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What Does Comparative Markedness Explain,

What Should It Explain, and How?

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1. Introduction

These seven commentaries treat a wide range of topics in interesting and insightful ways. It is not possible to write a coherent response that addresses all of the criticisms and suggestions, large and small, that the authors have brought up. Several main themes emerge, however, that transcend the individual commentaries, and these themes supply the structure for this reply. They include alternatives to comparative markedness, possible counterexamples, comparative markedness on other dimensions of correspondence, and questions about the authenticity of opaque phonological processes. These themes will each be addressed in turn.

2. Local Conjunction and Comparative Markedness

In earlier work (Łubowicz 2002), some of which is summarized in section4.2 of my article, Łubowicz showed how derived environment effects (DEE's) can be obtained from the local conjunction of markedness and faithfulness constraints. In her commentary, Łubowicz goes on to propose a novel way of looking at counter-feeding opacity, specifically chain-shifts, using local conjunction.

The idea is that a chain-shift occurs when the markedness constraint against the middle step of the chain is locally conjoined with an anti-faithfulness constraint (for which see Alderete 1998, 2001). In a language with /a/ → *e* and /e/ → *i* in some context C, the markedness constraint **e*/C is ranked below IDENT(hi), so it is not generally active. But the local conjunction of **e*/C with the anti-faithfulness constraint ¬IDENT(low) — that is, $[*e/C \ \& \ \neg\text{IDENT(low)}]_{Seg}$ — is ranked above IDENT(hi). This conjoined constraint is violated by surface *e*s that are not derived from a low vowel, because those *e*s are faithful to the feature [low] and hence violate ¬IDENT(low). The net effect is that underlying /e/ raises to *i*, but any *e*s from /a/ will not.

In the case Łubowicz discusses, the ranking is as given in (1a), but ranking permutation supplies another possibility, which is shown in (1b).

(1) Permuted Ranking of Antifaithfulness Conjunction

a. Markedness active only on faithful structure

$[*B \ \& \ \neg Ident(A)]_D \gg Ident(B) \gg *B$

b. Antifaithfulness active only on marked structure

$[*B \ \& \ \neg Ident(A)]_D \gg Ident(A) \gg \neg Ident(A)$

The effect of (1b) is that the anti-faithfulness constraint has scope only over those structures that are marked according to *B. For example, in Japanese the genitive suffix *-no* triggers deaccentuation of a preceding noun, but only if the noun is accented on the final syllable (and is not monosyllabic): /kawá-no/ → *kawano* 'of river' vs. /utíwa-no/ → *utíwano* 'of fan'. In Alderete's (1998) analysis, deaccentuation is a consequence of the antifaithfulness ranking ⟦¬MAX(accent) ≫ MAX(accent)⟧. Łubowicz's proposal suggests a further refinement using the ranking (1b): the high-ranking constraint is actually $[\text{NON-FINALITY} \ \& \ \neg\text{MAX(accent)}]_\sigma$, so that only marked stem-final accents are subject to this deletion process.

Overall, this is an intriguing idea and a surprising application of anti-faithfulness. It further emphasizes a point I make in section 4.3 of my article: because CM theory gives markedness constraints access to faithfulness information, it has effects that are similar to conjunction of markedness and faithfulness constraints.

Nonetheless, there are differences. Łubowicz introduces a hypothetical example (her (16)) that reveals one of these differences. With CM, "new" material is not distinguished for its source; if a language has several processes that potentially create the segment *x*, then $_N*x$ cannot tell these

new *x*s apart (see section 5.3.2 of my article as well as Blumenfeld's commentary). With local conjunction, the situation is somewhat different: if one process derives *x* from /y/ and another process derives *x* from /z/, then different conjoined faithfulness constraints can be used to tell them apart, as in Łubowicz's tableaux (18–20). Her hypothetical example, then, shows a potential empirical difference between CM and local conjunction.

Going one step further, we can rank several theories of opacity according to their restrictiveness on this dimension. CM is the most restrictive: a process that applies only to derived *x*s cannot discriminate among derived *x*s according to their underlying origin (as long as it is not /x/, of course). Local conjunction is less restrictive: a process can be made to apply only to *x*s derived by violating a specific faithfulness constraint. Derivational approaches, such as standard rule-based phonology or stratal OT, are the least restrictive: by judicious ordering, they can ensure that only those *x*s derived by a specific process will undergo some other process. For example, if a language maps /y/ to *x* in two different environments, local conjunction à la Łubowicz cannot distinguish these *x*s, nor can CM of course, but derivational approaches are able to. Obviously, the not inconsiderable burden of proof falls on proponents of the less restrictive theories.

In another respect, local conjunction is less restrictive than derivational models, not to mention CM. Local conjunction treats interaction and locality as if they were the same thing: *x* is (un)derived if *x* **or something nearby** violates the conjoined (anti)faithfulness constraint. As long as the domain of conjunction is limited to a single segment, then the "or something nearby" clause is inoperative, and interaction is the same thing as locality. But in general the domain of conjunction is not limited to a single segment, and dubious predictions ensue (see sections 4.3 and 5.3.1 of my

article for examples). Sorting out these problems of domain and interaction seems like the number one research problem facing the theory of local conjunction.

3. Problems with Comparative Markedness?

In his commentary, Mascaró raises an important problem that emerges from his original ground-breaking research on DEE's. He previously argued that vacuous rule application creates a derived environment (Mascaró 1976). For example, Catalan has a process that lowers *e* and *o* to *ɛ* and *ɔ* when stressed. On the assumption that stress is underlyingly present, *kɾóm* 'chromium' and other examples in his (7) and (9) show that this process applies only in a derived environment. Now, as in English, the suffix *-ik* assigns stress to the preceding syllable. When this suffix is added to /kɾóm/, it vacuously assigns stress to the /ó/, but this vacuous restressing is sufficient to create a derived environment for the lowering process, so the result is *kɾɔ́mik* 'chromic'.

As Mascaró shows in his commentary, vacuous processes are undetectable with IDENT constraints: since there is no discrepancy between input and output, IDENT is not violated.[1] Therefore, vacuous DEE's, if they exist, pose a significant challenge to the CM and local-conjunction approaches.

Mascaró goes on to reanalyze the examples of vacuous DEE's from his earlier work (also see Kiparsky 1993: 291-3), identifying a new phenomenon that we might call a pseudo-DEE. Under derivation, words that are phonologically exceptional are sometimes observed to become regularized. English speakers will perhaps recognize this phenomenon from the following example. Even if an Anglophone cineaste strains to pronounce the phrase *film noir* as [film nwɑʀ], he is unlikely to emit the recent neologism *noirist* as [nwɑʀɪst], opting instead for something much more Englishy like [nuwɑɹɪst]. Mascaró's example (10), where Spanish *θ* is preserved in underived Catalan words but

normalized to *s* in their derivatives, is similar. This is not a DEE because there is no environment to be derived: context-free markedness constraints ban *ʀ* from English and *θ* from Catalan. Rather, the relatively unassimilated loans are simply exceptions to these generalizations. Why do they regularize under derivation? One possibility is that the mere presence of recognizably native morphology marks the word as non-foreign. Another possibility is that diacritic features marking exceptionality cannot percolate up to the morphosyntactic node dominating the affix or other head (see Kiparsky (1973: 90) on French *le héros* vs. *l'héroine* (but cf. Withgott 1982) and Pinker (1995: 142f.) on *flied out*).

Descriptively, the difference between a pseudo-DEE and a real one is that only the latter involve crucial phonological interaction. When Catalan renders Spanish *cazalla* as *kəθáʎə* but its diminutive as *kəsəʎɛ́t*, there is no phonological interaction between the suffix *-ɛt* and the *θ* located more than a syllable away. There is no phonological reason why the presence of *-ɛt* should cause /θ/ to map to *s* — in OT terms, no markedness constraint in universal CON is responsible for this alternation. But when Korean adds the suffix *-i* to *mat* ((27)–(29) in my article), there is phonological interaction between the *t* and the following *i* — because CON does supply a markedness constraint against unpalatalized *ti* sequences. In actual practice, then, there is little danger of confounding pseudo- and real DEE's. One lacks a phonological rationale, the other has one.

Another empirical issue involves the treatment of grandfather effects. Yip asks for conditions under which a grandfather effect really requires CM. Classic OT certainly suffices for some grandfather effects; for example, Prince and Smolensky's (1993: Chapter 4) first example of blocking is Tagalog infixation, where NO-CODA does not affect inputs, because it is ranked below FILL and PARSE, but does block prefixation of *-um-*, because it is ranked above EDGEMOST(*um*, L).

The Warlpiri analysis in (16) of my article is another grandfather effect that can be analyzed within the strictures of classic OT, if certain (possibly undesirable) assumptions about faithfulness constraints are adopted.

It is not the case, though, that all grandfather effects submit to analysis in classic OT. In (20) of my article, I show why classic OT cannot deal with Sundanese liquids, even when aided by enhanced faithfulness constraints. Two other examples of the same type, Sri Lankan Portuguese Creole place assimilation and Yawelmani glottal association (mentioned briefly at the end of section 4.1), could not be included in my article for reasons of space but are presented in the longer version (McCarthy 2002a). Let us refer to these examples as "resistant" grandfather effects, because they resist analysis in classic OT but submit to analysis in OT with CM, thereby providing empirical support for CM.

How can we know a resistant grandfather effect when we see one? Yip (section 1.2) suggests that in resistant grandfather effects "exactly one feature is changing in the process in question, and exactly that feature must be preserved in underlying segments". It is not clear, though, that these conditions are necessary or sufficient for a resistant grandfather effect. They are not sufficient because Warlpiri meets these conditions but, as seen in (16) of my article, Warlpiri does not present a resistant grandfather effect. The conditions are not necessary because in Sri Lankan Portuguese Creole multiple features are affected (the process is place assimilation) but the grandfather effect is resistant.

It may not be possible to provide a sure diagnostic for resistant grandfather effects. The problem is that, as Warlpiri shows, grandfather effects may be resistant under one conception of the faithfulness constraints and non-resistant under another. It is, of course, convenient to have a quick

way of telling whether a grandfather effect is resistant or not, but the diagnostic is not a necessary concomitant of the theory. Ultimately, we know that a grandfather effect is resistant if we cannot analyze it within classic OT.

In her commentary, Yip also observes that the account of local tone spreading presented in (39) of my article makes the following prediction: tones that are unassociated in the input will exhibit long-distance spreading, if they spread at all. Again, this is a matter addressed in the long version of my article (McCarthy 2002a) that could not be included here for reasons of space. In my (39), local spreading is the result of combining high-ranking $_{O}$AGREE with low-ranking $_{N}$AGREE, so only the disagreeing sequence present in the input is affected. When a tone floats in the input/FFC, then it is not part of a syllabic sequence, so $_{O}$AGREE is vacuously satisfied by all candidates, and therefore it is unable to compel (local) spreading.

This prediction finds strong support in the autosegmental literature. Autosegmental theory typically grants (long-distance) spreading priority to floating tones, with principles like a preference for floating (vs. linked) tones to spread (Clements and Ford 1979: 185) or automatic spreading of floating tones only (Odden (1995: 458) citing Halle and Vergnaud (1982)).

What about Yip's example from the Northern Karanga dialect of Shona, which appears to have local spreading of an underlying floating tone? Though the data initially seem persuasive, it is perhaps not unreasonable to ask for a bit more analysis before this is endorsed as a solid counterexample. Hewitt and Prince (1989), working from Odden's (1984) earlier analysis, make several key points about the floating H of the Non-assertive:

•The Non-assertive H is a suffix that docks onto the final syllable with high-toned roots and onto the peninitial syllable with low-toned roots.

•High tones spread two syllables rightward, but not onto a syllable before Non-assertive H.

•The Non-assertive H spreads only one syllable rightward, unlike the other H tones.

For example, a stem that has the underlying tone pattern /HLLLLL/ becomes [HHHLLL] in the Assertive and [HHHLLH] in the Non-assertive, while a stem that has the underlying tone pattern /LLLLLL/ becomes [LLLLLL] in the Assertive and [LHHLLL] in the Non-assertive.

This is starting to look a lot less like a straightforward counterexample and a lot more like a challenging, unsolved research problem. Local spreading of the floating Non-assertive H is just one of several remarkable peculiarities of this phenomenon. The remaining oddities include the typologically unusual two-syllable spread of H tones, the seemingly arbitrary failure of floating H to participate in this otherwise general two-syllable spreading process, and the docking of the floating H on the final versus peninitial syllable depending on whether the verb root is high-toned or not. Though ad hoc constraints can always be concocted, I do not think that a principled analysis is possible with our current understanding of what constraints reside in CON. Hewitt and Prince have some interesting ideas about how to proceed, but their analysis is couched in derivational terms and will not easily carry over to OT. In sum, we understand far too little about this phenomenon to recruit it in an argument for or against CM.

4. Output-Output Correspondence and Comparative Markedness

Should CM theory include symmetric $_{N}$M and $_{O}$M constraints? Blumenfeld argues that it should not, and specifically that $_{O}$M constraints should be dropped in favor of plain M. With the $_{O}$M constraints gone, CM is no longer applicable to the phenomena discussed in section 5 of my article: non-iterative processes, such as apocope and local tone spreading; counter-feeding opacity, where the output of one process unexpectedly fails to undergo another process; and coalescence paradoxes,

which are discussed in the long version (McCarthy 2002a) but could not be included here for reasons of space.

Blumenfeld's argument is that CM is too restrictive a theory. As Łubowicz points out (see her commentary and section 2 of this response), CM makes the highly restrictive prediction that a process limited to derived *x*s cannot discriminate among derived *x*s according to their underlying origin. And as I show in section 5.3.2 of my article, CM makes a similar prediction about counter-feeding opacity: if several processes produce a structure that violates the markedness constraint M, none violates $_{O}$M and so none will be affected if we have the counter-feeding ranking ⟦$_{O}$M ≫ Faith ≫ $_{N}$M⟧. This is what is meant by the slogans in Blumenfeld's (3).

Derivational theories do not make these restrictive predictions (see Blumenfeld's section 3.2). Indeed, derivational theories make no predictions at all about relationships among different processes that produce the same structure. By careful ordering of rules or strata, any pattern of interaction or non-interaction involving such processes can be obtained. For example, if two processes A and B could in principle create a derived environment for a third process C, but B in fact does not, then it is just a matter of ordering B (or the congeries of constraints responsible for B) after (or in a stratum after) A and C.

Since more restrictive claims are always to be preferred, arguments for a less restrictive theory need to be made and weighed carefully. When we closely examine the argument from Latin rhotacism, however, a problem emerges. Descriptively, intervocalic *s* becomes *r* when the V*s*V sequence is derived by suffixation. This is an instance of a morphologically derived environment, so it involves comparative markedness on output-output rather than input-output correspondence (see section 4.2 of my article, the somewhat lengthier discussion in McCarthy (2002a), and the

commentaries by van Oostendorp and Crowhurst, who deal with output-output and base-reduplicant correspondence, respectively). The constraint responsible for Latin rhotacism is therefore OO-$_N$*V*s*V — and not IO-$_N$*V*s*V, which is a different constraint entirely. Its old counterpart OO-$_O$*V*s*V is ranked below faithfulness, so it does not affect V*s*V sequences that have not been derived by morpheme concatenation:

(2) Latin Rhotacism in Morphologically Derived Environment

	OO-$_N$*V*s*V	IDENT(son)	OO-$_O$*V*s*V
/corpos-is/			
a. ☞ corporis		*	
b. corposis	*!		
/causa/			
c. ☞ causa			*
d. caura		*!	

For an output-output comparative markedness constraint, the analogue of the FFC is the output form of the base of OO correspondence. (Analogously, the analogue of the FFC for a base-reduplicant comparative markedness constraint is the base portion of the same form (cf. Crowhurst's commentary).) Therefore, when the candidates (2a, b) are evaluated by OO-$_N$*V*s*V, they are compared with the surface form *corpus*. Informally, the V*s*V sequence of **corposis* is new relative to *corpus*, and so OO-$_N$*V*s*V is violated. Newness relative to the surface form of the base, not the input, is the basis of morphological DEE's.

At the formal level, some care is required. The coindexed *s*s of $corpus_i$ and $*corpos_i is$ stand in OO correspondence, as do the coindexed *s* and *r* of $corpus_i$ and $corpor_i is$. Applying the definition

in (7a) of my article under the assumption that the locus of *V*s*V violation is the *s*, Loc(*corpos*$_i$*is*) is {*s*$_i$} and Loc(*corpus*$_i$) is Ø, since the *s* in *corpus* is not intervocalic. Under (7a), one violation mark is assigned for each member of Loc(*corpos*$_i$*is*) that lacks a correspondent in Loc(*corpus*$_i$), so OO-$_N$*V*s*V is violated once by **corpos*$_i$*is*, as shown in the tableau. When *caus*$_j$*a* is evaluated, Loc(*caus*$_j$*a*) is {*s*$_j$}, and, since *causa* is itself the base, all of its violations are old. (The situation is equivalent to evaluating the FFC in CM on the input-output correspondence relation.) Therefore, *causa* violates only low-ranking OO-$_O$*V*s*V.

Latin phonology also creates intervocalic *s*s under certain circumstances. When a sequence of coronal stops arises across root+suffix juncture they change into the geminate *ss*: /fid-t-us/ → *fissus* 'split (nom. sg.)', /met-t-us/ → *messus* 'mowed'. If the preceding syllable is heavy, then the result is simple *s* instead: /vīd-t-us/ → *vīsus* 'seen'; /aud-t-us/ → *ausus* 'dared'; /flect-t-us/ → *fleksus* 'bent'; /mord-t-us/ → *morsus* 'bitten'. In intervocalic contexts, such as *vīsus* and *ausus*, this derived *s* does not rhotacize.

The intervocalic *s*s of *vīsus* and *ausus* are undoubtedly in a **phonologically** derived environment — that is, they violate IO-$_N$*V*s*V — but the *corpus/corporis* evidence pertains to **morphologically** derived environments — **corposis* violates OO-$_N$*V*s*V, which must therefore be high-ranking.[2] Since phonologically derived V*s*V sequences do not rhotacize but morphologically derived ones do, the ranking in force must be ⟦OO-$_N$*V*s*V ≫ IDENT(son) ≫ IO-$_N$*V*s*V, OO-$_O$*V*s*V, IO-$_O$*V*s*V⟧, which produces exactly the desired results. Blumenfeld's analyses of Latin and other languages disregard this distinction, conflating the two types of DEE. The theory he is arguing against, then, is not the theory presented in my article.

In making a basic distinction between morphologically and phonologically derived environments, CM has several precedents. Łubowicz's (2002) local-conjunction theory and Kiparsky's (1993) underspecification theory also decouple the two types of DEE. Even in works that seek to conflate the two types of DEE, there is considerable difficulty in achieving this end. The necessary definition of "derived environment" that subsumes both types is either absent (Kiparsky 1973) or formulated disjunctively (Mascaró 1976).

Though it decouples the two types of DEE, CM does not deny that they may go together in some languages. Among the possibilities afforded by ranking permutation is ⟦OO-$_N$M, IO-$_N$M ≫ Faith ≫ OO-$_O$M, IO-$_O$M⟧, which produces both DEE types simultaneously. If van Oostendorp is correct that "[t]his convergence of phonological and morphological DEE seems quite common", then this prediction of CM is confirmed; certainly, it is wrong to say that the convergence "cannot be accounted for in the CM framework", since the ranking just given does exactly that. What CM shares with more recent approaches but not with the original Alternation Condition (Kiparsky 1972) or Strict Cycle Condition (Mascaró 1976) is the possibility of having languages where only phonologically or only morphologically derived environments undergo a process, such as Latin. The responsible rankings are ⟦OO-$_N$M ≫ Faith ≫ OO-$_O$M, IO-$_O$M, IO-$_N$M⟧ and ⟦IO-$_N$M ≫ Faith ≫ OO-$_N$M, OO-$_O$M, IO-$_O$M⟧, which account for morphologically and phonologically derived environments, respectively.

5. Is Opacity Really a Problem?

In their commentary, **Mielke**, **Armstrong**, and **Hume** (hereafter MAH) make three points: the existence of opacity is explained diachronically, though it must be analyzed synchronically; allophonic opacity, which is problematic for CM (see section 5.3.2 of my article), can be analyzed

within the strictures of classic OT; and other opacity is lexicalized, so it is outside the scope of the synchronic phonological system. The first of these points may very well be correct, but the other two are more dubious.

MAH illustrate their treatment of allophonic opacity with a reanalysis of Sea Dayak. In this language, vowels are predictably nasalized after nasal consonants and predictably oral elsewhere. The sole exception is that vowels are oral when they are post-nasal as a result of optional deletion of a voiced stop following a nasal: [rambo̍] ~ [ramo̍], *[ramõ̍] 'a kind of flowering plant'.

To account for this distribution, MAH cite four constraints, which are given in (3).

(3) Constraints in MAH's Analysis of Sea Dayak

a. NASALCOND[3]

Nasal vowels are prohibited except when preceded by a nasal segment.

b. IDENT-V(nasal)

Vowels retain their value for [nasal] in input-output mappings.

c. *ND

A homorganic nasal+stop cluster is prohibited.

d. $*NV_{Oral}$

Oral vowels are prohibited after nasal consonants.

NASALCOND dominates IDENT-V(nasal) (see their tableau (3)). The constraint *ND is variably ranked with respect to MAX, accounting for the variable deletion of /b/ in [rambo̍] ~ [ramo̍]. In their tableaux (5) and (6), $*NV_{Oral}$ is ranked below IDENT-V(nasal) — in other words, it is ranked too low to have any effect on the outcome.

Under the core OT hypothesis of richness of the base (McCarthy 2002b: 68-82, Prince and Smolensky 1993), the grammar of a language must assign correct outputs to any input, without the aid of morpheme structure constraints, underspecification, or other language-particular restrictions on the form of inputs. MAH accept this premise, but their analysis is not compatible with it. Observe in (4) what happens when the proposed grammar is given the inputs /ma/, /mã/, /ba/, and /bã/.

(4) An Unwanted Result

	NASALCOND	IDENT-V(nasal)	*NV$_{Oral}$
/ma/			
a. 💣 ma			*
b. mã		*!	
/mã/			
c. ma		*!	*
d. ☞ mã			
/ba/			
e. ☞ ba			
f. bã	*!	*	
/bã/			
g. ☞ ba		*	
h. bã	*!		

The mapping /bã/ → *ba* in (4g) shows that this analysis correctly has a process denasalizing vowels that are not preceded by nasals. But the mapping /ma/ → *ma* in (4a) shows that this analysis has no process nasalizing vowels after nasal consonants. True, vowels do not become nasalized when they are preceded by a nasal solely by virtue of optional consonant deletion, as in [rambo?] ~ [ramo?].

But what about the many words that do not have this C~Ø variation? Is it credible that speakers do not control the complete predictability of nasality in these words? For example, initial nasals are always followed by nasal vowels, since there is no C~Ø variation initially, yet this analysis fails to predict that.

MAH's tableau (6) reveals an even more serious complication. Given an input like /rambõ/ — an input that richness of the base requires us to consider — the proposed grammar will emit two variants, [rambo?] and [ramõ?]. But this sort of variation is not reported for Sea Dayak: whenever there is C~Ø variation after a nasal, the following vowel is oral in both variants. In short, the proposed analysis does not get the facts right.

Allophonic opacity definitely presents thorny problems; after all, as I show in section 5.3.2 of my article, CM cannot deal with it. But those problems will not be solved simply by denying that there is any sort of allophonic process involved. The same goes for opacity in general, to which I will now turn.

The idea that there are no opaque phonological processes has a long history. It was pursued most vigorously in the theory of Natural Generative Phonology (Hooper [Bybee] 1976, 1979, Hudson 1974, Vennemann 1974), which held that all phonological processes or constraints must be "true generalizations". Classic OT denies that phonological constraints must be true generalizations, but it confines the conditions under which they be non-true to those that are attainable by interaction of markedness and faithfulness constraints. CM and the various other approaches to opacity mentioned in my article respond to situations involving non-surface-true generalizations that do not fall under this rubric.

Using Barrow Inupiaq as an example, MAH argue that opaque alternations are actually lexicalized, putting them outside the purview of markedness/faithfulness interactions and therefore outside the explanatory and analytic scope of OT. Their arguments about Barrow Inupiaq seem persuasive, but the real question is whether there are other examples of opaque processes that are unlikely to be lexicalized.

The best examples of such processes come from fast or casual speech, and they are extensively documented by Donegan and Stampe (1979: 145ff.). For instance, some speakers who have intervocalic flapping in *pat it* [pæɾɪt], nonetheless do not flap in *plant it* [plæ̃tɪt], where the *t* is intervocalic by virtue of loss of the preceding nasal. This is a typical counterfeeding interaction, no different in kind from Barrow Inupiaq. Or speakers who have a centralizing off-glide after a front vowel before a velar nasal in words like *bang* [bæᵊŋ] do not supply it in words like *mankind* [mæŋkɑjnd], where the velar nasal is derived by assimilation. It is very difficult to see how these and many other cases will be explained away as lexicalized remnants of formerly productive processes.

6. Conclusion

Of necessity, these brief comments cannot do justice to the depth of thought and analysis in the various commentaries. For example, Crowhurst has unearthed some intricate and fascinating predictions that arise when CM is extended to base-reduplicant relations, Łubowicz has proposed a novel application of local conjunction and antifaithfulness, and van Oostendorp has raised a question about possible connections between CM and the original PARSE/FILL model of faithfulness (Prince and Smolensky 1993). These are all potentially rich areas for future research, and perhaps we will one day hear more about them.

Notes

1. There are certain situations where vacuous processes are detectable using faithfulness constraints, though not of the IDENT type. Prince and Smolensky (1993: Chapter 7, footnote 62) consider a way of satisfying the antifaithfulness constraint FREE-V in Lardil while still obeying foot binarity. The idea is to have /mela/ map to *mel*□⟨*a*⟩, with the root-final *a* unparsed and an empty nucleus (realized phonetically as *a*) replacing it. In correspondence theory, similar candidates are possible: /$m_1e_2l_3a_4$/ → $m_1e_2l_3a$, with non-corresponding final *a*.

2. To complete the picture of Latin, it is also necessary to show that *ausus* and *vīsus* obey high-ranking OO-$_N$*V*s*V. They do so under the assumption that they are their own bases of OO correspondence, like *causa* in (2c). The form *causa* is its own OO base because it heads an inflectional paradigm, like *corpus*. Similarly, the participial forms *ausus* and *vīsus* head inflectional paradigms; their relation to the other principal parts of the verb is derivation from a common bound root, which does not bring with it an OO correspondence relation (Benua 1997: section 5.4), as shown by the observation that bound roots are not cyclic domains (Brame 1974, Inkelas 1989, Kiparsky 1982, 1985).

3. This definition of NASALCOND is problematic. Modifiers like "except when" should not be necessary in constraint definitions because the effect of "except when" can be obtained from constraint interaction under EVAL (McCarthy 2002b: 40). An example is the analysis of nasal harmony in McCarthy and Prince (1995, 1999), with these interacting constraints: $*NV_{Oral}$, which requires nasal vowels in a post-nasal context, and $*V_{Nasal}$, which prohibits nasal vowels everywhere. If nasal vowels are prohibited except when preceded by a nasal consonant, then the ranking is $[\![*NV_{Oral} \gg V_{Nasal}]\!]$, with the domination relation fulfilling the role of "except when" in NASALCOND.

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