

A Review of *The Syntax-Morphology Interface. A Study of Syncretism*  
by Matthew Baerman, Dunstan Brown, and Greville G. Corbett

Gereon Müller, Universität Leipzig\*  
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## 1. Overview

Current research on inflectional morphology is carried out both from a typological perspective and from the perspective of grammatical theory. As far as I can tell, the present book is the first serious attempt to bridge the gap between these two kinds of approaches, and it qualifies as an important and highly welcome contribution for this reason alone.<sup>1</sup> The empirical domain that the authors focus on is the phenomenon of syncretism in the world's languages, i.e., cases where the morphological component fails to realize a distinction made in the syntax by using homophonous markers for two (or more) instantiations of a grammatical category – e.g., two (or more) cases, or two (or more) persons. Evidently, then, syncretism poses a *prima facie* problem for any theory of the morphology/syntax interface, and this can be taken to explain the main title of the book.

Baerman, Brown, & Corbett (henceforth BB&C) proceed as follows. After a brief Introduction (ch. 1) in which they lay out basic methodological principles and clarify the notion of syncretism presupposed throughout, ch. 2 (Characteristics of Syncretism) introduces a number of concepts that prove useful for classifying and interpreting various kinds of syncretism in the subsequent chapters. For instance, the authors distinguish between simple syncretism, nested syncretism, contrary syncretism, and polarity effects. Simple syncretism is the standard case; here, two paradigm cells in a paradigm have the same marker, which creates a single syncretism domain. With nested syncretism, one syncretism domain embeds another one, as in the declensional system of Upper Sorbian, where dative and locative are syncretic in the singular, and dative, locative, and instrumental are expressed by the same form in the dual. The concept of contrary syncretism captures scenarios where there are three overlapping syncretism domains, as in noun inflection in the singular in Nuer (Nilo-Saharan): Of the three cases nominative, genitive, and locative, some nouns show syncretism of genitive and locative, others have identical forms for nominative and genitive, and yet others exhibit homophony of nominative and locative. Finally, polarity effects instantiate flip-flop distributions of syncretism. Old Irish declension is a case point. Here, genitive singular and nominative plural can be systematically identical. Under some theoretical accounts, this may be taken to suggest that genitive singular and nominative plural count as exact opposites. Therefore, on a more abstract level, they may be viewed as having a uniform function after all (see below).

Furthermore, the basic grammatical categories that can be affected by syncretism are illustrated in this chapter, and three morphological characteristics are introduced (regularity, directionality, and unmarkedness).

Ch. 3 ('Cross-Linguistic Typology of Features') is by far the longest chapter (pp. 37-125), and arguably the most important part of the book. Based on a comprehensive typological survey of syncretism in the world's languages, the authors go through one

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<sup>1</sup> Of course, studies like Wurzel (1984), Carstairs (1987), Aronoff (1994), and Stump (2001) also strive to reconcile formal morphological analysis with data drawn from a variety of typologically different languages. However, it seems to me that the present book is virtually unique in its combination of large-scale empirical research as it is characteristic of work in linguistic typology, and explicit formal analysis as it is known from work in grammatical theory.

grammatical category after the other, addressing the question of which groups of instantiations of the grammatical category can, or tend to be, affected by syncretism. A simple picture emerges: For each grammatical category, there are clear preferences among the world's languages as to which domains are affected by syncretism (i.e., syncretism is more likely to affect certain combinations of, say, cases or persons than others), but there are few, if any, strict generalizations as to what can or cannot be a syncretism domain for a given grammatical category. This had already been observed for some grammatical categories in the typological literature (see, e.g., Cysouw (2003) on person), but the generality and comprehensiveness with which the authors make this point is unique (and convincing). Assuming with Newmeyer (2005) that preferences are to be accounted for by extragrammatical factors, whereas strict generalizations follow from purely grammatical constraints, these findings have important repercussions for grammatical theory. In particular, they call into question various approaches to feature systems that postulate that certain combinations of instantiations of a grammatical category cannot form a natural class. This holds, e.g., for Harley & Ritter's (2002) fixed person hierarchy (which BB&C discuss on pp. 60f; also see Trommer (2006b), Nevins (2007) for relevant discussion.) The lesson to be learned for grammatical theory here is that if one is looking for universal principles of inflectional morphology, they are most likely not to be found in the domain of natural classes of instantiations of grammatical categories as they are documented by syncretism patterns; such natural classes exist, but they may vary from one language to another. Accordingly, any theoretical means to account for syncretism by invoking natural classes of instantiations of grammatical categories will have to be fairly flexible. To give a somewhat more concrete example: Given that systematic syncretism can in principle affect 1st and 2nd person, 2nd and 3rd person, and 1st and 3rd person, all these three groupings must qualify as potential natural classes (assuming that person syncretism is captured by underspecification of person information, which encodes natural classes of persons). Thus, if there are universal principles of inflection, they will have to be found in the ways that primitives underlying grammatical categories can be combined (rather than in the primitives themselves) – hence, in a more abstract area.

After having addressed syncretism phenomena with instantiations of all the major grammatical categories (case, person, gender, number, and tense/aspect/mood), the authors integrate a section on polarity effects, and finally discuss the interaction of grammatical categories at the end of the chapter.

Ch. 4 ('Formal Representation') marks the step from data to theory in the present book. Essentially, here the authors discuss and criticize various approaches to syncretism in inflectional morphology in light of the evidence presented in ch. 3.<sup>2</sup> They begin by addressing, and rejecting, hierarchical and flat feature structures as means to account for syncretism. Then, they turn to a classic idea of how to account for (a certain type of) polarity effects, viz.,  $\alpha$  notation: the use of variables of feature values (pp. 132-133). Again, it is argued on the basis of evidence presented earlier that this theoretical means should be abandoned.<sup>3</sup> Third, extending and elaborating on Stump's (2001) work, the authors provide a discussion of bidirectional syncretism, distinguishing between two kinds:

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<sup>2</sup> Throughout, it seems to be presupposed that syncretism should not be regarded as accidental in the unmarked case; cf., e.g., the remark that "accidental homophony may be reinterpreted as morphological systematicity" (p. 168; also cf. p. 10). In this respect, the present book differs from studies like Williams (1994) and Carstairs-McCarthy (2004), which are more ready to recognize accidental homophony.

<sup>3</sup> Incidentally, the literature on  $\alpha$  notation in grammar, including morphology, is vast. In its present form, the concept goes back to at least Chomsky (1965) and Chomsky & Halle (1968) – studies which, like all other work by Chomsky, are curiously neglected throughout the present monograph. The concept of  $\alpha$  notation also figures prominently in Noyer (1992), and it is critically discussed in some detail by Harley (1994) and Johnston (1996). However, in their discussion of  $\alpha$  notation, the authors exclusively rely on an article by Susane Béjar and Daniel Currie Hall.

divergent bidirectional syncretism (the kind of bidirectional syncretism that Stump was concerned with) and convergent bidirectional syncretism. Simplifying a bit, bidirectional syncretism describes syncretic patterns in paradigms where apparent take-overs of forms from one paradigm cell to another one go in two opposite directions (I will address this issue in some detail below, in section 2.2). BB&C propose to account for this by invoking directional rules of referral that basically just state (rather than derive) the respective take-overs (see Zwicky (1985), Stump (1993, 2001)).<sup>4</sup> In this context, the authors also discuss the idea of invoking violable and ranked constraints to account for bidirectional syncretism (see Wunderlich (2004), based on Prince & Smolensky (2004)). They argue (correctly, in my view) that such an optimality-theoretic approach is not as different from a referral-based approach as it might seem at first sight (“directional rules are ... inherent in the model”, p. 150), and may express the same kind of analysis for this reason. Fourth, the issue is addressed of whether there are systematic constraints on syncretism. Here the authors critically evaluate (what has become known as) Williams’s (1994) Basic Instantiated Paradigm Principle (also see Bobaljik (2002)); some of Andrew Carstairs-McCarthy’s work revolving around the Systematic Homonymy Claim (however, the Paradigm Economy Principle (see Carstairs (1987)) and the No Blur Principle (see Carstairs-McCarthy (1994), Cameron-Faulkner & Carstairs-McCarthy (2000)) are conspicuously absent from the discussion); and Stump’s (2001) taxonomy of syncretism types. Stump distinguishes four kinds of syncretism: (i) unstipulated syncretism, which is derivable by underspecification; (ii) directional syncretism, which is derivable by rules of referral; (iii) bidirectional syncretism, which is derivable by pairs of rules of referral, which arise as a consequence of a Bidirectional Referral Principle (that the authors fail to mention, despite the fact that it displays considerable ingenuity, in my view – see below); and (iv) symmetrical syncretism, derivable by a Symmetrical Syncretism Metarule (which also is not discussed in the present book).

Having considered a variety of recent theoretical approaches to syncretism, the authors arrive at the modest conclusion that “no attempt to impose formal constraints on the description of syncretism is wholly successful. On the other hand, there *are* some tendencies” (p. 169). The statistical patterns resulting from these tendencies are attributed to diachronic processes, which permits the authors to maintain that any kind of theoretical morphological analysis will have to be sufficiently flexible to account for the variation observed. This general view is reflected in the authors’ own approach that is developed in the final part of the book: Network Morphology. In ch. 5 (‘Formal Framework and Case Studies’), Network Morphology is first briefly introduced. Network Morphology (like the theories laid out in Anderson (1992), Aronoff (1994), and Stump (2001)) is an inferential-realizational approach (to use Stump’s terminology) according to which inflection markers are not (morpheme-like) lexical items in their own right. Rather, realization rules of the morphological component predict word forms for (possibly extended) stems if the latter carry the appropriate morphosyntactic feature specifications. On this view, syncretism goes back to underspecification, or it follows from rules of referral that either override basic rules of exponence or provide forms for what would otherwise remain paradigmatic gaps because no rules of exponence can apply.<sup>5</sup> Given the free availability of referral rules of all kinds, Network Morphology does not seem to impose any restrictions as to what may

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<sup>4</sup> The term ‘take-over’ is due to Carstairs (1984, 1987). Here and in what follows, I will generally reserve the notion ‘take-over’ for the phenomenon as such, and use the notion ‘referral’ when a specific rule in a formal framework is involved.

<sup>5</sup> Note in passing that this kind of situation can occur in BB&C’s Network Morphology analyses, but never in – otherwise sometimes very similar – analyses carried out within Stump’s (2001) Paradigm Function Morphology approach. The reason is that the latter envisages a general “Identity Function Default” rule that precludes the existence of paradigm gaps.

count as a systematic instance of syncretism that can be accounted for in the grammar. Apart from that, it can be pointed out that Network Morphology involves techniques and notations that lend themselves to simple computational implementation (via DATR), like default inheritance. However, it is not quite clear to what extent these concepts are crucial for the approach to work; it seems to me that most (if not all) aspects of the morphological analyses that have been developed in Network Morphology can straightforwardly be rephrased in other inferential-realizational approaches such as Paradigm Function Morphology, with the effects of, say, default inheritance captured by the Specificity Condition (also known as Blocking Principle, Panini's Principle) employed by these frameworks.<sup>6</sup> It is also worth noting that Network Morphology shares with many other morphological approaches (among them Paradigm Function Morphology (see Stump (2001)), Distributed Morphology (see Halle & Marantz (1993), Harley & Noyer (2003)), and Minimalist Morphology (see Wunderlich (1996, 1997c))) the assumption that standard paradigms as they are known from grammatical descriptions and reference grammars are not primitives of the theory but rather derivative. As BB&C remark on p. 173, "we make a distinction between the fully specified morphosyntactic paradigm and the particular formal analysis which accounts for it."

Finally, the book ends with three Network Morphology-based case studies of inflectional systems with massive display of syncretism, all of which have played a role in discussions in ch. 3. (subsections 5.2–5.4). The first study is on verb inflection in Dhaasanac (Cushitic; Ethiopia/Kenya); the second study is on verb inflection in Dalabon (Gunwinyguan; Australia); and the third on noun inflection in Russian. At least two of these studies have already been published elsewhere, in somewhat more comprehensive form (see Corbett & Fraser (1993), Fraser & Corbett (1994), and Brown (1998) on Russian, and Evans et al. (2001) on Dalabon). In ch. 5, the exposition focusses on particularly interesting aspects of the analyses of the three systems, but each case study is accompanied by an appendix (appendices 4–6) that presents a comprehensive DATR fragment. This turns out to be very helpful when one wants to understand the system as a whole.<sup>7</sup> Dhaasanac is discussed in ch. 3 in the context of polarity effects; the intricate (but fully systematic) polarity pattern that shows up in various tense/aspect paradigms is accounted for by introducing an abstract category that is not motivated syntactically (a case of "morphology by itself" (Aronoff (1994)), as the authors point out (p. 184): the *index* (I will come back to this). Dalabon is discussed in ch. 3 in the context of syncretism with transitive verbs. In ch. 5, the authors argue that the complex system of verb inflection, with abundant participant neutralization, requires both underspecification and rules of referral. Finally, noun (and adjective) inflection in Russian plays a role throughout the book; the analysis given in ch. 5 also makes use of both underspecification and (particularly) rules of referral to account for the many syncretisms that the system displays. In addition, a further abstract category is introduced that is not motivated by syntax: inflection class features (the analysis makes use of both simple inflection class features that encode a given inflection class, and a more general inflection class feature N\_O that encodes a natural class of inflection classes, viz., N\_I and N\_IV). All three analyses strike me as careful and elegant, and involve a high level of theoretical abstraction.

All in all, there is a lot in this book that one must be impressed by. It is unique

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<sup>6</sup> This is not surprising since Network Morphology is based on DATR (see Evans & Gazdar (1996)), and Gazdar (1992) has shown that Paradigm Function Morphology can be implemented in DATR.

<sup>7</sup> Another thing that I found immensely helpful is that the formal analyses are further backed up by material on websites of the Surrey Morphology Group, in which this book has its origins. For instance, the theory of verb inflection in Dalabon presented in appendix 5 generates all and only the grammatical word forms of verbs in this language, but to check this, it is instructive to consult the list of theorems provided at <http://www.surrey.ac.uk/LIS/SMG/dalabon>.

in its combination of empirical research carried out from a typological perspective and carefully developed theoretical analyses. It significantly enriches our knowledge of the varieties of syncretism in inflectional systems of the world's languages.<sup>8</sup> And it identifies systematic patterns of syncretism that often resist simple treatments in standard models (e.g., polarity effects, types of bidirectional syncretism), and that therefore pose a challenge for morphological theory. For these reasons, I believe that the book will have a huge impact on future developments in research on inflectional morphology (to some extent, this effect has already manifested itself in the two years that have passed since the book's publication). I, for one, find it a constant source of inspiration.

Still, there are problems. It seems to me that, somewhat paradoxically, the most severe problem can be traced back to what is arguably the book's greatest virtue: the attempt to reconcile large-scale empirical research with grammatical theory. The problem is related to the notion of subanalysis. I address this in the next subsection, and turn to other potential problems after that.

## 2. Problems

### 2.1. Subanalysis

Right at the outset (pp. 7-9), BB&C make a momentous decision: They state that they will consider only whole-word syncretisms (i.e., cases of homonymy where a whole word form can have two different syntactic functions), not partial syncretisms (i.e., cases of homonymy involving proper subparts of word forms). An account of partial syncretism requires a subanalysis of seemingly non-composite, primitive inflectional markers as they might be motivated by looking at the syntax: Under subanalysis, these markers are reanalyzed as composite, in the sense that they are made up of smaller units with separate specifications of morphosyntactic features, and often with segment-like size.

Subanalysis is a recurring feature of many current approaches to inflectional morphology. Subanalysis has been pursued in Distributed Morphology since its inception (see, e.g., Halle (1992, 1994) on noun inflection in Latvian and Russian, respectively; Noyer (1992) on the Afro-Asiatic prefix conjugation; Halle & Marantz (1993) on argument encoding markers on verbs in Georgian and Potawatomi; and Halle & Marantz (1994) on Spanish object clitics), and it continues to be applied in more recent analyses (see, e.g., Harbour (2003) on verb inflection in Kiowa, Müller (2005) on noun inflection in Icelandic, Trommer (2006b) and Nevins (2007) on verb inflection in Menominee, and the papers collected in Müller & Trommer (2006)).<sup>9</sup> Similarly, the fine-grained systems of rule blocks that can be found in stem-and-paradigm approaches like those in Anderson (1992) and Stump (2001) are capable of incorporating subanalysis; and accounts that make use of subanalysis have indeed often been developed in rule-block based frameworks (compare, e.g. Stump's Paradigm Function Morphology approach to Bulgarian verb inflection, which postulates four rule blocks for suffixal markers, or Anderson's analyses of Georgian and Potawatomi that Halle and Marantz base their discussion on). Janda & Joseph's (1992) analyses in terms of what they call "pseudo-agglutinative hypersegmentation" or, alternatively, "meta-templates" also belong in this tradition; so does Wiese's (2003) analysis of Latin noun

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<sup>8</sup> It is also worth mentioning in this contexts that there are three further appendices (appendices 1–3) that list case syncretisms, person syncretisms, and syncretisms with transitive verbs in fairly large samples of languages constructed for the World Atlas of Language Structures (see Haspelmath et al. (2005)), together with precise descriptions of the syncretism displayed in each case.

<sup>9</sup> There is more than one theoretical means to implement subanalysis in Distributed Morphology. Subanalysis can be brought about by invoking the concept of (post-syntactic) fission; see Halle & Marantz (1993) and Noyer (1992) for two (radically different) implementations. In addition, subanalysis may also follow from postulating many functional heads in the syntax that serve as insertion sites for inflectional exponents.

inflection in an approach that is otherwise very different from standard theories in that it radically dispenses with any direct association of form and function of exponents (relying on an association of abstract scales instead); and the same goes for the articulate system of iterated affixation that Wunderlich (1996, 1997c) develops within a Minimalist Morphology approach. Based initially on evidence from lesser-studied languages, subanalysis has also played a major role in analyses developed in the tradition of tagmemics (see, e.g., Pike (1963) on Fore and Elson & Pickett (1964) on Sierra Popoluca).

In justifying their position, BB&C adduce Pike’s (1965) analysis of the inflection of *sein* (‘be’) in German as a model case showing the perils that arise under subanalysis (p. 9). Pike suggests a subanalysis of the present tense and infinitive forms of *sein* as in (1); here, each exponent is of segmental size (or even subsegmental size, in the case of the decomposed diphthong [aɪ]).<sup>10</sup>

(1) *Pike’s (1965) subanalysis of verb inflection with sein (‘be’) in German:*

1.sg	b		ɪ	n	
2.sg	b		ɪ	s	t
3.sg			ɪ	s	t
1.pl	z		ɪ	n	t
2.pl	z	a	ɪ		t
3.pl	z		ɪ	n	t
inf	z	a	ɪ	n	

BB&C seem to presuppose that a subanalysis along the lines of (1) is obviously misguided; they claim that “whatever the merits of such an analysis, it is not one which is compatible with most morphological models” (p. 9). In light of the above remarks about the widespread use of subanalysis in many current theories of inflectional morphology, this would come as a surprise. Indeed, this statement turns out to be most probably false. In the appendices, I develop two straightforward implementations of Pike’s subanalysis in (1) – the first in the framework of Paradigm Function Morphology, and the second in the framework of Distributed Morphology. Since these two frameworks are arguably among the most influential current morphological theories, and since there is every reason to assume that this result can be generalized to many other current approaches, it seems clear that BB&C’s assumption is not tenable.

Interestingly, Pike’s (1965) article contains two further analyses of inflectional phenomena in German: a subanalysis of definite article inflection (*der, die, das*, etc), and a subanalysis of personal pronouns, including suppletion phenomena (*ich, mich, mir, meiner*, etc.). In both cases, analyses have recently been developed that presuppose a subanalysis of more or less exactly the kind documented in Pike (1965) (and, it might be added, without reference to Pike); see, e.g., Wunderlich (1997b), Wiese (1999) on the inflection of definite articles in German, and Wiese (2001) and (especially) Fischer (2006) on the inflection of personal pronouns in German.<sup>11</sup>

<sup>10</sup> I render the exponents in this paradigm in Pike’s original phoneme-based notation; curiously, BB&C change this to a grapheme-based notation (p. 9). Deviating from both Pike and BB&C, I have retained the standard textbook order of the persons and numbers, in the hope that the syncretism fields are sufficiently evident (e.g., in the case of *n* and *s*) without reorganizing the rows.

<sup>11</sup> It is also worth pointing out that, whereas Pike’s (1965) analysis does not yet meet modern standard concerning formal rigour and predictiveness, Pike’s informal remarks clearly point in interesting directions that have subsequently successfully been pursued by modern theories of inflection. For instance, he realizes that subanalysis tends to require some concept of extended exponence (see Matthews (1972)). He also suggests a decomposition of standard morphosyntactic features so as to encode natural classes of instantiations of a grammatical category (like person); the exponent *b* in (1), for instance, is classified as [−3.sg]. Furthermore, he discusses polarity effects in Fore from a theoretical point of view (see Pike (1963, 1965)).

To sum up so far, there is no denying that subanalysis is a common approach in current (and not-so-current) morphological theories. As it turns out, BB&C themselves violate their rule of “limiting the investigation to whole word forms” (p. 9) repeatedly throughout the book (without mentioning this once). In what follows, I present an incomplete list of cases where BB&C presuppose subanalysis.

- *Udihe verb inflection*

To illustrate the notion of directionality, BB&C point out (pp. 24f) that in Udihe (Tungusic), “the syncretic 1SG/2SG ending of the future tense is identical to the 2SG ending as found in the past tense”. The endings may be identical, but the whole word forms certainly aren’t (compare *jexezeje-i* with *jexe:-i*). Given the methodological principle of limiting the investigation to whole word forms, this syncretism should be irrelevant, and cannot be used to illustrate a syncretic pattern.

- *Orokaiva verb inflection*

Exactly the same problem shows up with Orokaiva (Trans-New Guinea) verb inflection, adduced by BB&C (p. 26) in order to illustrate the notion of unmarkedness. Here, the relevant word forms are *hembu-a*, *hembu-w-a*, and *hembu-ha-r-a*, with an assumed identical *a* syncretism that is unavailable if only whole-word syncretisms are taken into account.

- *Macedonian verb inflection*

To illustrate directional effects with person syncretisms (and discuss the question of whether fixed hierarchies play a role here), BB&C (p. 63) address the system of Macedonian conjugation, based on Stump (1993) (also see Stump (2001) on Bulgarian). The discussion here would also seem to presuppose that a partial syncretism involving zero exponence can be identified: The argument under discussion is that zero marking is copied from third person singular to second person singular contexts in the aorist because it also occurs in third person singular contexts in the present tense – but the full third person word forms in present tense and aorist are not identical (in two of the three conjugations discussed by Stump), due to stem alternation. Such a subanalysis corresponds to the treatment in Stump (1993, 452) (where three rule blocks are identified: I, II, and III).

- *Murle verb inflection*

As an example of a system where there is a directional syncretism from first person to third person, BB&C (pp. 64f) address verb inflection in Murle (Nilo-Saharan), where there is a syncretism of first person (inclusive) and third person in the subjunctive (both in the singular and in the plural; in the latter, there is a combination of a prefix *k-* and a suffix *-it*). Directionality is assumed to go from first to third person because the same exponents also show up in the first person inclusive in the perfect (*k-*, *-it*) but not in third person plural environments (*-it*). The discussion here presupposes that the syncretisms with *k-* and *-it* can be treated separately, i.e., that there is subanalysis.

- *Old Icelandic verb inflection*

To illustrate a directional syncretism going from second to third person, BB&C present evidence from Old Icelandic weak conjugation (p. 66). The relevant subjunctive forms are *-er*, *-e* (second and third person singular, respectively); the corresponding present indicative forms are *-ar*, *-ar*. Clearly, directionality of syncretism can only be assumed if *-er* and *-ar* are subanalyzed as *-e-r*, *-a-r*, with *-r* the syncretic exponent that signals the directionality of the syncretism. If only whole-word syncretism are considered, there is no directionality observable here.

- *Dani verb inflection*

Exactly the same problem arises with Dani (Trans-New Guinea), where the relevant forms are *-ip* and *-ep*, with a presupposed subanalysis as *-i-p*, *-e-p* and syncretism involving *p* (see BB&C, p. 67). (In both these cases, the authors state that a vowel alternation is involved, but since this alternation must be morphological in nature (and cannot be purely phonological), the relevant word forms cannot qualify as identical, even in a more abstract sense.)

- *Hungarian verb inflection*

On pp. 156-157, in the context of discussing some of Andrew Carstairs-McCarthy’s constraints on syncretism, BB&C address a directional syncretism in Hungarian conjugation, viz., a take-over of a first person singular indefinite form by the definite form in the past. The argument for the directionality comes from first person singular forms in the present tense; relevant word forms are *vár-o-k*, *vár-o-m* (present) and *vár-t-a-m*, *vár-t-a-m* (past). BB&C’s remark that “at issue is the distribution of the 1SG affixes *-k* and *-m*, which are isolated in [the following table]” presupposes subanalysis – under the whole-word syncretism restriction, there could be no take-over because the present and past tense forms are invariably different.

- *Trung verb inflection*

BB&C (pp. 78f) assume that in transitive contexts where both core arguments need to be encoded, the Tibeto-Burman language Trung exhibits a substitution of subject person if the object is first person. The directionality is supposed to go from second person to third person. The authors suggest that “the second person marker *nə-* is used for third person subjects as well.” It seems to me that there are independent problems with this analysis (it is questionable whether *nə-* should be viewed as a second person marker in the first place), but it can at least be noted that it is at variance with the restriction to whole-word syncretisms: Ignoring zero exponence, the paradigm has three exponents: (i) *nə-*, (ii) *-ŋ*, and (iii) *nə- -ŋ*, where (iii) is assumed to be composed of (i) (the second person marker) and (ii) – but this reasoning involves subanalysis.

For the sake of concreteness, let me sketch a simple alternative analysis of the Trung paradigm before moving on. The paradigm is given here in (2) (“-” indicates reflexive context, “ $\emptyset$ ” absence of exponence).

(2) *Trung verb inflection:*

		obj		
		[+1,-2]	[-1,+2]	[-1,-2]
subj	[+1,-2]	-	-ŋ	-ŋ
	[-1,+2]	nə- -ŋ	-	nə-
	[-1,-2]	nə- -ŋ	nə-	$\emptyset$

The analysis is developed in Distributed Morphology, but not much hinges on that (see appendix 2 for a sketch of the basic assumptions of this model, including the person feature decomposition and Noyer’s (1992) notion of fission adopted here). Suppose that there are two rules for vocabulary items, as in (3), plus one general impoverishment rule that captures participant neutralization effects, as in (4). That is all that is needed to capture the paradigm in (2); note that *nə-* is not a second person marker under this analysis, but a more general [-1] marker; and there is no directionality involved.

- (3) a. /-ŋ/ ↔ [+1,-2]  
 b. /nə-/ ↔ [-1,subj]



(4) [-1,-2,subj] → ∅ / \_\_\_[-1,-2,obj]

Finally, the most spectacular violation of the authors' statement that only inflected whole word forms will be considered in the book comes from the formal analyses in ch. 5 (and the corresponding appendices). The approach to Dhaasanac verb inflection makes massive use of subanalysis. It splits up word forms into several positions (such as “steminitial”–“formcgrade”, in analogy to rule blocks in stem-and-paradigm approaches, or multiple functional heads in Distributed Morphology) which are separately realized by exponents of typically segmental size. This accounts for instances of block syncretism. Similarly, the approach to Dalabon verb inflection relies on subanalysis. In the course of laying out the analysis (in appendix 5), BB&C actually state (p. 246): “It should be noted that we have tried to break down the prefixes beyond a standard analysis of morphemes.” Thus, the claim at the beginning of the book that “we focus our attention on syncretism between inflected whole word forms” (p. 7) is undermined throughout the book, and is finally explicitly contradicted by an opposite claim towards the very end of the book; however, no attempt is made to resolve this inconsistency.

As it turns out, the only formal case study in this book that does not crucially rely on subanalysis is the approach to Russian noun inflection.<sup>12</sup> This may or may not be supported by the empirical evidence. Halle (1994) assumes that it is not, and develops an approach to Russian noun (and adjective) inflection based on subanalysis; then again, the inflectional markers involved here are of fairly small size to begin with (at least in the domain of noun inflection proper). However, independently of these considerations, one might speculate that what underlies approaches that dispense with subanalysis in this case (but not in others; see, e.g., Alexiadou & Müller (2007)) is some form of “reverse Indo-European bias” – a reluctance to apply segmentation techniques that are well established for lesser-studied languages to the well-studied Indo-European languages, where there is a long tradition of morpheme-based analysis. From this perspective, Pike's (1965) analysis could perhaps be viewed as the result of a deliberate decision to treat German in the same way that one would naturally treat, say, Fore or Potawatomi, with a conscious neglect of the existing tradition of grammatical analysis.

More generally, given that there is convincing evidence for subanalysis in inflectional systems of the world's languages (at least some of which is also implicitly accepted by the authors), the decision to formally exclude instances of partial syncretism from inclusion in the data base that provides the empirical foundation for the present study may be viewed as unfortunate. As we have seen, some partial syncretisms were considered after all, but it is likely that many of them will have been ignored.<sup>13</sup> All in all, this may well have led to a

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<sup>12</sup> At least, this would seem to hold for the presentation in the main text. Following Corbett & Fraser (1993, 136), the full analysis presented in the appendix does break up a marker like *ami* (instrumental plural) into a theme vowel and an ending, and thus implies subanalysis, too.

<sup>13</sup> Thus, there remain several phenomena in the book where the authors do not invoke subanalysis even though the data might arguably suggest it. The verb inflection system of Diyari is a case in point. BB&C discuss Diyari twice: first, in a section on syncretism among non-structural cases (p. 54); and second, in a section on bidirectional syncretism (pp. 143f). As for the latter, the authors observe that the Diyari paradigm involves overlapping syncretism domains. On their view, *na* is an accusative form that one inflection class (viz., male personal names) also uses in nominative (absolutive) contexts; zero exponence is used in nominative (absolutive) contexts, and also in the accusative by singular nouns. Notwithstanding the problem posed by the slightly more general distribution of zero marking (which BB&C tackle in a footnote), it seems clear that *na* has inherently a much wider distribution than just the accusative in the female name declension – but this can only be discovered if subanalysis is applied: Exactly the same exponent shows up as *the first* part of a composite marker in dative, allative, and locative contexts with female names (with two different exponents able to follow it): *na-ŋu*, *na-ŋka*. It is therefore unclear why an occurrence of *na* in the nominative in one declension should be treated differently (viz., as an instance of a bidirectional syncretism) from an occurrence of *na* in other object cases in another declension. Consequently, there is no pattern of bidirectionality left in the data: There are three types of occurrences of *na* that must be captured

blurring of the overall picture, and may ultimately even call into question the typological generalizations that the authors arrive at.

That said, it seems clear that a typological approach which recognizes partial syncretism faces a number of practical problems that can be avoided by looking only at whole-word syncretism: To properly apply subanalysis, the inflectional system of each language under consideration must be looked at in much more detail than is otherwise needed. In addition, subanalysis requires a close investigation of the language’s phonological system. Thus, subanalysis makes large-scale empirical investigations even more complex than they are anyway. To conclude, it seems that the book’s outstanding characteristic, viz., the attempt to bridge the gap between typology and grammatical theory, is responsible for a major shortcoming: In a nutshell, the typological approach tends to resist subanalysis, and grammatical theory tends to suggest it.<sup>14</sup>

## 2.2. Bidirectional Syncretism

In cases of bidirectional syncretism the data suggest that there are two take-overs within a single grammatical domain (e.g., noun inflection, or verb inflection) that are related and go in opposite directions.<sup>15</sup> As noted above, BB&C distinguish between convergent bidirectional syncretism and divergent bidirectional syncretism (pp. 136ff). The system of noun and pronoun declension in Bonan (Mongolian) exemplifies convergent bidirectional syncretism (pp. 136-138). Here, it looks like the accusative form of nouns is a take-over from the genitive forms (of both nouns and pronouns), and the accusative form of pronouns is a take-over from the dative forms (of both nouns and pronouns).

### (5) *Bonan declension*

	noun ('foliage')	pronoun ('he')
NOM	labčon-Ø	ndžan-Ø
GEN	labčon-ne	ndžan-ne
ACC	labčon-ne	ndžan-de
DAT	labčon-de	ndžan-de
ABL	labčon-se	ndžan-se
INS	labčon-gale	ndžan-gale

In contrast, the Latin *o*-declension (pp. 139-142) is assumed to instantiate a case of divergent bidirectional syncretism. BB&C assume that the regular neuter inflection (NEUT<sub>a</sub>) in these environments can be described in terms of a take-over of the accusative marker for nominative contexts. However, for the few neuter noun stems that instantiate the third pattern (NEUT<sub>b</sub>), the take-over then goes in the opposite direction, such that the nominative marker is also used for accusative contexts.

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by three distinct rules of exponence in standard underspecification-based approaches (see Bierkandt (2006, 58)). Any approach in terms of directional rules of referral will minimally have to postulate two separate rules of referral to cover the distribution of  $\bar{n}a$ , with no bidirectionality involved (since spreading goes from a single source – accusative – to two different domains – nominative, oblique).

<sup>14</sup> As noted by Andrew Carstairs-McCarthy (p.c.), the decision to focus on whole-word syncretisms may also raise problems independently of the issue of subanalysis of affixal material, in the domain of stem alternations. For instance, nominative and accusative plural are clearly syncretic in forms like *ra:janas*, *ra:jñas* ('king') in Sanskrit, even though different stem variants are involved. Strictly speaking, this syncretism does not hold on the whole-word level, so it would have to be ignored under BB&C's approach.

<sup>15</sup> Parts of this subsection are based on material in Müller (2007b).

(6) *Latin o-declension, singular*

	NEUT <sub>a</sub>	MASC	NEUT <sub>b</sub>
	bell- ('war')	serv- ('slave')	vulg- ('crowd')
NOM	-um	-us	-us
ACC	-um	-um	-us
GEN	-ī	-ī	-ī
DAT	-ō	-ō	-ō
ABL	-ō	-ō	-ō

At this point, the question arises of what the difference is between convergent and divergent bidirectional syncretism. BB&C offer a description according to which, e.g., under convergent bidirectional syncretism, the form of one case *x* is taken over from the form of some other case *y* in certain contexts, and from the form of some other case *z* in other contexts, whereas, under divergent bidirectional syncretism, the form of one case *x* is taken over from the form of some other case *y* in certain contexts, and the form of case *y* is taken over from the form of case *x* in other contexts (pp. 136, 139). Strictly speaking, however, there is nothing in the *structure* of the paradigm in (5) that makes the bidirectional syncretism in Bonan convergent (Lennart Bierkandt, p.c.). If the paradigm is rotated through 90 degrees, it looks very much like all the paradigms instantiating divergent bidirectional syncretism discussed by BB&C, including the one in (6); see (7).

(7) *Bonan declension, rotated paradigm*

	NOM	ACC	GEN	DAT	ABL	INST
pronoun	ndžan-Ø	ndžan-ne	ndžan-de	ndžan-de	ndžan-se	ndžan-Gale
noun	labčon-Ø	labčon-ne	labčon-ne	labčon-de	labčon-se	labčon-Gale

Thus, at first sight it might seem that the distinction between convergent and divergent bidirectional syncretism is spurious. However, closer scrutiny reveals that this is in fact not the case. To see this, we need to look the approach to bidirectional syncretism in Stump (2001), which forms the background to BB&C's analysis but (as noted above) is not actually addressed in the book. Stump (2001, 219) develops an elegant analysis of bidirectional syncretism that centers around a *Bidirectional Referral Principle*. This meta-principle ties the existence of one rule of referral to the existence of another, complementary rule of referral. It is given in (8).

(8) *Bidirectional Referral Principle*

The existence of a rule of referral ' $RR_{n,\tau,C}(\langle X,\sigma \rangle) =_{def} \langle Y,\sigma \rangle$ , where  $Nar_n(\langle X,\sigma/\rho \rangle) = \langle Y,\sigma/\rho \rangle$ ' with referral domain *D* entails the existence of a second rule of referral ' $RR_{n,\tau/\rho,D-C}(\langle X,\sigma \rangle) =_{def} \langle Y,\sigma \rangle$ , where  $Nar_n(\langle X,\sigma/\tau \rangle) = \langle Y,\sigma/\tau \rangle$ ' with referral domain *D*.

Here, RR stands for a realization rule that is a rule of referral which states that the exponent for some fully specified morphosyntactic context  $\sigma$  is going to be the one determined independently (by some other RR) for a minimally different fully specified morphosyntactic context in which  $\sigma$  is modified by  $\rho$ ; *n* designates the number of the block in which the rule applies;  $\tau$  encodes a (possibly underspecified) well-formed set of morphosyntactic features that the rule realizes by its application; *C* is the domain in which the rule is applicable (e.g., nouns, or certain kinds of nouns); *X* stands for the wordform before the application of the rule, and *Y* stands for the form yielded by the application of the rule (simplifying a bit, *Y* differs from *X* in that it has the exponent introduced by the rule added to *X*); and  $Nar_n$  designates the most specific rule that is applicable in block *n*.

So far, this is the canonical approach to inflectional morphology in Stump (2001) (see appendix 1 for a slightly more elaborate exposition). However, Stump makes one important additional stipulation, viz., that every rule of referral  $RR_{n,\tau,C}$  has a *referral domain*  $D$  associated with it, in addition to the domain in which it can apply ( $C$ ).  $C$  must be a subset of  $D$ . Two possibilities arise:  $C$  may be a proper subset of  $D$ , or it may be identical to  $D$ . If there is a proper subset relation (i.e., referral domain ( $D$ ) and domain of application ( $C$ ) are not identical in a rule), the Bidirectional Referral Principle implies the existence of an inverse rule in which the domain of application is changed from  $C$  to the complement of  $C$  in  $D$  (the relevant items are set in boldface in (8)). On the other hand, the referral domain  $D$  may be identical to the domain of application  $C$  (i.e.,  $C$  may not be a proper subset); in that case, (8) does not have any further consequences because the inverse rule triggered by (8) must apply to an empty set of expressions.

Stump illustrates the Bidirectional Referral Principle with data from Romanian verb inflection: In all verb inflection classes except conjugation 1, 1.SG and 3.PL exponents are identical in indicative paradigms. Sometimes, 3.PL is considered the dependent part (based on evidence from conjugation 1, where the same ending ( $-u$ ) shows up *only* in 1.SG contexts); but with the verb *a fi* ('to be'), 1.SG. is assumed to be the dependent part (because the stem *sînt* for both 1.SG and 3.PL occurs throughout in the plural). Stump argues that there is a rule of referral as in (9-a), which has only the verb *a fi* as its application domain, and which has associated with it a domain of referral  $V$  (i.e., the set of all verbs). The Bidirectional Referral Principle then predicts that there must also be the inverse rule of referral in (9-b), with  $V-a fi$  as the application domain. Whereas (9-a) assigns the 3.PL exponent to 1.SG contexts, (9-b) assigns the 1.SG exponent to 3.PL contexts. (Both rules are slightly simplified here.)

- (9) a.  $RR_{0/1,\{agr(su):\{per:1,num:sg\}\},a fi}(\langle X,\sigma \rangle) =_{def} \langle Y,\sigma \rangle$ , where  
 $Nar_n(\langle X,\sigma / \{AGR(su):\{PER:3,NUM:pl\}\} \rangle) =$   
 $\langle Y,\sigma / \{AGR(su):\{per:3,num:pl\}\} \rangle$  Referral domain:  $V$
- b.  $RR_{0/1,\{agr(su):\{per:3,num:pl\}\},V-a fi}(\langle X,\sigma \rangle) =_{def} \langle Y,\sigma \rangle$ , where  
 $Nar_n(\langle X,\sigma / \{AGR(su):\{PER:1,NUM:sg\}\} \rangle) =$   
 $\langle Y,\sigma / \{AGR(su):\{PER:1,NUM:sg\}\} \rangle$  Referral domain:  $V$

In the same way, the Bidirectional Referral Principle may be applied to the case of Latin  $\sigma$ -declension in (6). Recall that BB&C assume that the masculine noun stems that take *us* in the nominative and *um* in the accusative are basic, and that the regular neuter inflection ( $NEUT_a$ ) in these environments can be described in terms of a take-over of the accusative marker for nominative contexts, whereas, for the few neuter noun stems that instantiate the third pattern ( $NEUT_b$ ), the take-over goes in the opposite direction, such that the nominative marker is also used for accusative contexts. This syncretism can be derived by invoking the Bidirectional Referral Principle if we assume that the application domain ( $C$ ) of one rule of referral is the set of regular neuter noun stems, with the whole set of neuter noun stems as the referral domain ( $D$ ). (10-a) then implies (10-b). The analysis further presupposes two simple rules of exponence that yield *us* for nominative contexts and *um* for accusative contexts.

- (10) a.  $RR_{1,\{case:nom,num:sg\},Neut_a}(\langle X,\sigma \rangle) =_{def} \langle Y,\sigma \rangle$ , where  
 $Nar_n(\langle X,\sigma / \{case:acc\} \rangle) = \langle Y,\sigma / \{case:acc\} \rangle$  Referral domain:  $N:NEUT$
- b.  $RR_{1,\{case:acc,num:sg\},Neut-Neut_a}(\langle X,\sigma \rangle) =_{def} \langle Y,\sigma \rangle$ , where  
 $Nar_n(\langle X,\sigma / \{case:nom\} \rangle) = \langle Y,\sigma / \{case:nom\} \rangle$  Referral domain:  $N:NEUT$

Turning next to the Bonan paradigm in (5), one might hope that the same kind of analysis can be given. However, this is not the case. Thus, suppose that there is a rule of referral like

(11-a) with the set of nouns as the application domain (= C) and a more comprehensive referral domain comprising nouns and pronouns (= D), which states that the exponent for ACC contexts is the exponent determined by the most specific rule applicable in GEN contexts. The Bidirectional Referral Principle would then predict the existence of the inverse rule in (11-b) with the complement of the set of nouns in D (i.e., only the set of pronouns) as the domain of application, and the choice of the exponent for GEN determined by the exponent selected for ACC contexts. But this is not what we want to derive: It is not the GEN exponent that is introduced by referral in the case of pronouns in Bonan, but the ACC exponent, and the referral does not go to ACC, but to DAT.

- (11) a.  $\text{RR}_{1,\{\text{acc}\},\text{N}}(\langle \text{X}, \sigma \rangle) =_{\text{def}} \langle \text{Y}, \sigma \rangle$ , where  $\text{Nar}_n(\langle \text{X}, \sigma / \{\text{gen}\} \rangle) = \langle \text{Y}, \sigma / \{\text{gen}\} \rangle$  Referral domain: NU<sub>Pron</sub>  
 b.  $\text{RR}_{1,\{\text{gen}\},\text{NU}_{\text{Pron-N}}}(\langle \text{X}, \sigma \rangle) =_{\text{def}} \langle \text{Y}, \sigma \rangle$ , where  $\text{Nar}_n(\langle \text{X}, \sigma / \{\text{acc}\} \rangle) = \langle \text{Y}, \sigma / \{\text{acc}\} \rangle$  Referral domain: NU<sub>Pron</sub>

That the syncretism cannot be accounted for by invoking the Bidirectional Referral Principle is due to the fact that in (8),  $\tau$  would have to encode part of speech (N vs. Pron) (rather than case) as the (possibly underspecified) feature set characterising an exponent, and C would have to encode case (rather than, e.g., N) as the application domain. This does not seem adequate. We may therefore conclude that the Bidirectional Referral Principle does not have anything to say about cases like the one from Bonan in (5).

More generally, we can conclude that the distinction between divergent and convergent bidirectional syncretism is one of substance: Divergent bidirectional syncretism (as in Latin *o*-declension and Romanian conjugation) lends itself to an analysis in terms of the Bidirectional Referral Principle; convergent bidirectional syncretism (as in Bonan declension) does not. It would thus seem that an account of convergent bidirectional syncretism in Stump's (2001) approach will most likely involve referral, but the two relevant rules must be stipulated separately, with no possibility of deriving one rule's existence from the existence of another one. Consequently, the concept of bidirectionality is not built into the analysis of a bidirectional syncretism in this case.

This leads me back to BB&C's approach to bidirectional syncretism. To the extent that formal analyses of instances of (either kind of) bidirectional syncretism are provided, these analyses rely on rules of referral. However, these rules of referral are not intrinsically related – neither with divergent bidirectional syncretism (where something like the Bidirectional Referral Principle is not invoked), nor with convergent bidirectional syncretism. BB&C's account of the pattern in (6), for example, relies on two independent rules of referral, which are given in (12) (see p. 140 of BB&C); formalization issues aside, these rules are basically identical to the ones in (10).

- (12) a. NOM SG in neuter = ACC SG  
 b. ACC SG in 'vulgus' type = NOM SG

Thus, it seems to me that we end up with an unsatisfactory state of affairs: On the one hand, BB&C argue at length that bidirectionality is a crucial concept underlying certain kinds of syncretism; but on the other hand, in the actual grammatical analysis that the authors provide, bidirectionality does not play any role whatsoever – the concept is simply not incorporated into the theory. This would then seem to suggest that the authors take bidirectional syncretism to be insignificant from the point of view of grammatical theory after all.<sup>16</sup>

<sup>16</sup> Note, however, that this critique is entirely independent of whether one assumes bidirectional syncretism to be theoretically relevant or not; it is the gap between a pre-theoretical look at the data, and the actual theoretical analysis that I take issue with. (The analysis developed in Müller (2007b) does not rely on

## 2.3. Abstract Morphological Features

### 2.3.1. Feature Decomposition

As with the notion of subanalysis, the authors hold an ambiguous position concerning abstract morphological features as they have often been postulated in grammatical theory. An idea going back to Jakobson (1962) and Bierwisch (1967) is that natural classes of cases (in Russian and German, respectively) as they are relevant for syncretism can be captured by decomposing standard case features (like nominative, accusative) into more primitive binary features. On this view, a cross-classification of these features yields the standard cases, and underspecification of exponents with respect to these features accounts for case syncretism. This method has then been applied to other grammatical categories as well: person, number, and so forth. In their short exposition on p. 39, the authors mention Jakobson's approach to Russian (but ignore the extensive discussion of Jakobsonian case decomposition that can be found in Franks (1995)). They also state that whereas the specific abstract features suggested by Jakobson are rarely postulated nowadays, his assumption that "individual morphosyntactic cases are the reflection of some underlying *semantic* network" (my emphasis) remains "widely accepted". This strikes me as debatable in view of the fact that Bierwisch (1967) and many scholars whose work is based on the tradition he initiated (e.g., Wiese (1999, 2003), Wunderlich (1997a,b), to name but a few) assume the primitive case features to be *syntactic* in nature (Bierwisch's features are [ $\pm$ oblique] and [ $\pm$ governed]; Wiese (2003) adds [ $\pm$ subject] for languages with more than four cases).

Later in the book (pp. 52ff), BB&C explicitly argue against (case) feature decomposition as a means to capture syncretism. They state that "evidence for such networks is exceedingly sparse outside of Indo-European." However, they make no attempt to show that decomposition actually fails when confronted with the main bulk of evidence from the world's languages; and, frankly, I see no reason why it should (abstracting away from difficult patterns that the authors rightly focus on, like polarity and bidirectional effects). Thus, it is unclear to me why a collapse of peripheral cases in Caucasian languages like Ingush and Georgian should resist a decomposition approach; the analysis of an identical (albeit partial) syncretism pattern in Archi in Müller (2007a) is based on case feature decomposition (as a result of which the ergative and (depending on the analysis, other) peripheral cases form a natural class excluding the absolutive. Similar reasonings apply to the other examples adduced here.<sup>17</sup>

Interestingly, even though BB&C are critical of invoking abstract features, they are prepared to adopt such features themselves. While discussing nominative/accusative syncretism with neuters in Latin, they contemplate a rule " $\{\text{NOM SG} \cup \text{ACC SG}\} = \text{X}$ " (ex. (17) on p. 135).<sup>18</sup> X is then expanded as "*stem* + -um". X is called an *index*; but it seems clear that this is nothing but an abstract feature encoding a natural class comprising nominative singular and accusative singular contexts. The same tool is put to use on p. 137f, in the analysis of Bonan declension: Here, X is an abstract feature denoting the natural class of accusative and genitive, and Y is an abstract feature that captures the natural class of accusative and dative-locative. Eventually, BB&C decide against analyses of Latin and

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bidirectionality either in an account of data such as the ones in this subsection.)

<sup>17</sup> Also, my hunch is that the argument against a pure underspecification analysis of Dalabon on p. 194 ("underspecification fails when used on its own") is not as strong as it might be because a more abstract analysis that relies on feature decomposition is not envisaged. Furthermore, on p. 129 the authors discuss, and reject, abstract features that correspond to branching nodes in a feature-geometric tree representation. Their counterargument (which is essentially that feature geometry does not suffice to yield all natural classes that are required) strikes me as valid; however, it would lose its force under a Jakobsonian decomposition approach.

<sup>18</sup> Also see Baerman (2005, 812). BB&C give further versions of this kind of rule on pp. 141f.

Bonan using an index feature (in favour of accounts in terms of rules of referral that do not rely on indices, as discussed in the previous subsection), but what is wrong (on their view) with the index-based analyses is not the presence of the index as such, but rather the simple fact that the analyses do not work in the cases at hand.

Yet another use of index features (and, this time, one that is not eventually abandoned) shows up in the authors' analysis of the intricate polarity pattern in Dhaasanac (introduced on p. 106) that is given in the appendix (p. 239): "Second person, third person singular feminine, and first person plural are associated with index B. All other person and number combinations are associated with index A by default."

Thus, not only do BB&C employ abstract features in their morphological analyses; the feature system they use is much less restricted than a system relying on feature decomposition (e.g., it is hard to see how the contexts identified by index B in the analysis of Dhaasanac could be captured as a natural class with the help of primitive decomposed features, given restrictions on inventories of decomposed features as they are standardly assumed).<sup>19</sup>

### 2.3.2. *Inflection Class Features*

Inflection class features are, in a sense, also abstract features because they cannot be motivated on the basis of syntax (or, more generally, outside morphology). Inflection class features form an important part of the authors' analysis of Russian (as argued in Corbett & Fraser (1993) and Fraser & Corbett (1994), the system of Russian noun inflection cannot be derived by invoking only independently motivated features, like gender features, semantic features, or phonological features; but see Wunderlich (1996, 2004) for a different view). Similarly, inflection class is shown to be relevant in the inflectional system of the Australian (Non-Pama-Nyungan) language Gaagudju (p. 91), and the concept shows up in various other parts of the book. However, what BB&C do not discuss in their overview of syncretism with various grammatical categories in ch. 3 is syncretism involving inflectional class – i.e., two (or more) inflection classes share exponents that do not show up in other classes. True, inflection class is not a grammatical category in the traditional sense; but the phenomenon of syncretism spanning inflection classes is real (even if it does not technically involve a case of morphology failing to make a distinction that is made in the syntax because the syntax does not "see" inflection class, and even if it is inherently incompatible with an approach that recognizes only whole-word syncretism), and could have been discussed after the overview over syncretism with grammatical categories, and before the issue of polarity effects (i.e., on p. 103) – at least, this is the point in the book where I missed a discussion of this issue.<sup>20</sup>

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<sup>19</sup> Incidentally, a problem similar to the one discussed in the previous subsection with respect to bidirectional syncretism holds more generally in the case of polarity effects (see BB&C, pp. 103-111). If polarity is a genuine phenomenon (and on p. 111, it is concluded that it can be systematic), then this should arguably be reflected in the theoretical analysis. However, BB&C do not attempt to incorporate a notion of polarity into their analysis, not even in the case of Dhaasanac. Lahne (2007) and Weisser (2007) are two recent attempts, based on BB&C's discussion, that do try to integrate polarity into the analysis.

<sup>20</sup> Also, there are a number of studies dealing with inflection-class-based syncretism by decomposing inflection class features and associating exponents with underspecified inflection class information. See, e.g., Halle (1992) on Latvian noun inflection; Nessel (1994) on Russian noun inflection; Oltra Massuet (1999) on verbal inflection in Catalan; Stump (2001) on verbal inflection in Bulgarian; Müller (2005) on Icelandic noun inflection; Trommer (2005) on Amharic verbs; Opitz (2006) on Albanian noun declension; and Alexiadou & Müller (2007) on Russian, Greek, and German noun inflection. Carstairs-McCarthy (1994) and Noyer (2005) provide accounts of inflection-class-based syncretism that solely rely on radical underspecification with respect to inflection class; these approaches are incompatible with class feature decomposition.

## 2.4. Other Morphological Theories

As remarked at the outset, BB&C's book has many strong points, but I am not sure whether the critical discussion of alternative morphological models can be counted among them. It is good scientific practise to attack the strongest possible version of an alternative theory; but it seems to me that BB&C do not always adhere to this rule. It is also important to distinguish between general, defining features of a theory and (often minor) decisions made in an analysis couched in this general theory that are strictly speaking orthogonal to the basic tenets of the framework and might also be very different. The discussion of the concept of impoverishment that plays an important role in Distributed Morphology is a case in point that illustrates the latter shortcoming. In their discussion of directional effects, BB&C remark (p. 63): "More recently, under Impoverishment Theory (Noyer 1998), it is held that third person is unmarked with respect to other persons". Central assumptions of Distributed Morphology are late insertion, underspecification, and syntactic-structure-all-the-way-down, perhaps even impoverishment (although impoverishment is absent from early work in Distributed Morphology). However, specific assumptions about morphosyntactic features and their status as marked or unmarked have nothing to do with the general theory. In the case at hand, the assumption may be rightfully criticized, but that critique should then not be taken to affect Distributed Morphology (or "Impoverishment Theory") as a whole – one must distinguish between analysis and framework for analysis. Similarly, there is a subsection dedicated to the discussion of whether impoverishment can derive directional effects (pp. 160-163). Relying on Carstairs-McCarthy's (1998) critique of Noyer (1998), BB&C show that "impoverishment makes no consistent predictions ... about directional effects" because it could also help to derive a directional effect in Nimboran that is the opposite of what can be observed. This is certainly the case, but it is not incumbent on impoverishment to do so in the first place; this is solely a matter of the underlying feature structure and the specification of exponents that is postulated.<sup>21</sup> In any event, since it is standardly assumed that impoverishment can only *delete* features (Noyer (1998) is a notable exception in that he postulates that impoverishment can, within certain limits, also change features), it would seem uncontroversial that impoverishment is more restrictive than the means of accounting for syncretism (which is not fully derivable by underspecification alone) that is adopted by BB&C, viz., rules of referral. From this point of view, attacking the concept of impoverishment on the basis of restrictiveness considerations may not be the best way to go.

Another case where an alternative model may not have been treated fully adequately concerns the Minimalist Morphology approach developed by Wunderlich (1996, 1997c, 2004). On pp. 146-147, BB&C present a version of Wunderlich's (2004) optimality-theoretic approach to accusative marking in Russian declension. However, in their exposition, the original feature specifications associated with exponents are modified, and this gives rise to a wrong prediction: In Wunderlich's analysis, *a* is a marker for [+hr] (accusative/genitive) contexts for the regular masculine and neuter inflection classes (see Wunderlich (2004, 381) – note that Wunderlich adheres to stem endings and characteristic nominative markers so as to be able to describe inflection classes without genuine inflection class features). BB&C leave out the inflection class information in the lexical entries of the exponents (ex. (36), p. 146). Consequently, the optimal word form in table (39) (that illustrates the competition for accusative marking of feminine *i*-stems) should be *mater-ja* (which violates none of the three constraints: Compatibility, \*[+hr]/<sub>V</sub>inanimate, and MAX([+hr])), rather than (the

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<sup>21</sup> In fact, the original pattern of syncretism in ex. (60) on p. 162, with a three-out-of-four pattern of syncretism in a paradigmatic space comprising four cells, may just as well be analyzed without invoking impoverishment: The exponent *i* would have to be specified as [-2, -sg, +pl], and *k* would have to be marked [-sg], as in the analysis discussed here.



intended winner) *mat'*; the fatal additional candidate is does not show up in the paradigm. In Wunderlich's analysis, *mater-ja* is blocked here because of a compatibility violation (conflicting inflection class information).

A potential problem that is slightly different in nature but also relates to Wunderlich's Minimalist Morphology model can be found on p. 194 (footnote 8). Here, BB&C discuss Wunderlich's analysis of the Dalabon data, which relies on 'taboos', modelled as optimality-theoretic constraints. As is the case with the Russian example just discussed, these optimality-theoretic constraints have effects that are very similar to the effects that can be obtained by rules of referral, and I think that the authors are correct in pointing this out. However, they then conclude that this "demonstrat[es] that underspecification alone cannot account for the Dalabon data". This is a non-sequitur: If one has shown that there are two analyses that do not rely on underspecification alone in an account of the Dalabon data, this does not imply that there cannot be an underspecification-based analysis that accounts for the evidence.

I believe that similar problems can be pointed out with the authors' criticisms of other theoretical approaches, but I will refrain from doing so here because this is clearly not a central concern of the book (as a matter of fact, at times one gets the impression that the authors consider it an unattractive duty more than anything else to say something about alternative approaches).

## 2.5. The Syntax-Morphology Interface

The title of the book is a misnomer. Syncretism is certainly one of the central phenomena that need to be looked at when investigating the interface of morphology and syntax, but it is not the only one (other relevant phenomena include extended exponence, periphrasis, deponency, bracketing paradoxes, etc.). To the extent that the authors have provided a comprehensive discussion of syncretism, they have made available a rich set of data and generalizations that will be relevant for all actual research on the syntax-morphology interface. However, the book itself does not document any such research. I would like to contend that an investigation of the interface of two components of grammar is possible only from a theoretical perspective; and there is no syntactic theory to be found in this book. As a matter of fact, as has been noted by Juge (2006), there is hardly any discussion of syntax (whether theory-based or not) in the first place. This omission strikes me as unproblematic in most parts of the study; however, there are a few places where invoking syntax (even on the most elementary level) might have improved the analysis (or at least the presentation). For instance, BB&C's discussion of person syncretism in two-place verbs in languages like Guaraní, Koryak, Trung, and Mohave (pp. 75-81) might have benefitted from paying attention to the syntactic argument encoding patterns (ergative, accusative, active) that are involved in these cases. Similarly, it might be that a better understanding of certain complex inflectional systems, as in Diyari and Dalabon, is available if the syntax of argument encoding in these languages is clarified first (see Bittner & Hale (1996), Legate (2006), Bierkandt (2006), and Wunderlich (2001) for relevant discussion).

## 2.6. Conclusion

To sum up, I think that there are a few problematic issues, related to the concept of subanalysis, bidirectionality of syncretism, the abstractness of features, the treatment of alternative theories, and the role of syntax. However, in my view, these in no way diminish the tremendous overall value of the book.

### 3. Appendices

The goal of the two appendices is to show that Pike’s (1965) subanalysis of verb inflection in German is straightforwardly compatible with two current, influential theories of morphology, viz., Paradigm Function Morphology (see Stump (2001)) and Distributed Morphology (see, e.g., Halle & Marantz (1993), Harley & Noyer (2003)). To prove this, I reconstruct Pike’s approach in these two frameworks. It seems clear that this result can be generalized. For instance, it would be unproblematic to implement Pike’s analysis in frameworks like Minimalist Morphology (see Wunderlich (1996, 1997c)) or, indeed, Network Morphology (recall, e.g., the Network Morphology analyses of Dalabon and Dhaasanac); these approaches share many properties with Paradigm Function Morphology and Distributed Morphology, and they both regularly employ subanalysis. Thus, it seems fair to conclude that BB&C’s claim that Pike’s (1965) analysis “is not one which is compatible with most morphological models” (p. 9) is most likely wrong (at least if we restrict “morphological models” to those that are influential to some degree).<sup>22</sup>

#### 3.1. Appendix 1: Pike’s (1965) Subanalysis of German Verb Inflection in Paradigm Function Morphology

In Paradigm Function Morphology (see Stump (2001)), inflection markers are added to stems by morphological realization rules (RRs), which take the abstract form of (13).<sup>23</sup>

$$(13) \text{RR}_{n,\tau,C}(\langle X,\sigma \rangle) =_{def} \langle Y,\sigma \rangle$$

Here,  $\tau$  is the set of morphosyntactic features associated with the inflection marker (the inflection marker emerges as the difference between the stem  $X$  and the inflected form  $Y$ );  $\tau$  can be underspecified. In contrast,  $\sigma$  is the set of morphosyntactic features that the fully inflected word form bears (the analogue to the insertion contexts provided by functional heads in Distributed Morphology, see below). A constraint on rule/argument coherence ensures that  $\sigma$  is an *extension* of  $\tau$ .<sup>24</sup> Given that  $\tau$  may be underspecified, a competition of rules can arise. The competition is resolved by Panini’s Principle, which forces application of the most specific, or ‘narrowest’ rule permitted by the extension requirement (see Stump (2001) for definitions of all the concepts involved here).  $C$  in (13) is the class index; it designates the class of lexemes whose paradigms the rule can help define. Finally, and perhaps most importantly in the present context,  $n$  is a *block index* that stands for the rule block in which the morphological realization rule applies. In a non-morphemic (inferential) approach, standard morpheme positions are captured by rule blocks (see Anderson (1992)); and this straightforwardly opens the door to subanalysis.

Realization rules come in two types: On the one hand, there are rules of exponence. These rules are the main building blocks of inflectional systems; they correlate morphological exponents (inflection markers) with fully specified morphosyntactic feature structures. On the other hand, there are rules of referral. Rules of referral state that the exponent for some given feature specification takes the form of the exponent determined independently for some other feature specification. Both types of realization rules can capture

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<sup>22</sup> I hasten to add that I do not wish to claim that Pike’s (1965) analysis is superior to others, under either the Paradigm Function Morphology or the Distributed Morphology version. I do not want to argue for the correctness of the approach; I merely want to show that it is compatible with current frameworks.

<sup>23</sup> This is a simplification. To integrate the effect of additional morphophonological rules,  $Y$  in (13) should be replaced by  $Y'$ , with the convention added that  $Y'$  differs from  $Y$  only to the extent required by the morphophonological rules. I omit this complication here because morphophonological rules will not play a role.

<sup>24</sup> This is comparable to the subset and compatibility requirements of other morphological theories like Distributed Morphology (Halle & Marantz (1993), Harley & Noyer (2003)) and Minimalist Morphology (Wunderlich (1996, 1997c)), respectively.

syncretism, the former via underspecification, the latter by stipulation. Underspecification is a particularly powerful tool when combined with a more abstract feature system, gained by decomposition of the standard morphosyntactic features as they suggest themselves on the basis of syntactic evidence alone (see Jakobson (1962), Bierwisch (1967)). Although there is nothing in the theory that would preclude the use of abstract morpho-syntactic features, Stump minimizes the use of these kinds of features. Consequently, many instances of syncretism are derived by rules of referral.

Against this background, consider again Pike's (1965) subanalysis of German verb inflection. (14) is slightly modified from (1), with the vertical lines identified with five separate rule blocks that the realization rules are sensitive to.

(14) *Pike's (1965) subanalysis of verb inflection with sein ('be') in German:*

	A	B	C	D	E
1.sg	b		ɪ	n	
2.sg	b		ɪ	s	t
3.sg			ɪ	s	t
1.pl	z		ɪ	n	t
2.pl	z	a	ɪ		t
3.pl	z		ɪ	n	t
inf	z	a	ɪ	n	

Abstracting away from the infinitive for the time being, a Paradigm Function Morphology analysis capturing Pike's main insights (particularly the subanalysis of the markers) might then look as in (15).<sup>25</sup>

(15) *Realization rules in Paradigm Function Morphology*

a. **Block A:**

**A1**  $RR_{A,\{AGR:\{PER:3,NUM:sg\}\},sein}(\langle X,\sigma \rangle) =_{def} \langle Y,\sigma \rangle$ , where Y is X's First Stem.

**A2**  $RR_{A,\{AGR:\{NUM:sg\}\},sein}(\langle X,\sigma \rangle) =_{def} \langle Y,\sigma \rangle$ , where Y is X's Second Stem.

**A3**  $RR_{A,\{\},sein}(\langle X,\sigma \rangle) =_{def} \langle Y,\sigma \rangle$ , where Y is X's Third Stem.

b. **Block B:**

**B1**  $RR_{B,\{AGR:\{PER:2,NUM:pl\}\},sein}(\langle X,\sigma \rangle) =_{def} \langle Xa,\sigma \rangle$ .

c. **Block C:**

**C1**  $RR_{C,\{\},sein}(\langle X,\sigma \rangle) =_{def} \langle Xi,\sigma \rangle$ .

d. **Block D:**

**D1**  $RR_{D,\{AGR:\{PER:2,NUM:pl\}\},sein}(\langle X,\sigma \rangle) =_{def} \langle X,\sigma \rangle$ .

**D2**  $RR_{D,\{AGR:\{NUM:sg\}\},sein}(\langle X,\sigma \rangle) =_{def} \langle Xs,\sigma \rangle$ .

**D3**  $RR_{D,\{\},sein}(\langle X,\sigma \rangle) =_{def} \langle Xn,\sigma \rangle$ .

**D4**  $RR_{D,\{AGR:\{PER:1,NUM:sg\}\},sein}(\langle X,\sigma \rangle) =_{def} \langle Y,\sigma \rangle$ , where  
 $Nar_D(\langle X,\sigma / \{AGR:\{Num:pl\}\} \rangle) = \langle Y,\sigma / \{AGR:\{Num:pl\}\} \rangle$

e. **Block E:**

**E1**  $RR_{E,\{AGR:\{PER:1,NUM:sg\}\},sein}(\langle X,\sigma \rangle) =_{def} \langle X,\sigma \rangle$ .

**E2**  $RR_{E,\{\},sein}(\langle X,\sigma \rangle) =_{def} \langle Xt,\sigma \rangle$ .

First, suppose that, as a lexical property, there are three stems associated with the verb *sein* ('be') in German: The First stem does not have a phonological realization; the Second stem is *b*, and the Third stem is *z*. Choice of stem exponent is regulated by the rules in block A.<sup>26</sup> Note that the rules' application domain (as encoded by the class index, i.e., *C*

<sup>25</sup> The analysis given here is modelled on Stump's (2001) accounts of verb inflection in Bulgarian and Romanian.

<sup>26</sup> See Stump (2001, 34, 45) for such a treatment of stem alternation in Bulgarian verb inflection. Note that there would be straightforward ways to avoid a zero stem if required – e.g., one might postulate a

in (13)) is just one single lexeme; this is permitted in Paradigm Function Morphology (see, e.g., Stump (2001, 219) on Romanian verb inflection, and p. 12 above). A1 is narrower (more specific) than A2 and A3, and consequently blocks application of A2 and A3 in third person singular contexts (where these rules would also be applicable in principle), by Panini's Principle. A2 is narrower than A3 and blocks it in singular contexts (where A3 would also be applicable in principle). Thus, A3 can only apply in the plural.

Block B only has one specific rule of exponence, viz., B1, which introduces *a* (recall that Pike assumes that the seemingly non-composite diphthong *ei* ([ai]) in (14) is composed of two separate markers *a* and *i*). B1 is not applicable in contexts which are not second person plural, and there is no other rule available in the rule block that is. In cases like these, a universal default realization rule applies, viz., the Identity Function Default (Stump (2001, 53)); see (16). This rule can apply in all rule blocks ( $n$  is a variable over rule blocks), and in all lexical classes (signalled by  $U$ ); it is compatible with all full specifications ( $\{ \}$ , which designates the empty set of the morphosyntactic properties of a language), and maps a form  $X$  onto itself, thereby producing the many empty cells in the B block. (This rule ensures that there are no paradigmatic gaps; but it is often blocked by more specific rules, and therefore limited in its effects.)

(16) *Identity Function Default:*

$$RR_{n, \{ \}, U}(\langle X, \sigma \rangle) =_{def} \langle X, \sigma \rangle$$

The C block also has only one rule, C1; C1 is applicable in all contexts with the verb *sein* ('be'), introducing *i* in the C block in all cells.

Block D has four rules that introduce three exponents ( $n$ ,  $s$ , and zero). D1, D2, and D3 are standard rules of exponence. D3 assigns the exponent  $n$  throughout. It is blocked by the more specific rule D1 for second person plural contexts which maps  $X$  onto  $X$  and thereby captures the absence of an exponent in this block.<sup>27</sup> D3 is also blocked by the more specific rule D2 which introduces an  $s$  in the singular. (D1 and D2 do not compete.) The fourth rule of block D differs from all the other rules discussed so far in that it is not a rule of exponence, but a rule of referral. Basically, D4 states that the block D exponent for first person singular specifications is whatever is independently determined by the system as the exponent for first person plural specifications (" $\sigma / \{AGR: \{Num: pl\}\}$ " is to be read as " $\sigma$  modified by  $\{AGR: \{Num: pl\}\}$ ", i.e. a feature specification that differs from the original  $\sigma$  minimally in that the number value is plural, whatever it was before; all other feature specifications stay the same). Thus, in this analysis, the block D syncretism involving  $n$  in (14) is captured by (radical) underspecification in the plural, and by referral in the singular: On this view, first person singular  $n$  with *sein* is a take-over from the first person plural  $n$  (which, incidentally, does not seem implausible, given that  $n$  otherwise never shows up in the singular of verb inflection in German, whereas it does in the plural). The rule of referral D4 interacts with the rules of exponence D1, D2, and D3 in the regular way, as predicted by Panini's Principle; i.e., it competes with (and blocks) D3 and D2, but not D1.

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segmental stem exponent that is subsequently deleted by a morphophonological rule; one might postulate a segmental marker ? as the exponent; and so on.

<sup>27</sup> Note that this implies that zero exponence can arise in a rule block either as a result of the maximally non-specific Identity Function Default (as in block C), or as the result of a highly specific realization rule (as in blocks A and D). Again, this dual source for zero exponence is a regular feature of Paradigm Function Morphology analyses (cf., e.g. the highly specific rule B4/C1 effecting zero exponence in the analysis of Bulgarian verb inflection given in Stump (2001, 45)); highly specific rules introducing zero exponents that serve to block other exponents figure prominently in the analysis of Potawatomi developed in Anderson (1992, 167ff); also see Halle & Marantz (1993, 161f) for discussion. – Incidentally, the situation is similar in Distributed Morphology. Trommer (1999) has shown that highly specific rules introducing zero exponents can be viewed as replacements for standard rules of impoverishment.

Finally, in block E, E2 is a non-specific rule introducing a final *t* exponent, and E1 is a narrower rule that introduces zero exponence in first person singular contexts.

So far, the infinitive form *zarn* is not predicted by the analysis (it should be *zmt*). The correct form is predicted if we assume two further rules of referral: one ensuring that the infinitive's exponent for block B is the exponent for second person plural; and one ensuring that the infinitive's exponent for block E is the exponent for first person singular.

As it stands, all relevant realization rules (except for (16)) have the verb *sein* as the application domain. However, as noted by Pike, the exponents in the last two blocks (D and E) are exactly those employed by the two remaining verb inflection classes in German (weak and strong conjugation), viz., *s*, *n*, and *t*. Thus, the obvious next step would be to extend the analysis in such a way that it also captures the two main inflection classes. This is beyond the scope of the present reconstruction of Pike's analysis. Still, let me sketch two possible strategies, which have in common that the rules D1, D2, D3, E1, and E2 (but not the referral rule D4) have the whole class of verbs (*V*), rather than just the verb *sein*, as their application domain. If nothing more is said, the predictions are correct for first person singular contexts (assuming that  $\emptyset$  insertion is morphophonological: *glaub-e* ('I believe')); for second person singular contexts (*glaub-s-t* ('You<sub>sg</sub> believe')); and for second person plural contexts (*glaub-t* ('You<sub>plur</sub> believe')). In third person singular contexts, the additional *s* would have to go: *glaub-t* ('He believes') instead of \**glaub-s-t* ('He believes'). Similarly, there is one exponent too many in first and third person plural contexts: It is *glaub-n* ('We/they believe') instead of \**glaub-n-t* ('We/they believe'). The absence of one of the two exponents in the clusters *s-t* and *n-t* in these contexts could plausibly be assumed to go back to morphophonological rules that block double exponence in blocks D and E in all contexts except for second person singular. Alternatively, one might assume that there are additional rules that ensure zero exponence in these contexts in the case of strong and weak verb inflections in German. Whatever direction is ultimately pursued, we may conclude that the fragment in (15) can be extended so as to capture the system of German verb inflection as a whole.<sup>28</sup>

To sum up, assuming Pike's subanalysis of verb inflection with *sein* in (14), there are a priori 35 different exponents; the Paradigm Function Morphology analysis outlined here needs only 13 realization rules to derive the whole set of markers (excluding the Identity Function Default rule, which is independent of the analysis). With the exception of some cases of zero exponence and the referral involving *n* in first person singular contexts in block D, there is only one rule for each of the exponents.

### 3.2. Appendix 2: Pike's (1965) Subanalysis of German Verb Inflection in Distributed Morphology

In Distributed Morphology (see, e.g., Halle & Marantz (1993), Noyer (1992), Halle (1997), Harley & Noyer (2003)), functional heads in syntax provide fully specified contexts for post-syntactic insertion of vocabulary items (post-syntactic insertion is also sometimes assumed to affect lexical heads); and whereas the functional heads are characterized by fully specified morphosyntactic features (with one qualification – see below), the vocabulary items can be (and often are) underspecified with respect to these features. A Subset Principle ensures that a vocabulary item can only be inserted if it does not bear features which contradict those in the functional morpheme in syntax. Given underspecification, a competition of vocabulary items can arise. This competition is resolved by a Specificity Condition that is usually formulated as part of the Subset Principle, and that guarantees that only the most specific vocabulary item (among those that fit) is selected for insertion.

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<sup>28</sup> Note in passing that a subanalysis of *s-t* exponents in German verb inflection is independently argued for in Müller (2006); the analysis there focusses on strong and weak conjugations and does not envisage a subanalysis of *sein* inflection, though.

Crucially, assuming vocabulary insertion to be post-syntactic opens up the possibility of operations applying after syntax but before morphological insertion that change the morphosyntactic feature specification. This way, systematic mismatches between morphology and syntax can be accounted for. Perhaps the most important such operation is impoverishment. Impoverishment deletes features (that are required in syntax) before morphology is reached, and thereby effects a “retreat to the general case”: An exponent that would have to be selected for a given non-impoverished context (because it both fits, and is most specific among its competitors) may cease to fit into the context if impoverishment has taken place, and then a less specific marker will have to be chosen. A further relevant concept is fission. In Halle & Marantz (1993, 1994), fission is viewed as a post-syntactic (and pre-morphological) operation that splits up a functional head by dislocating morphosyntactic features in syntactic phrase structures; this creates two positions that are then separately targeted by insertion. An alternative concept of fission is proposed in Noyer (1992): On this view, insertion of the most specific vocabulary item into a fissioned functional head leaves those features of the functional head which are not matched by features of this (underspecified) vocabulary item (and thereby discharged) available for further vocabulary insertion, and so on, until there is no inflection marker left that can be inserted in accordance with the Subset Principle. It is this latter concept of fission that I will presuppose in what follows. Finally, Distributed Morphology has so-called readjustment rules that apply after vocabulary insertion. These additional rules are akin to the morphophonological rules in Paradigm Function Morphology. They also have the power to insert inflectional exponents (see, e.g., Halle (1992, 1994) on noun inflection in Latvian and Russian). Fission, readjustment rules, and insertion into terminal nodes in the syntax all lend themselves to subanalysis (the latter under the assumption, often made in Distributed Morphology work, that syntactic phrase structures involve a relatively large number of functional heads).

Before I present a reconstruction of Pike’s basic analysis in Distributed Morphology, one further issue needs to be addressed: Work in Distributed Morphology often relies on a decomposition of standard features encoding instantiations of grammatical categories.<sup>29</sup> In line with this, I will assume a decomposition of person features into primitive features [ $\pm 1$ ], [ $\pm 2$ ], and [ $\pm 3$ ] (see Noyer (1992), Wiese (1994), Frampton (2002)), Trommer (2006a,b), Nevins (2007)). This feature system predicts that, given cross-classification of the primitive features, all combinations of persons (including first person inclusive) can form a natural class, reflected in syncretism patterns.<sup>30</sup> This conforms to the findings of Cysouw (2003) and BB&C. With all this in mind, let me now turn to the analysis of Pike’s paradigm.

Suppose first that head movement in the syntax has created complex heads like the one in (17) (either by raising or by lowering; for present purposes, this does not play a role – and neither does the categorial labelling of the complex head). V is the lexical head; at least in the case of *sein* (‘be’), this stem position is filled only post-syntactically, by vocabulary insertion. Next, Th is a theme vowel position associated with the lexical head.<sup>31</sup> Finally, Agr contains  $\Phi$ -features (relevant in the present contexts are person and number, which can be morphologically realized).<sup>32</sup> Th and Agr are both subject to fission

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<sup>29</sup> As noted, this tendency is in contrast to what is the case with Paradigm Function Morphology. Still, it is worth keeping in mind that there is nothing inherent in any of the two models that would force a decision either for or against abstract features gained by decomposition.

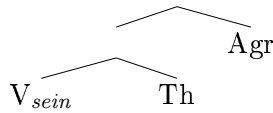
<sup>30</sup> Some of the eight persons derivable from cross-classifying the three binary features will be unavailable due to contradictory interpretations associated with the primitive features.

<sup>31</sup> See Halle (1992, 1994), Halle & Marantz (1994), and Oltra Massuet (1999). For present purposes, it is irrelevant at what point Th enters the syntactic derivation (options include base-generation and late insertion by dissociation, on which see Embick & Noyer (2001)); and whether it heads a projection or not (although the standard assumption would seem to be that it does not).

<sup>32</sup> It is usually assumed in work on Distributed Morphology that there is also a further separate functional head T(ense) (but see Chomsky (1995, 2005) for a different view). I will abstract away from this because

in Noyer’s (1992) sense (whether or not fission is assumed for  $V_{sein}$  does not matter).

(17)



The pattern in (14) can then be derived on the basis of the rules of vocabulary insertion in (18); these rules pair a phonological exponent (the vocabulary item) with a so-called insertion context (which is often underspecified, in analogy to Paradigm Function Morphology’s  $\tau$  – note that “insertion context” does not refer to the actual syntactic context of insertion –  $\sigma$  in Paradigm Function Morphology –, but rather to the features that are associated with the vocabulary item).<sup>33</sup>

(18) *Vocabulary insertion rules in Distributed Morphology*

- a. (i) /b/  $\leftrightarrow V_{sein} / \_ [-3, -pl]$
- (ii) /z/  $\leftrightarrow V_{sein} / \_ [+pl]$
- b. (i) /a/  $\leftrightarrow [+ \beta] / \_ V_{sein}, [-1, +2, +pl]$
- (ii) /ɪ/  $\leftrightarrow [+ \alpha] / \_ V_{sein}$
- c. (i) /∅/  $\leftrightarrow [-1, +2] / \_ V_{sein}, [+pl]$
- (ii) /s/  $\leftrightarrow [-1, \pm 2] / \_ V_{sein}, [-pl]$
- (iii) /n/  $\leftrightarrow [-2] / \_ V_{sein}$
- (iv) /∅/  $\leftrightarrow [-pl] / \_ V_{sein}, [+1]$
- (v) /t/  $\leftrightarrow V_{sein}, [\pm pl]$

The three groups of vocabulary insertion rules in (18) correspond to the five rule blocks in the previous section as follows: The rules in (18-a-i/ii) are counterparts of A1–A3 in block A; those in (18-b-i/ii) of B1 and C1 in blocks B and C; and those in (18-c-i/ii/iii/iv) of D1–D4 and E1–E2 in blocks D and E. Having fewer functional heads than rule blocks is a result of the option of fission. The rules in (18-a-i/ii) select the stem exponent: /b/ or /z/ if one of the two rules is applicable, and zero exponence if neither of the two rules is applicable. The rules rely on contextual features; these are not discharged by insertion in the case of fissioned heads.<sup>34</sup> The necessity for contextual features arises because the system (under Pike’s analysis) displays extended (multiple) exponence (in Matthews’s (1972) sense), a fact already noted by Pike (1965, 196) (see above). Note that the availability of a natural class comprising first and second person (encoded by the feature [-3]) makes it possible to dispense with a special rule introducing zero marking for third person singular contexts (as in the Paradigm Function Morphology Analysis).

Turning next to (18-b-i/ii), abstract features like [+ $\alpha$ ], [+ $\beta$ ] are adopted from Oltra Masuet (1999, 10ff), where they are also responsible for theme vowel selection. The assumption here is that  $V_{sein}$  is associated with a Th position bearing the features [+ $\alpha$ , + $\beta$ ]. The vocabulary item /ɪ/ (realizing [+ $\alpha$ ]) is inserted into all Th positions; in addition, an additional *a* (realizing [+ $\beta$ ]) is inserted into Th in second person plural contexts. To get the order of /a/ and /ɪ/ right, it can be postulated that relative specificity is not to be measured in terms of size of feature sets (or subset relations), but rather in terms of a hierarchy of features, with size of feature specifications a secondary factor. Then, assuming that [+ $\beta$ ] outranks [+ $\alpha$ ], and given the Strict Cycle Condition (which demands that successive

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I focus solely on present tense inflection, even though it is conceivable that T might replace Th in the present approach to *sein* conjugation.

<sup>33</sup> The notation follows standard Distributed Morphology practise. Note that ‘±’ basically instantiates the use of variables of feature values, i.e.,  $\alpha$  notation.

<sup>34</sup> Note that the /  $\_$  notation is supposed to be neutral with respect to linear order.

insertion applies cyclically, always at the outside of the form created so far), insertion of /ɪ/ follows insertion of /a/, and the latter must linearly precede the former since both exponents are suffixal.<sup>35</sup>

Finally, as concerns (18-c-i/ii/iii/iv), suppose that person features are inherently more specific than number features; that  $[\pm 1]$  is more specific than  $[\pm 2]$ ; and that  $[-pl]$  (and  $[+pl]$ ) are more specific than  $[\pm pl]$  – which seems natural (contextual features do not count for specificity). Under these assumptions, the paradigm in (14) is basically derived.<sup>36</sup> As with the Paradigm Function Morphology analysis, something extra needs to be said for the infinitive form, and the analysis needs to be generalized in the (18-c) domain to verb inflection in general. I think that neither of these tasks poses particular obstacles; but such extensions are again beyond the scope of this appendix.

To conclude, a Distributed Morphology analysis based on Pike's (1965) approach to verb inflection with *sein* in German is quite straightforward, and does not look too different from existing analyses of verb inflection in other languages (and other domains).<sup>37</sup> As before, most of the instances of syncretism (postulates under Pike's subanalysis) are derived systematically, and only zero exponence requires more than one rule.

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<sup>35</sup> A viable alternative would be to assume that the /a/ part of the diphthong /a-ɪ/ arises as a consequence of the application of a late readjustment rule. In that case, rule (18-b-ii) could be dispensed with, as could the feature  $[+\beta]$ .

<sup>36</sup> Note that the vocabulary items in (18-c) do not refer to  $[\pm 3]$  even though the feature is present in Agr (and needed for (18-a)).

<sup>37</sup> The analysis has not made use of impoverishment. However, one might want to add an impoverishment rule to the system that ensures that the difference between first and third person is always neutralized with respect to morphological exponence in plural contexts – not only with *sein*, but also with weak and strong conjugations. See Frampton (2002) and Müller (2006).



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