotation (or in propositional attitude reports), they are not part of what is reported as said (or believed, etc.) but are used to add the reporter's side comment. But there is nothing speaker-oriented about the CONTENT of a supplement. The proposition that Condi used to be provost at Stanford is something that the speaker, his audience, and anyone else can entertain and believe. Expressives are speaker-oriented in a more radical way, a way that has consequences for Potts's suggestion that they give rise to conventional implicatures, even in his revisionary sense of the term.

This is clearest with expressive adjectives. These are speaker-oriented at least in the way that supplements are. Suppose Sam utters (8) and his utterance is reported with (9).

- (8) Stan won't turn off his damn radio.
 (9) Sam says that Stan won't turn off his damn radio.

The reporter's use of the expressive adjective *damn* cannot be taken as giving part of what Sam said (unless, as we will see below, it is taken as directly quoted). The reporter is expressing his own feeling about Stan's radio. But expressives are speaker-oriented in a further way: they are not used to express sharable content. Being speaker-oriented in this stronger way, they are not vehicles for conventionally implicating anything, even in Potts's sense. Expressing a feeling is not a kind of implicating.

To implicate something entails meaning it, that is, intending to convey it to one's audience. Presumably what is meant is a proposition, something that anybody can entertain and believe. But what is meant when one uses an

expressive adjective? If I say, *That blasted TV isn't working*, what do I mean in addition to the proposition that the TV is not working? Is it something that my audience can agree or disagree with? I do not see that it is, and Potts agrees (157). But if that is right, then I do not mean ANYTHING in using *blasted*, although I certainly express a certain negative feeling toward my TV. Although my audience can recognize that I am expressing this feeling, in using *blasted* I do not MEAN that I have this feeling. I am expressing that feeling, not implicating it.

It is true that I am making a commitment – to actually having the feeling that I am expressing – but what is speaker-oriented is not my commitment but my feeling, which, in the relevant sense of *express*, only I can express. If you say, *That blasted TV isn't working* and I report you as having said that your blasted TV isn't working, I am not reporting you as having cursed it – I am cursing it myself. So it is no surprise that when one includes an epithet in an indirect report, the only way it can be understood as attributed rather than used is if it is taken as directly quoted. Potts recognizes this fact (154) but overlooks part of the reason for it: part of the import of curse words and other epithets is the word itself, not just its content. Indeed, its metalinguistic character is suggested by the fact that we describe the use of such terms as 'name-calling'.

I have been suggesting that expressives do not give rise to implicatures, even in Potts's sense of the term. The specific case of nominal epithets poses a further problem. As Larry Horn has observed (p.c.), they seem to be counter-examples to a core thesis of Potts's that '[n]o lexical item contributes both an at-issue and a CI meaning' (7). This thesis, which is more stringent than the independence condition, is built into Potts's multidimensional framework. However, it seems that the epithets in (10) and (11) play a dual role, both referential and expressive, in contrast to expressive adjectives like *damn* and *blasted*.

(10) I wouldn't hire that shyster.

(11) That scumbag never returned my CDs.

It is worth noting that the same problem arises with *tu* vs. *vous* in French (and *du* vs. *Sie* in German) and with Frege's example of *horse*, *steed*, and *nag*. These also illustrate Frege's notion of 'coloring' and his idea that 'the content of a sentence often goes beyond the thought expressed by an utterance of it' (1918/1997: 331).

Quibbles aside, this is, as I implicated at the outset, a damn good book. Read it!

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(Received 5 February 2006)

J. Linguistics 42 (2006). doi:10.1017/S0022226706314090
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Bernhard Wälchli, *Co-compounds and natural coordination*. Oxford: Oxford University Press, 2005. Pp. xviii + 334.

Reviewed by LAURIE BAUER, Victoria University of Wellington

Dvandva compounds have largely been ignored in morphological theory, although what precisely a dvandva is seems to have been subject to a certain amount of equivocation in the literature. It is thus timely to have a book that considers them in great detail – renaming them ‘co-compounds’ in the process. The two great strengths of this particular study are that it goes far beyond seeing co-compounds simply as a matter of a particular morphological pattern, and that it pays ample attention to the use of co-compounds in texts.

Chapter 1, ‘Introduction’, introduces the terms ‘co-compound’ and ‘natural coordination’. A co-compound is defined as a unit ‘consisting of two or more parts which express natural coordination’ (1), and natural coordination designates the linking of two elements which are expected to co-occur. Thus, *mother and father* is a case of natural coordination, while *mother and cat* is not. Natural coordination may be indicated by co-compounding, but may also be shown in other ways. For example, in English, natural coordination may involve two bare infinitives, as in *Kim was able to read and write*, but less natural coordination requires coordinating two *to*-infinitivals (*Kim was able to read and to swim*). Similarly, while we may say *My niece's brother and sister came too*, we would probably say *My niece's brother and her cat came too* (11–12).

Co-compounds can have a number of forms, ranging from pure juxtaposition of the two elements coordinated to overt inflection on one or both elements. They have a relatively restricted range of meanings, from which Wälchli specifically excludes examples like *south-west*, *poet-playwright*, *mother-child (relationship)*, *Austria-Hungary* and *whisky-soda* (7–8). Wälchli provides a brief overview of the literature on co-compounds, and argues that it is not possible to give a traditional typological description of co-compounds because (i) they are very restricted and not universal, (ii) they behave

differently in different communicative domains, (iii) the text-frequency of co-compounds is important in showing areal patterns, and (iv) co-compounds are often highly context-dependent. Although Wälchli does not say this, it seems likely that any traditional typological study would have to be a study of natural coordination rather than one of co-compounds.

Chapter 2, 'The marking patterns of natural coordination', begins with a detailed discussion of markedness, distinguishing carefully between various subtypes of markedness, including textual markedness. The rest of the chapter considers the ways in which coordination can be shown and how different types of coordination can be distinguished in the same language. Wälchli is also concerned with the iconicity of different marking patterns, showing how different coordination strategies make use of different principles of iconicity.

Chapter 3, 'Tight coordination', looks at formal and semantic patterns of coordination. In essence, tight coordination is distinguished from loose coordination by shorter coordinands that are placed closer together. Tight coordination may also be reflected in inflectional marking, so that $A Bx$ (where A and B are the coordinands and x is any inflectional marker) shows tighter coordination than, for example, Ax conjunction Bx (70). While Wälchli claims that it is not always possible to rank marking strategies in a tightest–loosest series, tight(er) coordination is often signalled differently from loose(r) coordination in individual languages. Semantically, Wälchli distinguishes a number of different patterns of coordination. For example, *John and Mary's children* is ambiguous between a reading in which the children belong to both of them and a reading in which each has their own; similarly, *a typologist, a mother of three children and an organist* (76) may refer to one, two or three people. Coordination marking in some languages, such as Navajo (75), distinguishes between some of these alternatives, often using a comitative marker to show the tighter link.

In chapter 4, 'Co-compounds as a lexical class type', Wälchli begins by reviewing the problems with the notion of word, which make it difficult to determine whether a co-compound is or is not a word. He introduces the interesting idea that co-compounds are not a grammatical but a lexical class (a notion which he spends some time developing) and suggests that as well as co-compounds, diminutives and middles are lexical classes. Wälchli provides a list of typical features of lexical classes (109f.), which includes having a discernible semantic core; allowing so-called 'heavy forms', i.e. periphrastic versions of morphologically more compact forms; and the existence of 'tantum forms', i.e. forms which are marked as belonging to the class even though there is no unmarked form (cf. pluralia tantum nouns in English like *scissors*). Elaborating on this idea, he proposes that lexical classes typically do not enter the collective lexicon of the speech community, but are used in individual interactions, where they may take on lexical features in the short term. Wälchli further suggests, originally and pertinently, that lexicalisation

SEMANTIC TYPE	EXAMPLE
Additive co-compound	Georgian <i>xel-p'exi</i> 'hand-foot'
Generalizing co-compound	Khalkhala <i>ödör šönö.güj</i> 'day night.without > day and night'
Collective co-compound	Chuvash <i>šet-šu</i> 'milk-butter > dairy products'
Synonymic co-compound	Uzbek <i>qadr-qimmat</i> 'value-dignity > dignity'
Ornamental co-compound	Erza Mordvin (epic) <i>vel'e-šado</i> 'village-hundred'
Imitative co-compound	Khasi <i>krpaat krpon</i> 'pray IMI[TATIVE] > worship'
Figurative co-compound	Vietnamese <i>giang hô</i> 'river lake > adventurous'
Alternative co-compound	Erza Mordvin <i>vest'-kavskt'</i> 'once-twice > once or twice'
Approximate co-compound	White Hmong <i>ob peb</i> 'two three > some'
Scalar co-compound	Old Uyghur <i>ulug.i kičig.i</i> 'big.its little.its > size'

Table 1
Semantic types of co-compounds

is made up of two distinct processes – demotivation and drift towards the permanent lexicon – and that the two need to be distinguished (115). This seems to me to be a major contribution toward the theory of lexicalisation, although it is tangential to the main descriptive focus of the book.

In chapter 5, 'A semantic classification of co-compounds', Wälchli sets up the classification illustrated in table 1 (Table 5.1, 138). On the one hand, there are facets of this classification that we might wish to query. Is an imitative co-compound, in which one of the elements is a meaningless item based on the phonology of the other item, really a compound at all if it is not made up of two independent lexemes? Are ornamental co-compounds, which are defined as those in which one of the elements is misleading (the example 'village-hundred' just means 'village'), really co-compounds since there is no clear coordination between the elements? Do we need a separate type of figurative compound, given that most words, whether compound or not, can be used figuratively? On the other hand, this is by far the best classification of co-compounds available in the literature, and it is one based on the close

SEMANTIC TYPE	EXAMPLE
Appositional compounds	French <i>wagon-restaurant</i> ‘carriage-restaurant > restaurant car/dining car’
Intermediate denoting compounds	English <i>blue-green</i>
Comparative compounds	French <i>chou-fleur</i> ‘cabbage-flower > cauliflower’
Ideophone compounds	English <i>ding-dong</i>
Reduplication	Mordvin <i>kolmo.ń -kolmo.ń</i> ‘three.GEN-three.GEN > three each’
Echo words	Kannada <i>hallu-gillu</i> ‘teeth and the like’
Affirmative-negative compounds	Latin <i>volens-nolens</i>

Table 2
Semantic types which do not constitute co-compounds

analysis of a large number of texts from a large number of languages (see further below).

Wälchli also provides a list of constructions which are not co-compounds, though they may share some features with them. These are summarised in table 2 (based on pages 161–170). Here, it is easier to agree with Wälchli’s categorisation, notwithstanding the fact that compounds of these types are often included as *dvandvas*. However, it is surprising that the list does not include compounds of the types illustrated by *Schleswig-Holstein* and *Australian-New Zealand (negotiations)*, which Wälchli elsewhere specifically excludes from the list of co-compounds (7).

Finally in this chapter, Wälchli discusses what he terms the ‘contextual motivation’ of compounds, pointing out that many co-compounds are uninterpretable out of context, and commenting on the motivation for co-compounding in those languages where it is frequent and productive.

In chapter 6, ‘The areal distribution of co-compounds in the languages of Eurasia’, Wälchli argues that ‘[c]o-compounds are most frequent in continental East and South East Asia, their frequency diminishing as one moves westward’ (196). This conclusion is based on an analysis of the Universal Declaration of Human Rights and the Gospel according to Mark in over 300 languages. The generalisation holds true even across languages in the same

language family, such as Turkic and Indo-European languages, which show a greater density of co-compounds the further east they are found. However, even within a single language there may be a great deal of variation in the use of co-compounds. Co-compounds do not appear to correlate with any other typological factor of the languages concerned. This chapter is a masterpiece of scholarship, not only because of the number of languages investigated but also because of the amount of detail that has been considered in so many of these languages.

In an avowedly speculative chapter 7, 'Some considerations about the diachronic evolution of co-compounds', Wälchli is concerned with the questions of how co-compounds might arise in the first place and how the class of co-compounds can come to expand. Following on from the previous chapter, which considers the use of co-compounds in texts, Wälchli points out the importance of co-compounding in folk poetry, and how folk poetry in languages that do not allow co-compounds has other forms with similar functions.

Chapter 8, 'Conclusions', summarises the main findings of the study. There is an appendix of languages mentioned, and another in the form of maps showing the geographical location of these languages; these are followed by a lengthy reference list, and three useful indexes.

This is an impressive work in many ways. Most significantly, it shows us what can be achieved by viewing a particular linguistic pattern not simply as something which either is or is not instantiated in a particular language, but as a pattern which occurs in texts where it has a specific function. Despite occasional comments on the textual anchoring of word-formation, most studies of affixation patterns completely fail to consider the way in which affixation is used to create meaning and focus in texts. If Wälchli's study has the influence it deserves, it may lead to a fashionable and enlightening way of merging morphological and discourse studies.

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(Received 29 November 2005)