

**DIRECTIONALITY AND LOCALITY IN ALLOMORPHY: A RESPONSE TO
ADGER, BÉJAR AND HARBOUR**

By ANDREW CARSTAIRS-McCARTHY

University of Canterbury, Christchurch

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Adger, Béjar and Harbour (this volume; henceforth ABH) criticise Carstairs-McCarthy (henceforth C-M) (2001b) on several points. I argue that most of ABH's criticisms are mistaken. However, they make an interesting proposal concerning locality that deserves further investigation.

1. 'A SIMPLE FALLACY OF LOGIC'?

C-M discusses the distribution of the two plural suffixes in Hungarian: the normal suffix *-ok*¹ (in e.g. *dal-ok* 'songs'), and the special possessed-plural suffix *-i* that accompanies markers of possession (e.g. *dal-a-i-m* 'my songs', *dal-a-i-d* 'your songs' etc.; compare *dal-om* 'my song', *dal-od* 'your song'). He describes a hypothetical language (Pseudo-Hungarian) that is like actual Hungarian except in that it uses the usual plural suffix *-ok* when the possessor is first person singular ("*dal-ok-am*" 'my songs', alongside *dal-a-id* 'your songs' etc.). In Bobaljik's framework, there is no reason why a language like Pseudo-Hungarian should not exist, with piecemeal outward sensitivity (i.e. sensitivity to some possessors and not others). C-M suggests however that piecemeal outward

sensitivity cannot occur, so Pseudo-Hungarian is an impossible language. C-M's framework is thus more restrictive than Bobaljik's in this respect, and makes stronger empirical predictions about what can and cannot happen in allomorphic conditioning.

ABH complain that C-M is guilty here of 'a simple fallacy of logic ... A theory that predicts non-existence of x is not supported by not finding a language exhibiting x .' But it is ABH whose reasoning is fallacious. Consider two rival theories of gravity: a more restrictive one, according to which acceleration due to gravity in a vacuum at the earth's surface is 9.81ms^{-2} , and a less restrictive one, according to which this acceleration can vary between 9ms^{-2} and 11ms^{-2} . Most of us are happy to acknowledge that the more restrictive theory is supported by the fact that we do not find objects whose acceleration is greater or less than 9.81ms^{-2} . ABH, however, would be forced by parity of reasoning to say that our failure to find such objects does not support the more restrictive theory. Physicists since Newton have been guilty of the same 'simple fallacy' as C-M, it would seem!

What ABH are entitled to say is that the non-existence of Pseudo-Hungarian does not guarantee that some language that violates C-M's prediction will not be found tomorrow. But that merely confirms that C-M's prediction is like all good empirical predictions, in that it is clear what would count as counterevidence (at least *prima facie*). Yet, in the years since C-M formulated the first version of his claim (Carstairs 1987), it has held up well, in the sense that no substantial body of counterevidence to it has accumulated.

¹ I ignore here the variability of the vowel that often shows up before the k and before possessive suffixes. According to the context this may be low or nonlow, and it is also subject to backness and rounding

2. DISCONTINUITY, ‘CLASS MORPHOLOGY’ AND ‘SYNTACTIC CATEGORIES’ IN ITELLEN

ABH’s rejection of C-M’s analysis of Itellen rests mainly on three assertions. Firstly, ‘the realisation of class morphology’ should be handled in the same way as ‘inflectional morphology’. Secondly, Distributed Morphology provides an adequate theory of discontinuous realisation. Thirdly, ‘syntactic categories’ are simply ‘bundles of features’, so that ‘the distinction between feature categories [Matthews’s (1991) ‘morphosyntactic categories’] and individual features [Matthews’s ‘morphosyntactic properties’] is an artificial one’ and C-M’s assignment of them to nonterminal and terminal nodes respectively must be a mistake.

By ‘class morphology’ in Itellen, ABH mean a set of affixes that occur only in one verbal inflection class and that vary according to tense and agreement features realised further out in the wordform. But there is good reason to treat such affixes as different from ordinary inflectional affixes, in that they realise nothing—or rather, what they ‘realise’ is simply the inflection class of the lexeme that they appear with. They are akin to stem vowels, and their appearance amounts to a kind of stem alternation. But there is by now a fair amount of evidence that stem alternation plays by different rules from affixal inflection and interacts with it in subtle ways (e.g. Aronoff 1994; Cameron-Faulkner and Carstairs-McCarthy 2000; Carstairs-McCarthy 2001a). ABH’s first assertion simply neglects this evidence, without discussion.

Discontinuous realisation is a puzzle for all morphologists, and is a principal reason why some (e.g. Stump 2001) reject entirely the kind of hierarchical structure (with morphemes at terminal nodes on trees) that Bobaljik, ABH and C-M all accept, at least for the examples under discussion. ABH say that Bobaljik’s theory provides a

mechanism for the handling discontinuous realisation that ‘correctly captures the form of the various morphemes in Itelmen’. But, in the absence of further detail, this seems to amount to no more than that Bobaljik’s theory is loose enough with respect to discontinuous realisation not to be incommoded by this phenomenon in Itelmen. In this respect, Bobaljik’s approach is no worse than C-M’s, but also no better.

In support of their third assertion, ABH invoke the authority of Chomsky (1965). However, they seem confused over the term ‘syntactic category’. It is true that Chomsky introduced the analysis of lexical categories and subcategories (nouns, common nouns, etc.) in terms of binary features such as $[\pm N]$, $[\pm \text{Common}]$ and $[\pm \text{Animate}]$, and then or shortly after it became commonplace to analyse adjectives as $[+N, +V]$, and prepositions as $[-N, -V]$. But the categories that C-M discusses are not these; rather they are grammatical entities such as Person, Number, Tense and Mood, each with a set of values (‘properties’, in Matthews’s terminology) such as first person, singular, past, and subjunctive. Despite what ABH imply, Chomsky himself (1965:170-5) handles categories in this sense in a similar fashion: he uses expressions such as $[\alpha \text{Gender}]$ and $[\alpha \text{Number}]$, where Gender and Number equate to Matthewsian categories, and where the Greek letters indicate a range of values (‘properties’) that is not necessarily limited to two. It may turn out to be the case that the set of properties within a category can usefully be analysed in terms of binary features. However, that is not a reason to question the validity of the categories that contain those properties, and their validity is evidently assumed by Bobaljik too, in that he has nonterminal nodes with category labels such as T[ense] and AgrO dominating terminal nodes with property labels such as [PRES] and [3PL]. C-M’s word trees are just like Bobaljik’s in this respect.

3. LOCALITY IN INWARD SENSITIVITY

Latin verbs have a peculiar person-number screeveshape (or set of suffixes) found in just the perfective present indicative screeve. There are two points of interest here: (i) the limitation of the screeveshape to just this screeve, and (ii) the limitation of some of the suffixes in question to just this screeveshape.

With respect to point (i), ABH suggest that it is not an accident that the unusual [2SG] perfective suffix *-isti*: (contrasting with *-s*) is found just in the present indicative, not in subjunctive or past tense forms. They suggest that this is because only in the present indicative is the node at which agreement suffixes are inserted a sister of the Aspect node (which, in their analysis, not only dominates a node labelled Perfect, but is itself labelled Perfect, because the mother node acquires, or retains, the feature specification of the daughter). This is an interesting suggestion. What it amounts to is that the unmarked tense (present) and the unmarked mood (indicative) are not represented in the tree, just by virtue of being unmarked, so that, in present indicative verbforms, no Tense or Mood nodes intervene in the tree between Aspect and Agreement. On this basis, they propose a relaxation of Bobaljik's ban on inward sensitivity, but in a strictly 'local' fashion, namely when the sensitive node is a sister rather than an aunt or a greataunt of the conditioning node.

Whether inward sensitivity is always local in this fashion is an important question, to which I will return later. Meanwhile, let us assume that ABH's claim is correct. Is it inexpressible in C-M's ancestry model, as they claim? The answer is no. In the tree diagram for *ama:-u-isti*: at (14) in C-M (2001b), ABH's suggestion obliterates

the branching Tense-Mood node with its right daughter [Pres Indic], and the [Perf] node thus becomes the grand-daughter, rather than the great-grand-daughter, of the topmost Agr node. So ABH's locality suggestion can be expressed as a restriction on ancestral distance: a node may be sensitive 'downwards' only to the head of its daughter node (equivalently, the head of the material contained within the immediately preceding cycle), and no further. ABH are wrong to say that their claim is 'a generalisation that cannot be incorporated into [C-M's] analysis'.

Although ABH's amendment does not discriminate between Bobaljik's and C-M's frameworks with respect to point (i), there is still an important difference between them with respect to point (ii). ABH's vocabulary item *-isti:* (item (5) in their article in this volume) deals only with one person-number combination. Nothing in what they say predicts that other cells behave in parallel: not only [2SG] *-isti:* but also [1SG] *-i:*, [2PL] *-istis* and [3PL] *-e:re* or *-e(:)runt* are restricted to the perfective present indicative. In their variant of Bobaljik's framework, there appears to be nothing to exclude a Pseudo-Latin in which (for example), of these four affixes, [2SG] *-isti:* alone appears in not only perfective but also past tense contexts. This Pseudo-Latin will differ crucially from actual Latin in its screeveshapes: instead of the three given at (13) in C-M (2001b), there will now be four, including a new variant of the m-shape for past tense contexts, with *-isti:* instead of *-s*. (The old variant of the m-shape will still appear in nonpast contexts such as the perfective and imperfective present subjunctive.) This new screeveshape, however, falls foul of C-M's predictions about 'blur avoidance' in screeveshapes. Both of the two [2SG] suffixes in Pseudo-Latin appear in two screeveshapes: *-isti:* in the i-shape and the new variant of the m-shape, and *-s* in the o-shape and the old variant of the

m-shape. Thus neither of them is either a screeveshape-identifier or a screeveshape-default. To exclude such a possibility, we need to not merely associate terminal nodes with morphemes, as Bobaljik and ABH do, but also associate certain nonterminal nodes with screeveshapes, as C-M does.

C-M's framework thus continues to have the advantage over Bobaljik's in that it is more restrictive, permitting actual Latin while excluding Pseudo-Latin (for example). It is of course an open question whether this restriction can be maintained in general. But it is empirically rich, and has the advantage of linking screeveshape behaviour to both inflection class behaviour (Carstairs-McCarthy 1994) and general restrictions on the meaning of vocabulary items (Carstairs-McCarthy 1998a; 1998b). These links blunt the force of ABH's complaint that the Ancestry Constraint is stipulative rather than explanatory.

I postponed earlier the issue of whether the locality restriction is empirically correct. I have argued elsewhere that, of the three Latin [1SG] suffixes *-m*, *-o:* and *-i:*, it is *-o:* which is the default, with *-m* realising not just [1SG] but [1SG, PAST OR SUBJUNCTIVE] (Carstairs-McCarthy 1998b). If we express this in terms of screeveshapes, it limits the m-shape to contexts that are past or subjunctive or both (with some future indicative forms too, handled by a rule of referral). If we assume that Tense is higher in the tree than Mood, then the sensitivity of the m-shape to [PAST] will be local in the sense required by ABH's proposal. This takes care of past subjunctive as well as past indicative forms. The only other tense compatible with the subjunctive mood in Latin is the present (there being no future subjunctive). But, if the present tense, being unmarked, is unrepresented in the tree (in line with ABH's proposal), then the Agr node in present

subjunctive forms will be close enough to the Mood node for the locality requirement to be met, in C-M's framework as well as in Bobaljik's.

ABH are thus wrong in thinking that their locality restriction can be used as a weapon against C-M's framework. However, it looks like a useful contribution to the growing battery of constraints on allomorphic conditioning. *Prima facie* counterexamples to locality in morphology certainly exist (Carstairs-McCarthy 1992:68-70). However, these generally seem to involve either stem alternation (e.g. the special passive stem in some Zulu verbs) or else lexical idiosyncrasy (e.g. deponent verbs in Latin)—that is, situations where more is going on than just the affixal realisation of inflectional properties. This lends weight to a distinction I drew earlier, between on the one hand 'class morphology' and stem alternation, and on the other hand the affixal realisation of morphosyntactic properties (what one is inclined to think of as inflectional morphology 'proper').

4. CONCLUSION

Despite my disagreements with ABH, I applaud their interest in trying to falsify C-M's proposals. An approach to morphology in which any conceivable morphological behaviour can be accommodated is not much use. Neither Bobaljik nor C-M falls into that trap. But I continue to be puzzled by the reluctance of some DM-oriented morphologists to acknowledge the importance of the paradigmatic dimension in morphological exponence, as manifested in constraints on screeveshapes and inflection class organisation. As Halle has said (1973:9): 'there is every reason to expect that

paradigms must appear as entities in their own right somewhere in the grammar'. Much evidence that has accumulated since then confirms that judgement.

Department of Linguistics

University of Canterbury

Private Bag 4800

Christchurch

New Zealand

e-mail andrew.carstairs-mccarthy@canterbury.ac.nz

website <http://www.ling.canterbury.ac.nz/>

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