On Double Marking and Containment in Realization Theory

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Abstract:

Embick (2012) argues that insertion into terminal nodes (aka "morpheme insertion only", or MIO) is preferable over insertion into non-terminals (aka, INT) within realizational theories of morphology. Embick (2012) provides two key arguments: double marking and the containment prediction. In this paper we respond to both criticisms of INT. We show that double marking is an artifact of the aggressive decomposition mandated by terminal-only insertion, and that containment is a problem for terminal-only insertion just as it is for non-terminal insertion. We then propose an alternative to both models which employs insertion that realizes many contiguous members of the string of features present after linearization. We argue that this proposal appears to better account for the data under discussion

List of Abbreviations:

DM: Distributed Morphology
INT: Insertion Into Non-Terminals
MIO: Morpheme Insertion Only: i.e. insertion only into terminal nodes.
RT: Realization Theory: i.e. realizational models of morphology.

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1. Introduction

Realizational theories of morphology, the collection of which we will herein call, following Koenig (1999), *Realization Theory* (or RT, for short), are those frameworks that assume Late Insertion of phonology into the syntactic derivation (Anderson 1992; Halle & Marantz 1993; Starke 2009). The core idea of RT is that morphology is the manipulation of abstract features which are then expressed by listed phonology—that is, abstract morphemes are *realized* by the phonology. One prominent debate going on in the domain of RT is the nature of this insertion. Most approaches adopting some version of Distributed Morphology (DM, Halle & Marantz 1993 et seq.) assume that insertion happens at terminal nodes (with Radkevitch 2010 being a notable exception). Contrarily, Nanosyntax (Starke 2009) assumes that insertion can happen at non-terminal nodes (see Caha 2009 for a very cogent description of insertion in Nanosyntax). In fact, this distinction is the crucial difference between Nanosyntax and DM. All the other differences between these two models effectively fall out of that first assumption.

In "On the targets of phonological realization", Embick (2012) compares these two types of insertion, calling the models that restrict insertion to terminals "Morpheme Insertion Only" (MIO) and the models that allow insertion at non-terminal nodes "Insertion at Non-Terminals" (INT). Embick (2012) concludes that the INT approach is metatheoretically preferable over the MIO approach, because it offers accounts for word/phrase blocking. At the same time, Embick raises an important point which falls out of INT, but which he claims to be a false prediction of those models. This is the *containment prediction* (previously described but unnamed as such in Embick & Marantz 2008) — namely, that the contained nodes must always be subsumed by the insertion at the dominating non-terminal. On the other hand, according to Embick, the

application of insertion and readjustment rules in MIO approaches predicts what Embick (2012) calls *double marking* (originally rearing its head in Halle & Marantz 1993, but, again, unnamed as such) — namely, that readjustment rules reflect the morphology in the stem while there is also an affix present, effectively marking the morphology twice, and only twice, in the word-form. Embick argues that MIO models predict such double marking while INT models fail to offer an account for them. Embick thus claims that these two predictions — one positive for MIO and one negative for INT — result in a situation wherein while INT may be more parsimonious and thus metatheoretically preferable to MIO, its false predictions (regarding the non-existence of containment and the actual existence of double-marking) fails to account for actually attested morphological patterns that MIO correctly accounts for. Thus, according to Embick's reasoning, it should be clear that, based on empirical coverage, MIO models are in the end preferable to INT models.

The purpose of this paper is to challenge Embick's conclusions regarding INT and MIO models, though not necessarily to argue that one of those approaches is superior to the other. Rather, the focus of this paper is to re-assess the nature of containment and double-marking as criteria for evaluating morphological theories.

First, in regard to the prediction of so-called *double marking*, we argue that data referred to as "double marking" is actually an epiphenomenal subset of weak stem suppletion and that its perceived existence is due to overly aggressive decomposition employed by MIO realizational models. This aggressive decomposition is not categorically restricted in any meaningful way, and it forces MIO theories to treat productive morphological processes and non-productive — and even moribund or borrowed — processes in precisely the same way. We argue that this aggressive decomposition clearly expands the data set beyond the bounds of what generative

morphological grammars really need to cover, even beyond what is typically explained by MIO models. We propose instead that realizational models need to adopt a categorical restriction on which apparent morphological processes are actually produced as a product of the grammar. We suggest that RT should adopt the restrictions outlined in Lieber (1992). In such a case, it is clear that double marking exists outside of that restriction, because such cases are not actually productive. Furthermore, we show that the double marking prediction itself is false *even if* we are maximally decompositional, as there exist word-forms that are even *triple-marked*, and MIO, under Embick's assumptions, would allow for a maximum of two morphological markings (i.e. double marking as the result of stem-readjustment, which is one marking, triggered by affixation, a second marking). The concessions that MIO theories need to make to account for triple marking are precisely those that INT theories need to make for double marking. So, we argue that these two types of theories are, in the end, equivalent on these grounds.

Secondly, we will show that, while Embick's containment prediction is in fact a correct assessment of a defect in RT, it is not necessarily a fatal flaw and it is not unique to INT models. Indeed, we show that MIO models have the same problem, except that in MIO models this containment flaw comes in the form of restrictions on fusion. We propose here that a possible solution to the containment problem for RT is to adopt neither MIO nor INT, but rather for a third way—namely, for insertion to happen as late as possible, *after* linearization. In such a model, portmanteaux data typically dealt with via non-terminal insertion would instead be the realization of two adjacent morphemes with only one Vocabulary item.

The purpose of Embick (2012) is to summarize arguments for and against non-terminal insertion. As a result, very often Embick (2012) recapitulates arguments made earlier in the literature. For example, Embick (2007b) and Embick & Marantz (2008) previously discuss

containment in their accounts of blocking (without explicitly naming the prediction as such). Double marking as a prediction of DM first appears in Halle & Marantz (1993). In this paper, we direct our remarks to the discussion of the relevant issues from Embick (2012) with the understanding that Embick (2012) is itself representing arguments that occur previously in the literature.

This paper is structured as follows. Section 2 surveys and builds upon the reasons that Embick (2012) suggests that INT models are theoretically preferable to MIO models. In particular, we reiterate the discussion in the literature that not only is the existence of mechanisms beyond insertion less parsimonious than an insertion-only model, but two of these apparently necessary theoretical mechanisms — fusion/fission and readjustment rules — are themselves highly suspect and worthy of rejection should a theoretical approach be able to cover the same empirical ground without the use of them. Section 3 represents the majority of the argument this paper puts forth. In that section we discuss Embick's double marking argument and present arguments against the claim that MIO correctly predicts double marking, and also show apparent examples of triple marking which undermine the supposed double marking limit. In Section 4, we discuss the containment prediction, which Embick (2012) uses as evidence against INT models, and show that it also applies to MIO models, which Embick (2012) supports, and propose a possible solution to the conundrum—Vocabulary insertion after linearization. Section 5 concludes.

2. The strengths of Insertion into Non-Terminals (INT)

Embick (2012) points out that Vocabulary insertion into non-terminals, i.e. INT, is appealing within RT for two main reasons. The first is that it allows for an explanation of a particular type of blocking that has been a prevalent part of formal morphological theory for about as long as

there has been formal morphological theory: i.e. the blocking of syntactic phrases with words (e.g. *happier* blocks **more happy*; *tomorrow* blocks **the day after today*; see DiSciullo & Williams 1987; Andrews 1990; Poser 1992; Sells 1997; Ackema & Neeleman 2001; Hankamer & Mikkelsen 2005; Kiparsky 2005; Embick 2007b; Bobaljik 2012; among many, many others). Non-terminal insertion allows for such blocking by allowing the node that dominates all the subsumed material to be realized by a single Vocabulary item.

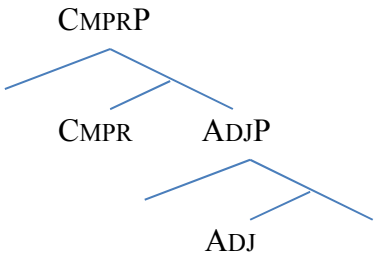
The competition of *-er* and *more* in English is often considered to be the strongest argument for this morphological blocking of phrases. Constructions such as *more intelligent* are typically treated as paraphrastic (i.e. phrasal) while constructions like *smarter* are typically treated as morphological (i.e. affixal) (cf., e.g., Bobaljik 2012: 67-93), and there is no reason to our knowledge to treat this distinction otherwise. However, typically in contemporary English the application of the *-er* affix blocks the use of *more*, both intra-derivationally and cross-derivationally (cf. **more smart* and **more smarter*; though this blocking wasn't true in earlier stages of English, and it also isn't exceptionless — *more better* is not especially uncommon dialectally and the blocking always fails in constructions such as *She is more savvy than smart*). In this competition, *more* is clearly the elsewhere condition and *-er* is clearly the irregular form. The distribution of the suffix *-er* is largely unpredictable and very often lexically-conditioned. Although there is an upward limit on the prosodic length of words it can attach to (i.e. two syllables), words of two syllables or less do not behave systematically (cf. **funner, *clutcher*).[1]

Since *-er* is the irregular, semi-productive form, the typical assumption, as far as the assumption that one form blocks the other one goes, is that *-er* blocks *more*. An INT model of

[1] Even this two syllable restriction is famously violated with *unhappier* — see Lieber 1992 among many others on the topic of bracketing paradoxes.

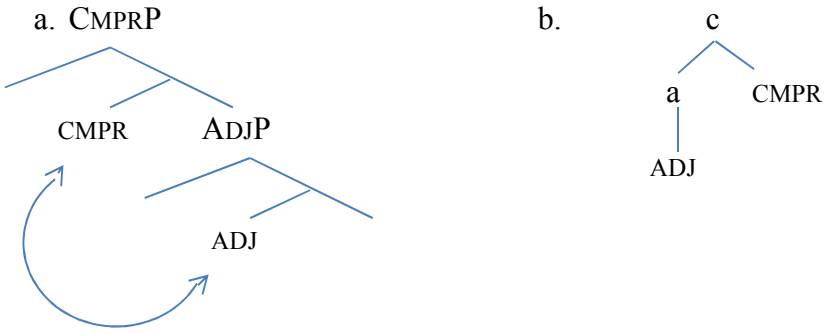
realization easily accounts for this blocking by positing that *smart-er* competes for insertion into the A-bar level that dominates both the root and the intensifier (see 1) and that it wins the competition over **more smart* because it better satisfies some economy constraint that either prefers fewer number of Vocabulary items (Siddiqi 2009) or prefers words over phrases (Embick 2012).

1) Syntactic representation of *more smart* (following Bobaljik 2012: 69 [91])[2]



Under an analysis like that given by Bobaljik (2012), forms with *-er* require fusion of the comparative degree head (CMPR) and the adjectival root (ADJ); the precise mechanism of this fusion is irrelevant to our purposes here, but could involve one of a number of standardly assumed operations, including incorporation, morphological merger, or something else. This fusion is shown in (2a), the result of which fusion would be the complex head in (2b).

2) Syntactic representation of *smarter* (following Bobaljik 2012: 69 [92])



[2] Many analyses would posit a Degree head (DEG^0) for the comparative morpheme. Bobaljik (2012) crucially divides different levels of degree (e.g. comparative and superlative) into their own syntactic heads because he claims that the superlative must *contain* the comparative. We assume this analysis here, and discuss the issue more fully in our discussion of containment in section 4 below.

The unfused heads in (1) would be spelled out by the paraphrastic phrase *more smart*, where the heads CMPR and ADJ would each receive their own Vocabulary item. In this way, affixation, brought about by some morphological process (incorporation, morphological merger, etc.), can block a syntactic phrase.

On the other hand, an INT account would simply argue that insertion of *more* targets CMPR and insertion of *smart* targets ADJ in the case of *more smart*, but the insertion of *smarter* targets CMPR' (or CMPRP) and simply subsumes the embedded structure.

A second reason that Embick (2012) gives for the preference of INT models over MIO models is metatheoretical. Embick's argument is that a model of RT with fewer secondary mechanisms is more parsimonious and is thus preferable (a la Occam's Razor) (see Trommer 1997). Starke (2009) makes a number of claims about the domains where this is evident (including claims about typological predictions and defective morphemes), but Embick (2012) points out that the most cogent of the domains where this difference is salient are portmanteau morphemes (see Caha 2009, Radkevich 2010) and stem allomorphy (see Siddiqi 2009, Starke 2009, Haugen & Siddiqi 2013).

Here we can take traditional DM to be typical of MIO models. DM requires a number of secondary processes to deal with portmanteau morphemes (such as Spanish *del* 'of the [masc]') and fusional affixes (such as verbal inflection for which Latin is a typical example). The most prevalent of those processes is fusion (and correspondingly fission), the process whereby neighboring terminal nodes (typically restricted to terminal nodes in a complex head) are replaced with (or fused into) a single node that contains all the features of the fused nodes and is then subject to insertion; fission is correspondingly the replacement of a single terminal node

with multiple features with a several nodes containing fewer features each (see Halle & Marantz 1994).

As well-detailed by Caha (2009) and works cited therein, fusion is rather objectionable on metatheoretical grounds because it is both circular and unmotivated. The important fact about fusion is that it only happens when it "needs to" happen, but doesn't happen elsewhere. In short, fusion is mostly stipulated and unmotivated.[3] Even cases like the English comparative alternation (i.e. paraphrastic *more X vs.* synthetic *X-er*) require extensive listing of roots which would be specified for taking the non-default spell out of the CMPR head; see Bobaljik 2012 for a recent analysis along these lines.

Fusion is entirely unnecessary in an INT model because two neighboring nodes can be realized by one Vocabulary item at a non-terminal node. Similarly, INT models have no need for fission since the assumption is that many terminal nodes are submorphemic, so rather than splitting the features off from a single node, an INT model simply assumes that all features are base generated in their own functional heads. While the obviation of fusion by INT models is valuable merely for reasons of parsimony and theoretical elegance, that fusion itself is an unmotivated and unrestricted process makes it a particularly appealing candidate for obviation.

Stem allomorphy similarly requires ancillary morphological processes. In this case, the typical process invoked is a suite of readjustment rules. Readjustment rules are phonological processes that replace part or all of the phonology of a stem. For example, when affixed with *-t,* the phonology of the English stem *seek* [sik] is replaced with the phonology *sough-* [sɔ-]. As detailed in Haugen & Siddiqi (2013), readjustment rules are effectively the replacement

[3] Though we note that Siddiqi (2009) does propose an insertion-driven way to restrict fusion to some degree by making it a default operation. Those proposals ultimately do not restrict fusion enough to stop it from occurring everywhere, and those moves displace the work of restriction on fusion to highly specifying Vocabulary items.

algorithms that are common in word-based approaches to morphology (Aronoff 1976, et seq.). Because these rules are the same as those employed in rule-based models of morphology, MIO models inherit all of the problems that rule-based models bring with them (as well reported throughout the literature; see Siddiqi *forthcoming* for a recent review and discussion). The chief among these is that they are completely unrestricted and overly powerful (as they are powerful enough to replace most if not all of the phonology of a stem, as seen below).

Distributed Morphology is an item-and-arrangement model of morphology, in the typology presented by Hockett (1954). One of the foremost appeals of an item-and-arrangement model is that it is, or at least in principle ought to be, restricted. This makes up for an item-and-arrangement model's lack of empirical coverage compared to a process-based model (which is noticeably less restricted). Since DM employs these unrestricted rules, its chief appeal (other than the single module hypothesis and the concatenation-prevalence observation) of restrictiveness evaporates. Furthermore, these readjustment rules tend to be highly idiosyncratic in nature (though the argument is that some of these generalize) since they are largely employed to account for irregular morphology. Finally, the existence of readjustment rules is what allows DM practitioners to make the claim that root suppletion does not exist (indeed, some variants of the theory, for theory-internal reasons, actually mandates such a claim). This position seems to simply be empirically false; see Siddiqi (2009), Harley (2011), Bobaljik (2012), Haugen & Siddiqi (2013), and Haugen & Everdell (2013) for discussion and examples of various patterns of root suppletion in the nominal, verbal, and/or adjectival domains, found in languages of different families and stocks from all over the world. INT models have no need for readjustment rules because a form with irregular base modification such as *sought* can be inserted into a non-terminal node that dominates both the root node and node with the feature that conditions the

allomorphy — in the case of *seek*, this would be the past tense feature at T^0. Elimination of readjustment rules immediately brings back to INT models the restrictiveness that makes item-and-arrangement models metatheoretically preferable to item-and-process models.

It is worthwhile to add to the present discussion a brief recapitulation of a relevant argument made by Siddiqi (2009). Siddiqi (2009) points out that another prominent weakness of arrangement models of morphology is the prevalence of null affixes, as arrangement models are forced to propose their existence for entirely ad hoc and theory-internal reasons (such as for an account of unmarked class change or conversion). These zero affixes are completely theoretical and lack any empirical support outside of arrangement assumptions and so have represented one of the chief criticisms of arrangement models since Aronoff (1976). Distributed Morphology, in particular, has typically had an especially large amount of these null-affixes because, outside of models that assume auto-segmental affixation, all stem allomorphy and base modification in DM is treated as a readjustment rule conditioned by a null affix. Siddiqi (2009) used rampant fusion to deal with this criticism, but INT models almost completely vacate the need for zero affixation without the use of fusion.

As Embick (2012) puts it: "the general idea— reducing as much as possible to the insertion mechanisms — is well-motivated." However, Embick (2012) assumes that the MIO and INT models are too disparate to just count the different number of mechanisms to make metatheoretical arguments: "Ultimately, both MIO and INT have to be associated with different auxiliary theories, making 'conceptual' arguments about which has more or less machinery secondary to questions about where they might differ empirically" (2). We respectfully disagree that the conceptual differences are necessarily secondary for several reasons:

First, it is not the case that what is at issue in comparing INT and MIO theories is just a matter of mere quantity – i.e. “more” versus “fewer” mechanisms employed by each type of grammar. Rather, the mechanisms that we propose to remove from RT are highly specific: namely, fusion and readjustment, and these are "conceptually" questionable already, as per our discussion above.

Second, the principle of parsimony (Occam's Razor) does not claim that empirical coverage should be preferred over parsimony. Quite the opposite, the principle of parsimony mandates that we only sacrifice parsimony for *explanatory power*. Readjustment rules and fusion may indeed offer more empirical coverage (in that they can be used to generate not only the attested patterns found in natural languages *as well as* patterns which would never be found in natural languages), but they don't add any explanatory power to realizational models because they are, by their very nature, *stipulative.* If we are making less parsimonious assumptions simply to expand the empirical coverage of the model without actually *explaining* that data, then we are clearly in violation of the principle of parsimony. In other words, readjustment rules and fusion, because they are stipulative in nature, are “explanatorily idle” (to quote Baker’s 2011 excellent description of the principle of parsimony).

Third, of course readjustment rules and fusion have great empirical coverage. We should expect them to. They are so powerful and so unrestricted that they can easily account for a large amount of data, but they also egregiously generate many kinds of patterns that don’t, and couldn’t, actually exist in natural human languages. Again, this is the crux of the larger debate between process models and arrangement models. RT is an arrangement model. It has already chosen to sacrifice empirical coverage for explanatory power.

Fourth, we can indeed look at the predictions of the models to evaluate the differences between the two, and that is the issue dealt with in §3 below, but a thorough evaluation has to look at the predictions of the MIO models. Because of their reliance on the unrestricted nature of readjustment, MIO models, like other rule-based models, predict the unrestricted replacement of phonology in morphologically related forms. That is, if a rule is powerful enough to replace the phonology of *think* with *thought* or *person* with *people* (or, to adopt another example of nominal suppletion for number from the Uto-Aztecan language Hopi: *wùuti* 'woman' with *momoyam* 'women'; see Haugen & Siddiqi 2013 for additional examples) then it is in principle so powerful that it can replace any phonological string with another. MIO models clearly overpredict. Embick claims that INT models *underpredict*. In our view, metatheoretically speaking, underprediction is actually preferable to overprediction because underprediction is side effect of an intrinsically restricted model.

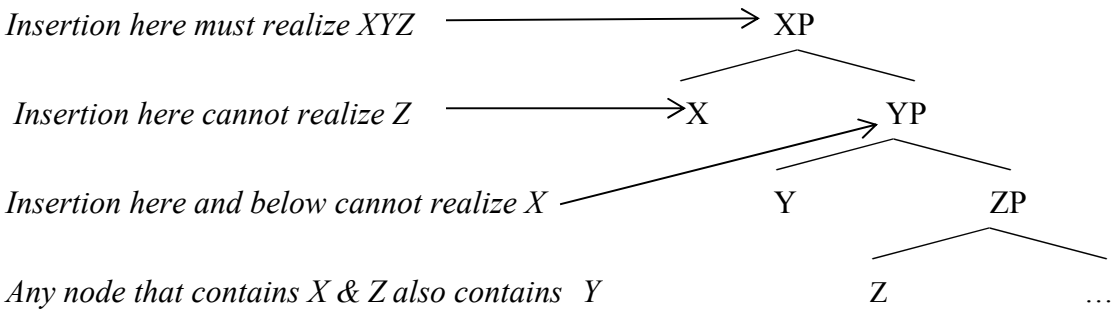
Despite our reluctance to accept that the predictions made by Embick (2012) are indeed the crucial evaluative measure by which we ought to compare INT models and MIO models, these predictions, i.e. double marking and containment, are open for assessment. We take up double marking and containment in §3 and §4, respectively, concluding that they are not the fatal flaws for INT models as Embick would have it.

3. Double Marking

One of the most cogent of Embick's (2012) criticisms of INT models is that they predict that all non-concatenative morphological processes, and in particular morphophonemic alternations, must be local. Under an INT theory there is no way for a morpheme X to affect root

Z with an intervening morpheme Y, such as in (2).[4] Any node that dominates both X and Z will also dominate Y and as a result must also absorb the features contained in Y as well (this is actually an expansion of the containment prediction to be discussed below, but leaving it to locality is sufficient for the present discussion).

2)



Embick (2012) claims, accurately, that it is not a difficult task to find non-local morphophonemic processes in the languages of the world. For example, Embick (2012) cites Zulu palatalization (Carstairs-McCarthy 1992) and Italian metaphony (Maiden 1991; Calabrese 1999, 2009) as the token examples to display non-local morphophonemic alternations. However, as Embick (2012) also admits, such criticisms are already addressed both in the MIO and INT models by proposing that they are purely phonological processes handled by the phonological component. The question in that case, then, is what the phonology can do with the inputs from the morphology. Bye & Svenonius (2012) propose an INT model whereby a constraint-based harmonic phonological component (Optimality Theoretic in this case) acts on the spelled-out phases to generate non-local phonological effects. Haugen (2008, 2011) proposes such a MIO model, as well, to address prosodic morphology such as reduplication and mora affixation; see also Bermúdez-Otero (2013) and Zimmermann (2013). Harmony approaches excel at non-local

[4]To be a bit more precise: X can affect Z, it just can't ignore Y; and Y can't be a phase boundary (Embick 2010).

phonological effects,[5] so treating these effects as purely phonological is trivial within standardly assumed mechanisms in current theories.

Embick (2012) then turns to a particular class of stem allomorphy where there are (apparently) two reflexes of an affix – the affix itself co-occurring with some kind of allomorphy within the stem – e.g. such examples as *broken* (i.e. the verb stem alternant *broke* + X affix *–en*), *sold* (i.e. the verb stem alternant of the root *sell*, *sol-*, plus the regular past tense suffix *–t*), and *thought* (i.e. the verb stem alternate of the root *think*, *though-*, plus the regular past tense suffix *–t*). This is the phenomenon of "double marking", and it has always been one of the strongest predictions of a readjustment model (Halle & Marantz 1993). MIO models with readjustment predict the existence of double marking because there are two mechanisms at work to account for different co-occurring kinds of allomorphy: competition and readjustment. Competition acts on the affix (in that the insertion of specific affixes depends on the presence of specific stems, with one variant being the unmarked, default, or "elsewhere" case) and readjustment acts on the stem (in that the stem is taken to be "readjusted" given the presence of (the morphosyntactic context for) the insertion of specific affixes. As such, this model predicts the four-way pattern seen in (3), modified from Halle & Marantz (1993):

3) a. Regular stem + regular affix: walk-ed

b. Irregular stem (readjustment) + regular affix: slep-t; sol-d

c. Regular stem + irregular affix (competition): hit-Ø; burn-t

d. Irregular stem + irregular affix: ran-Ø; dream-t[6]

[5] Although, of course, harmony approaches in many cases may be overly generous in the kinds of non-local effects that they will allow. See Embick (2010) for a thorough discussion and critique of globalist approaches in favor of localist approaches, with which we are largely sympathetic.

[6] This *–t* is an irregular affix because it does not assimilate voice as the regular English past tense affix does.

The cases in (3b) and (3d) are considered double-marked because there is both an affix and a stem change.[7] Embick (2012) claims that INT models would have to treat these as purely phonological (and indeed, were these to be at all productive, it seems clear that the approach offered by Bye & Svenonius 2012 easily handles this by treating them as phonological), but Embick (2012) assumes that they ought not to be because the relationship is non-local. Embick (2012) argues that an INT model is forced into treating these forms as unsegmentable (or truly suppletive) – as would the fusion-based model of stem allomorphy proposed by Siddiqi (2009).

Embick's (2012) implication is that treating these irregular cases as unsegmentable misses some generalization (such as an ablaut pattern that gets extended in cases of analogy). However, we argue that these forms ought to be treated as unsegmentable. Indeed, treating all morphologically complex forms as segmentable, a feature of DM called "aggressive decomposition" by Haugen & Siddiqi 2013 (following Siddiqi 2010), is the metatheoretical problem in the first place: not all forms that are historically complex need to be treated as such in the synchronic grammar of a given language. It is cognitively unrealistic to suggest that unproductive morphology is productively done by the grammar, especially since evidence of lexicalization indicates that these forms are stored, not composed (see Aronoff 1976 et seq., esp. DiSciullo & Williams 1987).

Before entering into a discussion about double marking, we ought to take another moment to recapitulate an argument made in Siddiqi (2009) and Haugen & Siddiqi (2013). Embick (2012) claims that INT models fail to predict double marking. We think this is not actually the case. Rather, INT models simply don't care about putative double marking. For INT

[7] Note, of course, that the presence of null affixes is considered an irregular way to mark the root in question, so this counts as one instance of marking—so, null affixes can be involved in both single marking (with a null affix and no stem change in 3c) and double marking (with a null affix co-occurring with a stem change in 3d).

models (and the Siddiqi 2009 model), it is not at all important how many times a listed item appears to be marked or changed from the monomorphemic form. Such marking is an historical artifact. The phonology of the listed item need not be similar at all to the monomorphemic root Vocabulary item (such as is the case in "strong" root suppletion like *go-went, bad-worse* and *good-best*). We would speculate that the dominant percentage of root suppletion is weak suppletion (i.e. there is obvious similarity between the inflected form and the base form) precisely because that form of suppletion is more likely to be learnable in light of the mutual exclusivity constraint (see Marantz 1997, Harley & Noyer 1999), and that form of suppletion is simply more likely to arise and be maintained historically. The prevalence of weak suppletion over strong suppletion is then not a function of the grammar, but a statistical aberration caused by acquisitional and diachronic effects. On the other hand, it is readjustment MIO models that predict that double marking *must* occur. Stem readjustment must be triggered by something, so null affixes are invoked where nothing else obviously spells out the morphosyntactic features in some given configuration. So, we suggest that the relevant question at hand is not "Does double marking exist even though INT models fail to predict it?", but, rather, "MIO models strongly predict double marking, but is it a real phenomenon?". The existence of double marking, we claim, would not necessarily falsify INT models, as Embick (2012) would have it, but, rather, it would seem to serve as confirmation of MIO models which need to have it by theory-internal stipulation. This is an important distinction we return to in section 3.4 below.

Before that, though, let us reconsider the extent to which double marking is a phenomenon that must be predicted to exist by a given theory. The issues that we will raise include productivity (or lack thereof) (3.1); potential cases of *triple* marking (exceeding the 2-

markings only limit predicted by Embick's version of MIO) (3.2); and cases of aggressive decomposition involving morphologically complex borrowings (3.3).

3.1 *The productivity of double marking*

Double marking is one of a great many morphophonemic alternations that must be assumed to exist as a side effect of Distributed Morphology's default assumption of aggressive decomposition. Another such case would be tri-syllabic shortening (e.g. *serene-serenity,* which could also be considered double marking in this discussion because it involves a stem change along with affixation). Like tri-syllabic shortening, double marking of the type found in English (as discussed by Embick 2012) is an historical artifact. For example, the double marking in the words *wives, knives, wolves,* etc. is an effect of Middle English intervocalic voicing. Middle English intervocalic voicing is no longer productive and in many cases has been preserved in places where it is no longer conditioned (such as above, but also in words like *mouthe, teethe, bathe,* etc.).[8] The extent to which this process can be seen as productive at all in English today is limited to words with final /θ/ (e.g. *booth-booths, sabertooth-sabertooths*). This is likely because the spelling of /θ/and /t/ is identical in English ("th") and so doesn't inform the speaker about which is the "right" phoneme. This would explain why this process is sporadic: it is likely the result of analogy.

A similar explanation is appropriate for cases like *slept, kept, leapt, wept,* and *crept* (some of these vowels were shortened in Early Modern English, while the others were shortened by analogy). In this case, a typical DM analysis according to the predictions illustrated above in (3) would take this as an example of a readjustment rule triggered by the application of the

[8] Indeed, that this voicing became unconditioned is one of the largest factors in the phonemicization of voiced fricatives in English (Brinton & Arnovic 2011).

default affix. However, we argue that the orthography of these forms reveals that this may not be the case. Even the more recent forms are spelled with a *-t* for the final sound, not the *-ed* that spells the regular past tense marker. The *-ed* spelling would be expected if speakers perceived this as the same (regular) affix. Indeed, another apparent irregular past tense affix has the same spelling: the affix that is always voiceless regardless of environment (i.e. it doesn't assimilate voice) regardless of whether the stem has vowel shortening (seen in *burnt, dreamt, spelt, thought, bought*). The spelling of these forms suggests that they are listed items. These forms aren't double marked, as Embick's analysis would have it. Rather, we hold that these forms are *suppletive*. As mentioned above, they are just more etymologically transparent than strong suppletive forms. While *slept* and *leapt* may seem much simpler deviations from the predicted form than *burnt* and *spelt,* we assert that that is epiphenomenal. These cases of readjusted stems with apparent regular affixes represent one end a scale of etymological transparency in listed forms. Indeed, there is compelling response time evidence that forms that look to be entirely productive may still be listed as a function of frequency (Stemberger & MacWhinney 1986, 1988).

Further down this scale of etymological transparency are forms like *thought* and *sought*, which truly embody the unrestricted nature of the claim of double marking. In the case of *thought,* not only must a readjustment MIO model stipulate a separate, *lexically-conditioned*, past tense Vocabulary item that does not assimilate voice, it must also assume a readjustment rule that is so powerful that it replaces all of the word save the initial segment (*think* [θiŋk] → *though-* [θɔ]). Such a rule is already powerful enough to do *go* → *went,* a clear case of root suppletion. Clearly, this type of readjustment (not to mention the circularity of the lexically-

conditioned affix -- see Siddiqi 2009) is far too powerful and, what is more, it is also far from elegant.

3.1.1 *Double marking and opacity*

Readjustment rules are, by hypothesis, phonological rules, occurring in the phonological component of the grammar. As Embick (2007a) shows, readjustment rules are also crucially ordered so that they feed and bleed each other. This actually presents a problem for the readjustment rules that are attested.[9] Readjustment rules are primarily the MIO solution to morphophonemic alternations such as trisyllabic shortening and the like. In other words, they are the equivalent to level one operations in Kiparsky (1982). Morphophonemic alternations are famous for not behaving like the other phonological processes.[10] Typically, these types of alternations are assumed to feed the generally more productive, phonologically-conditioned rules, so we expect there to be limited effects of the interaction of these rules with other phonological processes. Even with that caveat, however, since readjustment rules are phonological and ordered, we expect to see some opacity effects such as the famous interaction of Canadian Raising and the Flap Rule. This seems to be a strong empirical prediction of the existence of readjustment rules. Somewhere there ought to be some phonological effect where an unproductive and radical readjustment rule such as *think* → *thought* or *seek* → *sought* has opacity effects (for example, an effect of the velars on surrounding phonology before they are replaced by the readjustment rules).

Double marking is a compelling place to look for this because of ordering. For example, one could argue that the reason that the past tense affix in *thought* is unvoiced is because voicing

[9] We would like to thank Lev Blumenfeld for bringing this to our attention.
[10] For example, they tend to be lexically-conditioned, fail to target natural classes, be unproductive, and result in only phonemic changes, not phonetic.

assimilation occurred before the deletion of the [k] by the readjustment rule. Such an argument would be very compelling evidence for the existence of a readjustment rule replacing *seek* with *sough*. However, in this example, the evidence is not compelling for two reasons: (a) the historical [x] was unvoiced and it is that which would have conditioned the unvoiced variant before the irregular form was preserved; and b) unconditioned devoiced past tense markers are not uncommon, though also certainly not productive (cf. *burnt*, *dreamt*). If double marking is really a phenomenon that needs to be accounted for (i.e. one that is a true prediction of MIO models), we should see systematic opacity effects in cases of double marking that can only explained by double marking. To our knowledge, in the twenty years since the advent of the double marking prediction, such evidence of opacity effects has not been adduced.

3.2 *Triple marking*

This brings us to a related case, one that would be treated as double marking in readjustment MIO models: the irregular English plural for the root *child*: i.e. *children.* It seems to us that a typical claim in a MIO model would be to treat *children* as an instantiation of the prediction in (3d). The vowel in *child* is shortened through the application of a readjustment rule that is conditioned by the c-commanding plural morpheme, while the lexically-conditioned affix *-en*, also found in *oxen* and *brethren* (which importantly exists in parallel with *brothers* but has lexicalized meaning), competes with *-s* and wins through secondary exponence-driven licensing. While straightforwardly accounting for the plural-induced irregular stem change and the stem-specific plural suffix, this analysis misses entirely they presence of the word-medial *-r-*.[11]

[11] Note that the word-medial –r- in *brethren* is part of the noun stem, *brother-*, which, as such, takes the same irregular plural suffix *–en* as *ox*, etc.

The -*r*- in *children* is a fossilized remnant of morphology from the late Old English plural *cildru* 'children'. *Children* gained the -*en* plural marking, the plural marker in Old English weak noun class (a noun class to which *child* did not belong), in Middle English. In a MIO analysis of Modern English, this -*r*- ought to be regarded as a *third* marking of the plural on the word *children.* Readjustment MIO models, which are held (e.g. by Embick 2012) to a limit of two morphological markers (i.e. double marking), have no elegant way to account for this third marking.

One (inelegant) solution to the issue would be to insert the -*r*- as an effect of the readjustment rule (i.e. /ʧajld/ is replaced with /ʧɪldr-/). If this is the strategy, then that suggests that the same could be done with the suffix -*en*; i.e. the readjustment rule could replace all of /ʧajld/ with /ʧɪldrɛn/, as conditioned by the plural which is a zero affix that is present in other environments. Indeed, this would make sense since the only reason to treat -*en* as a Vocabulary item at all is its presence in *oxen* and *brethren*, which could be easily argued to be group plurals (like *cattle*). At that point, though, the solution is arguing the same thing that the INT models argue: i.e. that the form *children* isn't segmentable and the apparent double marking is epiphenomenal.

The original readjustment solution carries with it the noticeable concern that this readjustment rule (which shortens the vowel and appends an -*r*- to the stem) is not a change to some contiguous string, but has two discreet changes (first vowel laxing and word-final consonant epenthesis). The other possible solution is to combine -*r*- and -*en* into one affix *–ren*, and stipulate that it is a unique affix specified for the single root √*child* (and it is therefore synchronically not connected to its etymon which is specified for other roots such as *ox* and

brethren). This solution is equally unsatisfying as this again represents a claim of unsegmentability for *-ren* and this would be the only word in English with this affix.

Essentially, in order to deal with *children* via readjustment, MIO model needs stipulate that the restriction on its aggressive decomposition is that it must end at double marking, even in cases of what would clearly be triple marking if we are to be honestly decompositional. Not only is this stipulation deeply unsatisfying, it reveals that MIO's double marking prediction appears to be *false*. There does not seem to be an upward limit of two realizations of a morpheme in a word form (one readjusted and one inserted). The word *children* seems to have three.[12] The only solution that allows MIO to maintain its double marking prediction is to argue that one of these three markings is not segmentable. There is, then, no categorical way to limit the aggressive decomposition except to stipulate that it must end at two. A much more parsimonious and elegant solution is to claim that *no* part of the word *children* is segmentable. Under this alternative account, *children* is a listed lexical item which is inserted into the syntactic contexts that would otherwise call for the segmented output which it blocks, i.e. *child-s* in the context of √*child* plus Plural. This is straightforward under an INT approach.

3.3 *Borrowed affixation*

[12] We anticipate that some readers may object to our objection to the analysis of *–ren* as the plural suffix for √*child* and, and in so doing, maintain that *children* is really a result of double-marking after all. We'll leave this question as a matter of analysis rather than one of empirical facts, but still insist that Embick's version of MIO is limited to double-marking. This claim should be falsifiable if any examples of uncontroversial triple (or any n-tuple marking > 2) can be found in some natural language. (As we discuss above, number of markings is not a prediction, limitation, or requirement for INT models, so it is beside the point in the evaluation of such models). If we are to search for such cases, though, clear criteria for identifying and distinguishing "number of morphological markings", in the relevant sense, need to be established. Most examples of readjustment rules in the literature, for instance, involve a single vowel change (e.g. *sing* > *sang*). But how many readjustment rules can count as "one marking"? To take a hypothetical scenario where a null suffix causes a vowel change (e.g. *child* > *children*) but also epenthesizes a consonant, e.g. an analysis where [r] in inserted by readjustment rule in *children* – is that, in concert with the vowel shift, one marking, or two? A lack of explicitness for the limits on readjustment operations is one of our major criticisms against allowing them in the theory in the first place.

Aggressive decomposition leads to readjustment analyses of borrowed morphology as well. On the most transparent end of the spectrum of borrowed morphology is Latinate morphology in contemporary English Vocabulary, which has often been treated as a part of the synchronic grammar in theoretical analyses of contemporary English. Within the confines of this discussion, treatments of words like *divinity, productivity, defensible,* and *violable* require powerful readjustment rules and extensive encyclopedic information. Of course, they also demand an explanation for why the more productive and semantically transparent forms – *divineness*, *productiveness*, *defendable*, and *violatable* – also exist in parallel. However, Latinate morphology is fairly well known to English-speaking morphologists (though not necessarily as well known to English lay speakers—cf. Latinate plural suffixes being erroneously placed on Latin-sounding words of Greek origin, e.g. *octopi*, or overextending a pattern to a different declension, e.g. *syllabi*), so it is relatively easy for English-speaking morphologists to make claims about decomposing Latinate words in the synchronic grammar of English.

At the opposite end of this spectrum is the more obscure word-internal morphology of loanwords from other languages, which can also be quite frequent in the Vocabulary of English, even while not always being obvious borrowings from a foreign language. An interesting case in point is the ~90 English loanwords from the Uto-Aztecan language Nahuatl (aka "Mexicano" or "Aztec") (Haugen 2009). These typically derive from secondary borrowings from European languages (from the 16th to the 19th century) or Mexican Spanish from the 19th century to the present. These loanwords include such everyday items as *tomato, chocolate,* and *coyote*, each of which involve, in the Nahuatl language, a root plus a suffix, *-tl*, which is an affixal marker of

non-possessed status;[13] thus, these words would appropriately be broken down morphologically into *toma-tl*, *coyo-tl*, etc.[14] Indeed, the native morphology of *Spanish* loanwords is also ignored for speakers of English, as in the case of *enchilada*, which derived from the Spanish verb *en-chil-ar* (in-chile[15]-verbalizer) 'to put chile in'. Even cases where Nahuatl loanwords into English, via Spanish, involve apparent word-internal complexity (e.g. *guacamole*, from Nah. *ahuaca-mol-li* avocado-sauce-ABS 'guacamole (= 'avocado sauce')', and *chipotle*, bastardized from Nah. *chil-poc-tli* 'chile.pepper-smoke-ABS', most likely get treated in the synchronic grammar as simplex roots by speakers of contemporary English.

Unlike the case of English morphology derived from Latin, or Old English, or Middle English, no morphologist would make the claim that we ought to decompose Nahuatl or Spanish morphology in these loanwords in the grammar of contemporary English. Indeed, such an idea universally would (or at least ought to) seem absurd. We suggest though, that it may be just as absurd to decompose morphology on the other end of the transparency scale (i.e. Latinate and moribund English morphology), because it is just as unlikely that these decompositions reflect actual synchronic English grammatical processes. The aggressive decomposition maintained within MIO theories seems to mandate accounting for all borrowed morphology (like the Nahuatl or Spanish loanword morphology mentioned above), or stipulating some line on the scale of transparency where it stops. INT models have no such requirement: *transmission* (Marantz 1997) can be stored as a listed item and inserted into a non-terminal node to capture the intuition that it is not simplex. Whether a particular word in a given language is really

[13] This suffix is referred to as the 'absolutive' in the Uto-Aztecanist literature, although it has nothing to do with ergative/absolutive case marking familiar from other language families. We adopt the traditional term, and its corresponding gloss (ABS), here.
[14] We ignore *chocolate* here since its morphological composition and etymology within Nahuatl is uncertain and subject to some debate. See Dakin and Wichmann (2000) for discussion of issues with various proposals.
[15] Note that *chile* is itself a Nahuatl loanword into Spanish, < *chil-li* chile.pepper-ABS 'chile pepper', the word-internal morphology of which is also ignored by speakers of Spanish.

morphologically composed by the grammar ought to be an empirical question. It is to this important consideration that we now turn.

3.4 *Morphological creativity and the exclusion of unproductive forms*

Sensitive readers may fear that we are perilously heading down a dark path towards claiming unlimited listed morphology. As much as MIO's decomposition seems unrestricted, INT's listing of morphology seems, potentially at least, to be equally unrestricted. If *transmission* and *slept* are listed, then why not *walked*? We suggest that there is indeed a categorical restriction that we ought to adopt to differentiate between those morphological processes which account for decompositionality, and those which we should assume are lexically listed. Bybee (1985) claims, for example, that less productive affixes have no representation in the lexicon outside of the complex listed words that they are a part of. This claim ultimately led to the restriction we are concerned with here: that which was proposed by Lieber (1992) for the explicit purpose of excluding unproductive morphology from the grammar: namely, *productivity* versus *creativity.*

Lieber (1992), following work by (Baayen & Lieber 1991), argues that a model that accounts for morphological processes within the syntactic component must eliminate non-productive processes from the set of data that it sets out to explain, as the syntax should be wholly productive (cf. Chomsky 1970). Lieber (1992) adopts Lieber & Baayen's (1991) algorithm to separate productive processes from those that are *creative*, following Schultnik (1961). Creative processes are crucially those that are not performed by the grammar but are, rather, those performed by the (extralinguistic) human creative faculty. As such they are done willfully by speakers and are noticeable. Such processes include the use of certain affixes (such

as -*hood* or -*dom*), blending, and backformation. Analogy, especially with moribund or borrowed morphological processes, would also fall under the domain of creative processes. Lieber and Baayen's (1991) algorithm measures productivity similar to the measure originally proposed by Aronoff (1976), except what Lieber proposes is that, rather than using a proportion of actual words to possible words, the algorithm calculates number of hapaxes created with the morphological process over the overall number of uses of the process. If the process is less likely to happen than coining in a given corpus, then the process is not productive.

We find compelling Bybee (1985)'s and Lieber's (1992) arguments that non-productive processes ought to be excluded from the productive grammar. The algorithm for determining that line has been a major topic of discussion in the work on productivity since Baayen & Lieber (1991), and the algorithm for setting this line has changed subtly over that time (see Baayen 1992, 1993, 1994; Baayen & Renouf 1996; Hay & Baayen 2002, 2003; among others). The details of that discussion are too extensive to summarize here, but if we are allowed to skip to the end (so to speak), the current state of the division between productivity and creativity is called "the parsing line" by Hay & Baayen (2002, 2003). This line is roughly the limit at which it is faster to recover a word whole than it is to parse it into its component parts. This line does not identify affixes as productive or unproductive, but rather identifies whole words as parsed or unparsed. Indeed, a good example from Hay & Baayen (2003) is the difference between *tasteless,* which is argued to be parsed by the grammar, and *listless*, which is not (i.e. it is "characterized by whole word access rather that parsing", Hay & Baayen 2003:2). As of Hay & Baayen (2002), the parsing line incorporates features of frequency, regularity, and transparency. Hay & Baayen (2003) later add phonotactics.

While the nature of the parsing line is sure to continue to be a fertile field of research in language processing and as such will equally surely change over time, we think that in principle Hay and Baayen's parsing line, or something very much similar to it in spirit, is an adequate categorical division between which complex forms are listed (i.e. which are not actually composed by the grammar) and those that are not. This, then, we suggest, is a solution to the problem of aggressive decomposition inherent to many RT analyses. How does all of this apply to our discussion of double marking?

3.5 *Productivity, listing, and double marking*

If we adopt the parsing line as a categorical way of sorting productive grammatical processes from creative extra-grammatical processes, INT models could then assume that any word that falls below the parsing line is listed as part of a Vocabulary item that includes the stem with its affixes and, as a unit, is inserted at a (non-)terminal node. We feel safe in assuming that the double marking that Embick (2012) discusses, as well as moribund processes that have been preserved diachronically, would fall squarely into this category of the extra-grammatical (i.e. below the parsing line), as would much borrowed morphology (clearly *-able* and *non-* are totally productive). Here we propose that this division from Lieber (1992), updated to the parsing line of Hay and Baayen (2003), be extended to RT. Complex lexical items below the parsing line would be listed and subject for insertion at non-terminal nodes.

In practice, this would mean that these non-productive forms are *suppletive* in the sense that they represent one Vocabulary item, not many. This view is similar to the fusion proposal put forth by Siddiqi 2009. In this view, double marking is neither an argument for nor against INT models. (Siddiqi's 2009 model is an MIO model and makes these complex listed item

claims). Double marking is just an instantiation of several different types of suppletive (listed) morphology. Instead, the proposed existence of double marking turns out to be a mere artifact of the MIO model's use of readjustment rules and aggressive decomposition. The supposed upward bound of two instances of morphological marking in double marking may well turn out to be a failed prediction of readjustment INT models. They predict not only that double marking should exist, but that it is upwardly bound at two. Following the same principles that lead to decomposing *slept* into double marking, we must also decompose *children* into triple marking, which readjustment MIO models can only stipulate as unsegmentable in order to provide an account for because the number of markings are capped at two in a readjustment model. This stipulation could as easily have applied to *slept* in the first place, and it probably would have were it not for the fact that readjustment MIO models actually predict the existence of double marking.

Indeed, not only does the listing of non-productive morphology account for double marking and borrowed affixes, it can also be extended to another supposed weakness of the item-and-arrangement approach to morphology: cran-morphs. Aronoff (1976) argues that all morpheme-based approaches (and this would be especially true of aggressively decompositional approaches such as MIO realizational models) are forced to assume the existence of cran-morphs in words such as *werewolf* – where *wolf* is argued to be a segmentable root, leaving *were-* as an empty morpheme that means nothing synchronically.[16] However, the existence of *were-* is necessary for *werewolf* to mean something different from *wolf.* Whether or not this is necessarily true of MIO models is debatable (for example, MIO models could assume that *werewolf* and *wolf*, or *cranberry* and *berry*, do not actually share a common root—though that might just be

[16] The morpheme *were-* meant 'man' in Old English. Many English cran-morphs have similar historical explanations; *cran-* itself, for example, comes from *crane*

begging the question), but it is certainly not true of INT models.INT models could simply assume that *werewolf* and *cranberry* are listed and inserted above the relevant roots (assuming they share a root with *wolf* and *berry*).

This argument can be extended to bound roots as well. Aronoff (1976) also argues that morpheme-based models need to predict a shared stem in words like *produce, deduce,* and *induce* because the obvious affixes need to be attached to something. Again, INT models can argue that these are listed items that may indeed be inserted into a projection above the relevant affixes or the relevant stems (or both). In this way, INT models can directly address some of the oldest criticisms of item-and-arrangement morphology. MIO models, on the other hand, are still subject to Aronoff's (1976) criticisms of bound roots and cran morphemes. Indeed, a great deal of work within DM has been done treating these bound forms as compositional (for example Harley 2009, which explains the lack of co-occurrence of prefixed Latinate forms such as *exhibit* with particles by arguing that the prefixes and the particles occupy the same syntactic position).

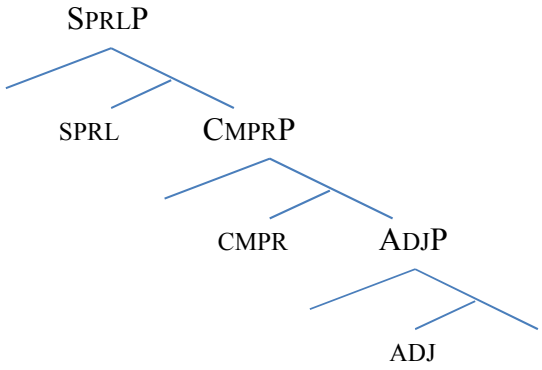
4. CONTAINMENT

The majority of Embick's (2012) argument against INT focuses on what he calls the *containment prediction* (previously described but unnamed as such in Embick & Marantz 2008). Containment is offered as one of the prize jewels in the crown of theories like DM, and Bobaljik (2012) provides a compelling case in point involving a very cross-linguistically robust generalization about suppletion patterns in adjectival paradigms. In short, Bobaljik proposes the *comparative-superlative generalization*, which has two parts: (i) that "if the comparative degree of an adjective is suppletive, then the superlative is also suppletive (i.e. with respect to the positive)" (p. 2), and (ii) that "if the superlative degree of an adjective is suppletive, then the comparative is also suppletive (i.e. with respect to the positive)" (p.2). This generalization is

meant to explain the otherwise unexpected observation that there are no clear cases of superlatives being derived from the regular ("positive") adjective form while the comparative is suppletive (i.e. *good-better-goodest, as per part i. of the generalization; this is Bobaljik's *ABA pattern); and, further, that there are no clear cases of superlatives being uniquely suppletive within a positive-comparative-superlative paradigm (i.e. *good-gooder-best, as per part ii. of the generalization; this is Bobaljik's *AAB pattern). The actually attested patterns involving suppletion, rather, include the English-like ABB pattern (with forms like *good-better-best*, where the superlative is derived from the comparative) and the Latin-like ABC pattern (e.g. *bonus-melior-optimus*, where the superlative is suppletive to both the comparative and the positive).

Bobaljik's explanation for these generalizations is based upon the syntactic structure that he posits for the morphology. In particular, Bobaljik adopts the containment hypothesis, wherein "the representation of the superlative properly contains that of the comparative" (p.4); this can be demonstrated by observing the structure that Bobaljik proposes, as in (4):

4) Containment in the Adjectival Domain (Bobaljik 2012: 81 [109])

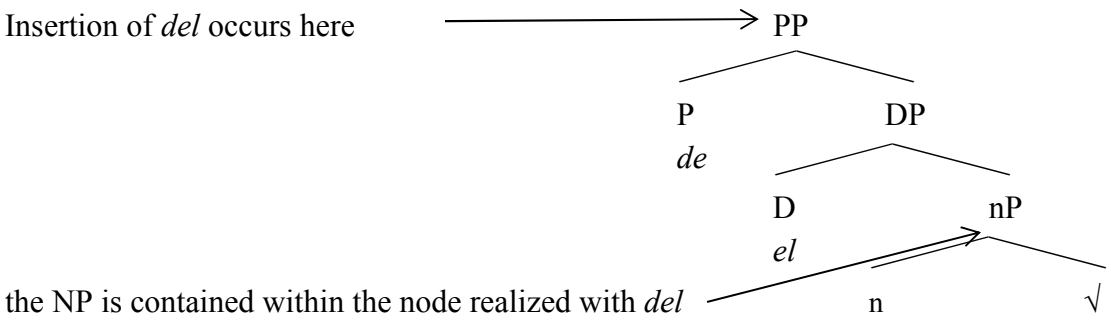


Any insertion process targeting the superlative head (SPRL) necessarily contains CMPR and ADJ. This is a quintessential MIO analysis featuring containment.

The containment prediction, at first glance, seems to be a fatal flaw for INT models. The crux of the containment critique is that, if insertion were to happen at a non-terminal node, it *must* be the case that *everything* under that non-terminal node is replaced by the listed form; for example, if insertion targets CMPR in (4) above then it must also subsume ADJ (and it also cannot subsume SPRL); insertion targeting SPRL must subsume both CMPR and ADJ.

Haugen & Siddiqi (2013) point to the example of the Spanish preposition/determiner portmanteau *del* as an example of a problematic case for INT theories. The lowest node to dominate both D and P is P', which arguably would be the site of insertion. However, if this happened, all of NP would also be replaced with *del*, meaning that the portmanteau would always "destroy" the noun it modifies (5).

5)



This prediction is clearly false. Embick (2012) and Embick & Marantz (2008) use the parallel French preposition/determiner portmanteau *du* (and *aux*) for the same illustration. Embick further elaborates that *du* provides a significant problem because *du* fusion only happens before consonant initial nouns (c.f. *du jus*), while in the case of vowel initial nouns, the determiner instead cliticizes to the noun (c.f. *l'arbor*). Embick (2012) claims that any movement that would displace the noun outside of the target non-terminal node prior to insertion would fail to predict

that the fusion would be sensitive to the vowel-initial quality of the noun. On the contrary, Embick (2007, 2010) has a MIO account with two different ordered readjustment rules (one that cliticizes and one that fuses) where one bleeds the other.

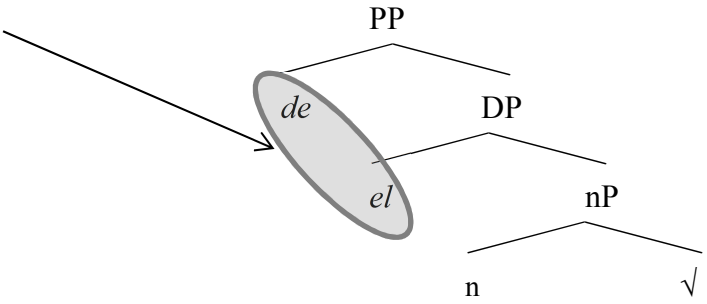
Embick's (2012) examples of failure of containment are not limited to preposition/ determiner portmanteaux. He also shows that in the case of the *more/-er* competition, *-er* forms ought to be prohibited from having complements, which is clearly false (cf. *Sarah is easier to get to know than John; Bill is prouder of himself than he ought to be*); that any verb with tense conditioned stem allomorphy couldn't have an object, which is also clearly false (cf. *John sang the rest of the song*); that complex forms derived from roots that show stem allomorphy could never show that same allomorphy (provided that the conditioning morpheme dominates the prefix), which is once again clearly false (cf. *understood, withstood, John outsold Jim, John resold the painting*).

It seems to us that Embick & Marantz (2008), Embick (2012), and Haugen & Siddiqi (2013) may be correct and containment could indeed be a fatal flaw for INT models, strictly speaking. Any model such as Radkevich (2011), which has insertion at non-terminal nodes, or Starke (2009) (which has cyclic override, which effectively functions as if it has insertion at non-terminal nodes), will fail to capture any instance of fused morphology dominating any other overtly expressed material, and indeed predicts that such instances should not exist. However, while these models do in fact specifically claim that insertion happens at phrase-level and bar-level nodes (the strict interpretation of non-terminal nodes), there is also a model that claims insertion can target several terminal nodes at once without claiming that it is targeting non-terminal nodes. This is the model provided by Svenonius (2011). Svenonius (2011) proposes that

insertion targets what Melnar (2004) and Williams (2003) call *spans*. A span is a "subpart" of a complement line within an extended projection (Grimshaw 2000).

6)

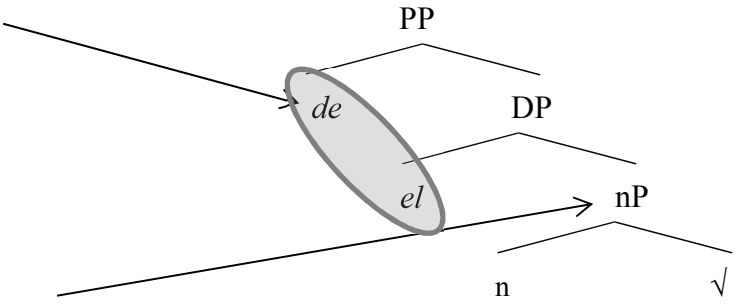
span that includes *de* and *el*



Svenonius claims that, since insertion always target spans (with the understanding that a terminal node constitutes a minimal span), insertion can realize a contiguous series of nodes without necessarily realizing any dominated terminal nodes (7).

7)

Insertion of *del* occurs here



the nP is not realized by *del*

Svenonius (2011) claims that *spanning* is precisely the solution to the containment prediction (without calling it that), and, indeed, the data that he examines is the same data that Embick (2007, 2010, 2012) analyzes: the French preposition/determiner portmanteau *du* and its alternation with the clitic *l'* (as per 7 above). Svenonius (2011) claims that the spanning analysis

is superior to Embick's (2007b, 2010) readjustment analysis[17] for two main reasons: a) (as we have discussed above) readjustment is overly powerful and unrestricted while spanning is restricted; and b) the rules that Embick proposes are syntactic but make reference to phonology, which violates the nearly universally accepted Principle of Phonology-Free Syntax (Zwicky & Pullum 1986). Svenonius (2011) puts it this way:

8) Svenonius (2011) on DM:

> "The problem for DM is essentially that if lexical insertion is to be applied to terminal nodes, then before lexical insertion occurs, there must be some independent organization of the syntax into terminal nodes. Here, items which are only visible through lexical insertion itself (namely the portmanteaux *au* and *du*) are the only clue that P and D must be reorganized into terminal nodes." (6)

We are sensitive to Svenonius's (2011) concerns. One of the main proposals of Embick (2000) is that Vocabulary items that realize roots are present throughout the syntax. In his version of the theory, only functional material is realized with late insertion. Indeed, it is this proposal of Embick (2010) that Haugen & Siddiqi (2013) argue that root suppletion is counter-evidence for; see also Harley (2011) and Bobaljik (2012). For Embick (2010), it is absolutely mandatory that syntax is sensitive to phonology. That is the purpose of the proposal.

While we find Svenonius's (2011) proposal compelling, it is not without its own problems. It can easily account for many cases of containment, such as most of the adjectival suppletion patterns discussed by Bobaljik (2012). For example, when SPRL is based on the CMPR form it forms a span with CMPR, which itself is either based on the positive adjectival form (thus forming a three-headed span composed of ADJ-CMPR-SPRL, yielding an A-A-A pattern, or suppletive with respect to ADJ, thus forming a two-headed span composed of CMPR-SPRL,

[17] Embick's (2007, 2010) analysis employs two ordered readjustment rules which he calls local dislocation. The first adjoins the determiner to a right adjacent word if that word is vowel initial and deletes the vowel. The second adjoins it to a left adjacent preposition and replaces its phonology. The first rule bleeds the second.

yielding the A-B-B pattern; ADJ, on this analysis, would then form its own span). A problem arises, however, because Svenonius's obviation of containment should allow for the (apparently unattested) *A-A-B pattern, since nothing on Svenonius's analysis forces containment for a given head—i.e. SPRL need not contain CMPR or ADJ. Other beneficial aspects of Bobaljik's containment account would also be potentially lost, such as Bobaljik's *synthetic-superlative generalization*: i.e. that "no language has morphological superlatives (x-*est*), but only periphrastic comparatives (*more X*)" (p.3). This observation follows from Bobaljik's analysis where CMPR is always contained under SPRL.

We would like to suggest that spanning is not the only proposal that could accomplish insertion outside of terminal nodes without proposing that insertion happens at bar-level and phrase level nodes. These proposals would bypass the containment problem (though they encounter other problems of their own -- we leave that to future research to explore). For the remainder of this paper we will propose and evaluate two additional non-terminal insertion models. The first will propose that non-terminal insertion is limited to complex heads, but we will show that such a proposal is fatally flawed. The second will propose that insertion is purely phonological and is post-linearization; as such, this latter proposal is worth exploring in more detail.

4.1 *Non-terminal insertion is limited to complex heads*

We would like first to suggest that a large number of the containment problems disappear if non-terminal insertion could be limited to complex heads formed through head-movement (Baker 1988), morphological merger (Marantz 1984), Local Dislocation (Embick 2007a), or some similar process. This would restrict insertion to X^0, if not to non-terminal nodes strictly

speaking. This would not be an arbitrary restriction. On the one hand, complex heads are formed, by hypothesis, through post-spell-out morphological processes rather than the syntax per se,[18] so in this sense they really are a different domain. On the other hand, DiSciullo & Williams (1987) thoroughly detail the properties of X^0 and identify it as a domain separate from syntax proper. We propose the following potential principle for non-terminal insertion (as an alternative to spanning and cyclic override):

9) Non-terminal insertion principle

> Insertion may occur at a non-terminal X^0 provided that those features realized by the inserted Vocabulary item are a superset (though not necessarily a proper superset) of those features that could otherwise be expressed by separate overt Vocabulary items at the contained X^0s (both terminal and non-terminal).

Such a principle would restrict non-terminal insertion to X^0 (i.e. both simplex and complex heads) and to Vocabulary items that express the same content that would otherwise be realized by regular morphology (thus stopping an irregular form realizing one feature from "blocking" inflection for another).

We see several pros and cons to a proposal such as this. On the one hand, the most egregious of the containment failures for INT models is that complements, such as the arguments of irregular verbs, ought to be blocked by non-terminal insertion. This proposal would eliminate this problem for INT models nearly entirely (though the separate problem of stipulated morphological merger of heads such as in the case of Spanish *del* would persist). The cost of this proposal for INT models would be the loss of the accounts of word/phrase blocking such as the *more/-er* alternation. As mentioned above, the loss of such an account may not be too high a cost

[18] Depending on one's analysis, head movement may or may not be post-spell-out. We leave that discussion to another venue and assume for the sake of this argument that it is post-spell-out.

to pay as the *more*/*-er* alternation is not exceptionless, as the case of sentences like *I am more sad than angry* attest (Embick 2007).

This novel proposal would not be able to account for the shared irregular inflection in derived forms (such as *understood* and *withstood, undersold* and *outsold*). We don't find this concern too compelling, though, as these types of affixations tend to be irregular themselves. For example, *understood* is only related to *stood* in form; there is nothing compositional about the meaning. It is clearly a listed item. Similarly *withstood* uses a morpheme (*with* 'against') that hasn't existed productively in English for a millennium (the current prepositional *with* is a more recent Norse borrowing), and it only appears as a fossilized morphology in compounds such as *withhold*, *withdraw*, *within*, *without*; these are never compositional in the synchronic grammar of Modern English. Furthermore, *withstood* frequently regularizes from speaker to speaker to *withstanded* (indeed, *grandstanded* is perfectly regular; Lexicalist models, like Kiparsky (1982), attribute its regularity to its being converted from a noun; we are not sure that DM has the same mechanism).[19] Similarly *undersold* is clearly also not compositional. Indeed, it is not even clear that there is an active equivalent (Cf. *We will not be undersold!* versus *?We will undersell everyone!*) outside of specific jargon (*We hope this stock doesn't undersell*). These type of constructions can very easily be treated as analogy and, as such, below the parsing line and therefore extragrammatical. In fact, it seems like an extragrammatical treatment is preferable, especially in cases such as *sabertooths* (the cats) versus *saberteeth* (the teeth) (Zwicky 1985; DiScullio & Williams 1987). The one that is most compelling is *outsold*, though again *out-* has a

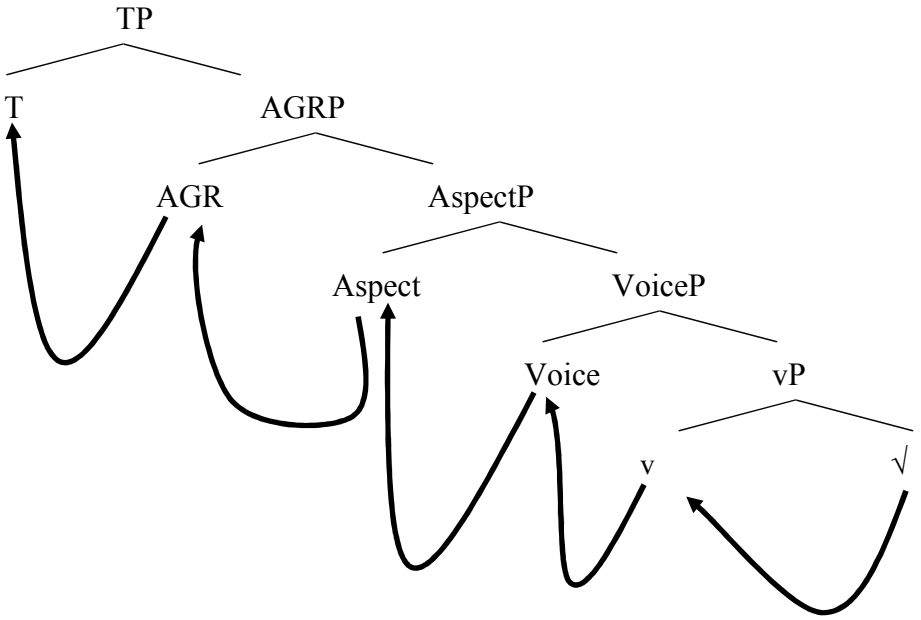
[19] Arad (2003) provides a DM account of denominal verbs in English and Modern Hebrew which implements Kiprarsky's notion by claiming that some denominalizations involve incorporation of Root into v, while others nominalize Roots by incorporation of the Root into n before incorporating into v. It is not obvious that some of the generalizations (about English) which Kiparsky wanted to account for actually stand, however; see Harley and Haugen (2007) for discussion, and the proposal that all Root incorporation in English is Root to v with no intermediate n.

certain metalinguistic quality to it that we are hesitant to call productive (*I out-Mary'd Mary!*). With this caveat, we do concede that some of these may be productive (i.e. above the parsing line) such as *rewrite/rewrote*. We return to this below.

Another argument against this alternate proposal, which we would anticipate from Embick (2012) and like discussions, is that it is *trans-derivational*, much like other such economy constraints as *Minimize Exponence* (use the fewest Vocabulary items necessary to express syntactic features; Siddiqi 2009) which is regularly used in Nanosyntax accounts such as Svenonius (2011) to motivate phrase/word blocking. This constraint must look at other derivations to conclude whether its insertion is ideal. To some extent, this is a forceful criticism as the implementation of some kind of trans-derivational evaluation metric is not cognitively realistic. On the other hand, this type of economy constraint is typical of Minimalism (cf. Chomsky 1995's *Minimal Link Condition*); but see Johnson and Lappin (1999) for critique.

However, there is an additional, striking concern that we have about this proposal. Complex heads are created, by hypothesis, through head movement. The order that they are composed is absolutely essential for this proposal to work. For example, in a fusional language, the verb raises through a variety of functional heads, including (under a standardly assumed architecture): v, voice, Aspect, AGR, and T, as seen in (10).

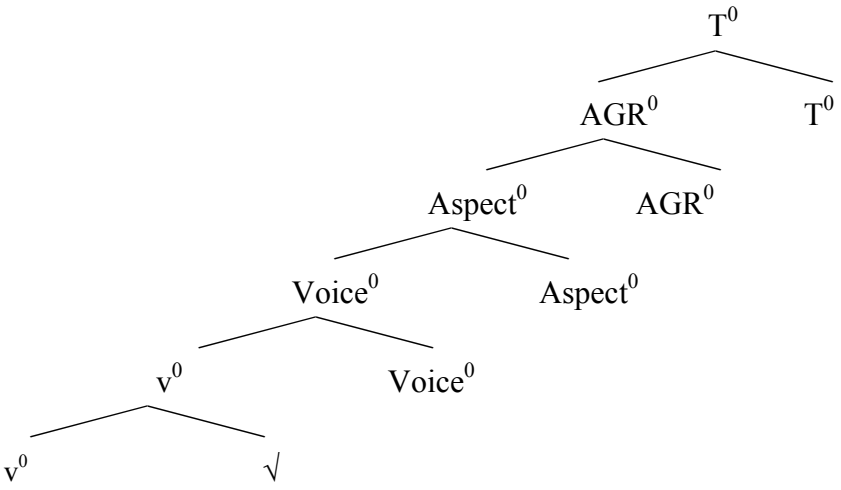
10)



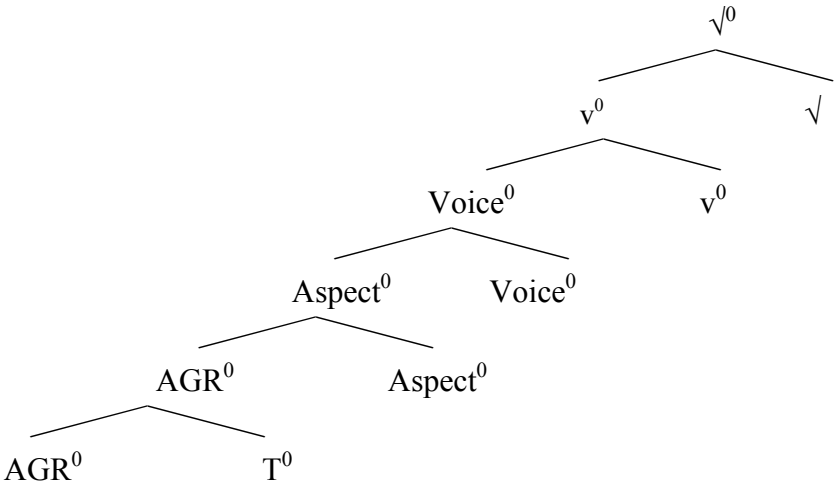
We see two theoretically possible resulting structures for such a complex head. In (12a) the root is the root node and it is this at the bottom of the tree. In (12b) the functional material builds on itself and the root is on the top of the structure.

12)

a) √ on bottom



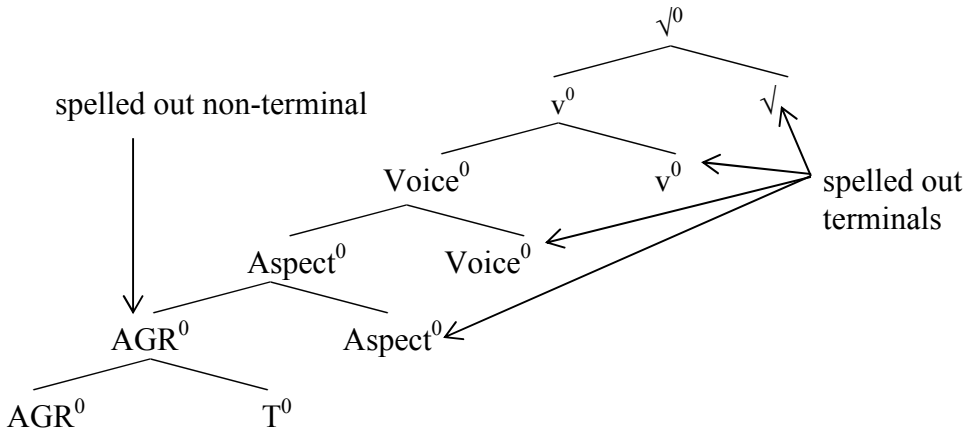
b) √ on top



The problem with this is that this model is only compatible with the root c-commanding the rest of the material, as in (12b), otherwise the root would be replaced with all fusional morphology in every fusional language (12a). This is clearly not the case. However, (12b) is very much the opposite of typical structure assumed by item-and-arrangement analyses and is thus fairly unsatisfactory. This does allow for a testable prediction of this model: if this model is true, barring some ad hoc movement mechanism, fusional material should always fuse from the outside in (13):

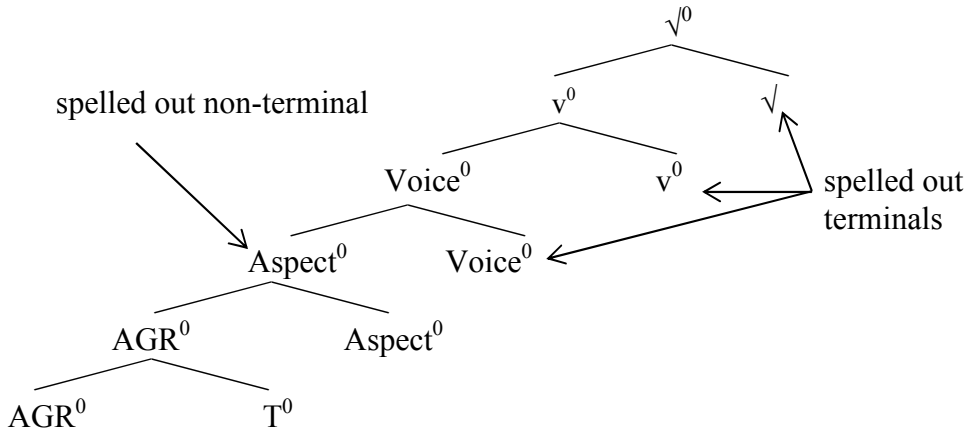
13) Outside in non-terminal replacement

a)



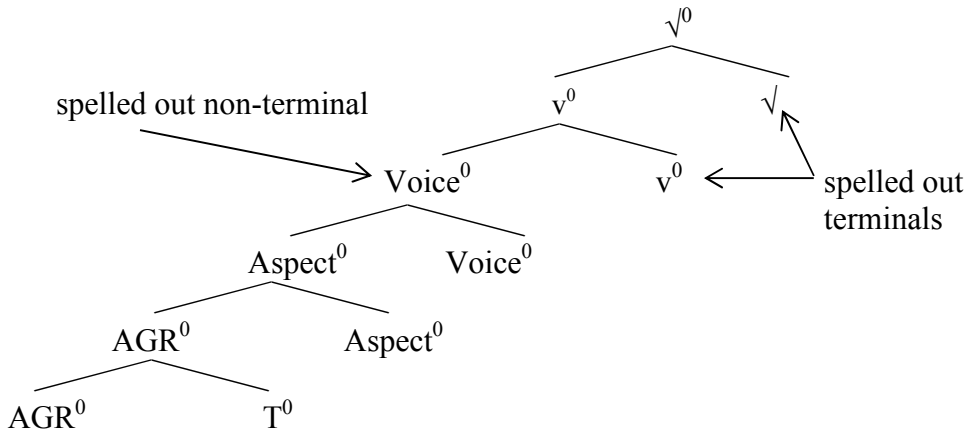
Surface order: √ + v + voice + aspect + agreement/tense

b)



Surface order: √ + v + voice + aspect/agreement/tense

c)



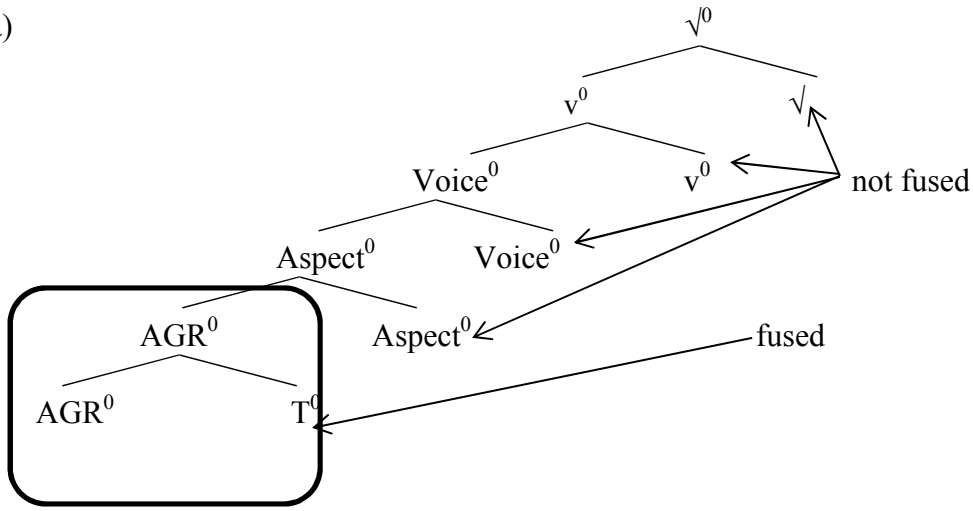
Surface order: √ + v + voice/aspect/agreement/tense

This seems to us to be precisely the opposite prediction from the one made by the Complexity-based Ordering hypothesis (Hay & Plag 2004, Plag & Baayen 2009) and is in stark contrast to Bobaljik (2000)'s standardly adopted proposal that spellout occurs from the inside out.

However, what this illustration shows is that MIO models that employ fusion make precisely the same prediction. The operation of fusion is defined in the literature as the collapse of two sister nodes into a single node (Kandybowicz 2007). If the requirement is that the two fused nodes must be sisters (and that is the only way that makes sense within the confines of DM), then fusion must also be limited to (12b) and it must also make the same outside-in prediction, as shown in in (14).

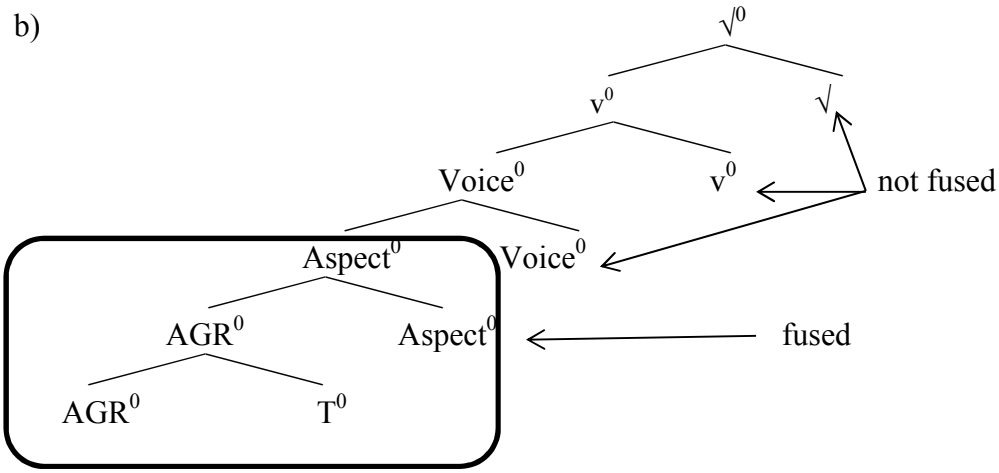
14) Outside-in fusion

a)



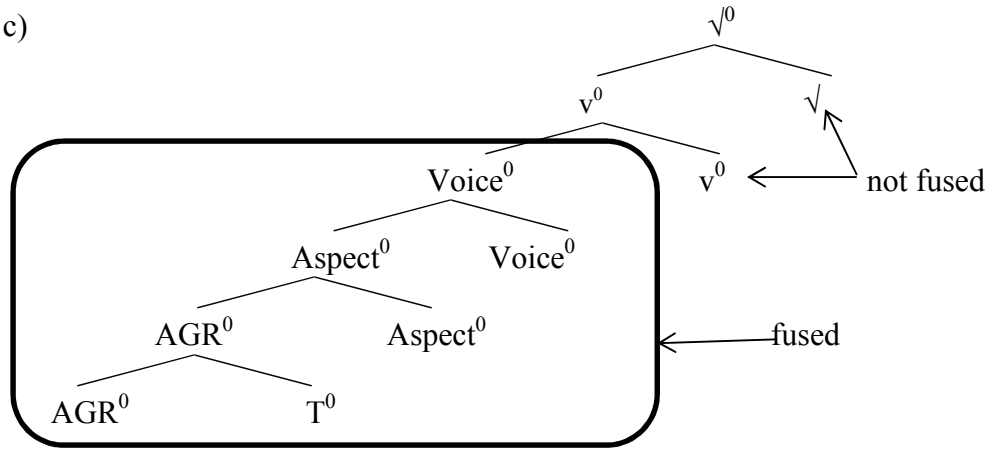
Surface order: √ + v + voice + aspect + agreement/tense

b)



Surface order: √ + v + voice + aspect/agreement/tense

c)



Surface order: √ + v + voice/aspect/agreement/tense

While this is not fatal for MIO models or X^0 INT, we find this particular aspect of both models to be unsatisfactory as it is in direct conflict with the complexity-based ordering hypothesis (Hay & Plag 2004, Plag & Baayen 2009), which seems to us to be a strong description of typological patterns. We therefore conclude that this approach to insertion is, in the end, untenable.

4.2 *Post-linearization Insertion*

We would like to propose a rather radical revision to the non-terminal INT model that has none of the weaknesses described above, and which is also not susceptible to Embick's (2012) containment critique. Furthermore, while it functions very similarly to Svenonius's (2011) *Spanning* proposal, it does so without proposing that hierarchical structure is replaced with insertion. We propose that insertion is *post-linearization.* After linearization,[20] Vocabulary items

[20] Linearization is the process by which the hierarchical structure of an utterance is replaced by the linear string of sounds that we actually speak and perceive. Linearization is usually assumed to be either the last stage of syntax or the first stage of phonology. In DM, traditionally, insertion is before linearization. Indeed, INT models only work with insertion before linearization because in INT models insertion explicitly replaces syntactic structure, which is

realize morphemes on a one-for-one basis (perhaps constrained by harmony-theoretic constraint interaction such as proposed by Bye & Svenonius 2012 or Haugen 2008, 2011). The following principle would allow a series of contiguous morphemes to be realized by a single Vocabulary item (in essence doing the same function of non-terminal insertion and spanning):

15) Post-linearization contiguous morpheme insertion principle:

> Insertion may realize multiple adjacent X^0s (features) provided that the features realized by the inserted Vocabulary item are a larger subset of the string of adjacent X^0s (features) than that which could otherwise be expressed by separate overt Vocabulary items at the contained X^0s (features).

This principle works very well to capture the types of stem allomorphy and blocking effects we have been discussing when combined with the Minimize Exponence constraint originally proposed by Siddiqi (2009), which says that the most economical derivation (i.e. the one to be preferred) is the derivation that maximally realizes the features of a derivation with the fewest possible Vocabulary items. The combination of these two principles will require that a fused form realizes a string of contiguous features if and only if it expresses the same features that would be expressed by a greater number of overt Vocabulary items. Let us consider some examples to demonstrate the effects of this kind of analysis.

4.2.1 *English participle and past tense convergence*

As an illustration of this principle we turn to the convergence of the past tense and the past participle in English (which is discussed in Starke 2010, but the details of an analysis are not described there). Despite a tradition in English which typically posits a three-way distinction (cf. *write, wrote, written*), in fact, for most roots, the pattern is not [A, B, C]. Rather, it is [A, B, B] both as a result of regular affixation (*kick, kicked, kicked*) and irregular realization (*bring,*

---

gone after linearization. Embick (2010, et seq), which assumes an MIO model, also employs insertion after linearization.

brought, brought; sit, sat, sat; sleep, slept, slept; cling, clung, clung; feed, fed, fed etc.). Even when the [A, B, C] pattern does occur, it is extremely uncommon that one is regular and the other is irregular (e.g. *show, showed, shown; swell, swelled, swollen; sew, sewed, sewn)*. It is a rather well known fact about English that the irregular participle and the irregular past continue to converge, though sporadically from dialect to dialect. Examples of the irregular past tense used in place of the irregular participle are rather easy to find (16) (see Kayne 1997 for discussion).

16) Irregular Past for Irregular Participle

a. I shouldn't've **ate** that cake last night. (cf. *eaten*)
b. I got **bit** three times (cf. *bitten*).
c. He must've **rode** his bike (cf. *ridden*)
d. I shouldn't've **drove** to the party (cf. *driven*)
e. You should've **went** the other way (cf. *gone*)

But much less frequently the irregular participle "absorbs" the irregular past (17).

17) Irregular Participle for Irregular Past

a. I **seen** that movie yesterday.
b. I **been** there yesterday.
c. I **done** that yesterday.

To generalize the data, this appears to be the distribution of the past and past participle in English (18):

18) <u>Distribution of Past Tense and Past Participle in English</u>:

a. The regular expression *-ed* realizes both [past] and [participle].

b. Typically, irregular forms realize the √ and both [past] and [participle]:
brought, sat, slept, fed, clung

c. Less frequently √+[participle] is a different irregular from the √+[past] irregular form: *wrote, written; drove, driven; ate, eaten*

d. Sporadically, the √+[past] is regular while the √+[participle] is irregular:
showed, shown; swelled, swollen; sewed, sewn; shined, shone

We suggest that that the post-linearization INT model that we present here is precisely suited to account for these data. First, we need to assume that there must be some common feature between the past tense and the past participle, otherwise there is no way to capture the distribution of *-ed* in an underspecification model. Drawing on the semantics arguments previously made by Iatridou, Anagnostopoulou, & Izvokski (2003) and Pancheva (2003), namely that the past participle should be treated within the framework of DM as a morphological realization of boundedness (Jackendoff 1983, 1990, 1992), we will assume the following: boundedness is always sub-morphemic (in the Nanosyntax sense; Starke 2010) and is projected below past tense and below the past participle.[21] This gives us the following two post-linearization strings for participles and past tense (19):

19) Post-linearization strings for participles and past tense

a. Past Tense: √+[bound]+[past]
b. Participle: √+[bound]+[participle]

At that point, it is straightforward to give the feature specifications of particular Vocabulary items to predict their insertion. For example:

- The regular *-ed* is specified for [bound] and there is no competing VI that is specified for [past] or [participle], so *-ed* in default situations will realize both [bound] and the adjacent [past], *and* it will realize both [bound] and the adjacent [participle]. The roots of regular verbs will always surface because, for example, *talk-ed* is competing with just *talk* and just *-ed* for insertion into the string √TALK+[bound]+[past] and, of the three

[21] In a MIO DM approach, we would assume that the terminal node carrying [past] also carries [bound] as does the terminal node that carries the feature [participle], but the post-linearization approach does not have access to fission and so then must assume that every terminal node carries one and only one feature (what Embick calls *morpheme*), and, similarly, that every feature is contained in some syntactic terminal node. For the sake of discussion, let us call the relevant terminal for the feature [bound], and the head is bound^0 which projects a "boundedness phrase", boundP.

choices, *talk-ed* expresses the most information with the fewest Vocabulary items (VIs).

- In paradigms such as *write, wrote, written*: *wrote* is specified as realizing the √WRITE, and [past]; *written* is specified as realizing √WRITE, and [participle]. *Wrote* is competing with *write+ed* for insertion into √WRITE +[bound]+[past] while *written* is competing with *write+ed* for insertion into √WRITE +[bound]+[participle]. In both cases, the irregular form wins because it realizes the most features with the fewest VIs.

- In paradigms such as *buy, bought, bought*: *bought* is specified as realizing √BUY and [bound] so it appears in both environments and defeats *buy+ed* because it is one VI rather than two—as such, it realizes the most features with the fewest VIs.

- In paradigms such as *shine, shined, shone*: *shone* is specified for √SHINE, and [participle] so it blocks *shine+ed* into √SHINE +[bound]+[participle]. However, it cannot be inserted into √SHINE +[bound]+[past] because of the feature [participle] it realizes, so instead *shine+ed* is inserted there.

However, there is a final pattern of irregular English verb inflection that we have yet to cover because it showcases a concern we have about this proposed model. That pattern is [A, A, A] and is typically found with roots end in an alveolar stop (*hit, hit, hit; bid, bid, bid*). This pattern is likely a result of the clash of the root coda and the regular alveolar affix *-ed*.

We anticipate that any INT approach would have to stipulate that there are two Vocabulary items *hit* and that their homophony is an historical artifact. This is because the optimal form of this pattern is always going to be to find *some* way to express [bound], most likely with the regular affix (*pitted, kitted*). Similarly, typical MIO models need to stipulate a lexically conditioned zero allomorph here. Siddiqi's (2009) fusion proposal actually accounts for these quite nicely, but by stipulating negative specifications for [bound] in all forms where the [A, A, A] pattern doesn't occur. This shows a problem that this model and perhaps all INT models have with some forms of zero inflection: if a zero past tense form (*hit*) or a zero plural (*deer*) is indeed the effect of underspecification and thus is inserted at any relevant node dominating the root, then the problem is that *any* root ought to be able to do this and thus there

needs to be some restriction (such as Siddiqi's 2009 mechanism) that allows it in irregular cases but prohibits it elsewhere. Again, this is not fatal, as an INT model and the one that we are proposing here can just stipulate two homophonous Vocabulary items (a stipulation that is not entirely unsatisfactory as these appear to be sporadic and unpredictable and in many ways seem to be creative—compare the prescriptively "correct" (within the Star Wars universe) plural *Jedi* to the productive plural *Jedis* (used by casual fans of said universe), but clearly a more parsimonious account would be preferable.

4.2.2 *-ity* vs. *-abil-ity*

Another phenomenon where this model provides an elegant account is the apparent productivity of *-ity*. The nominalizer *-ity* is unproductive *except* that it can freely attach to words previously affixed with *-able* (Aronoff 1976, Lieber 1992). For example, we could talk about an electronic attachment being *un-email-able*, and *-ity* could freely attach to that so we could then talk about its *un-email-abil-ity.* Post-insertion INT allows for us easily explain these data: there is an affixal Vocabulary item *-ability* that can realize the string *-able*+[n] rather than postulating that there is a Vocabulary item *-ity* that can only affix to a fixed set of specific words, and also those words previously affixed with *–able*. As a happy side effect, this account also helps us to explain the *-able* → *-abil* allomorphy without resorting to readjustment rules.

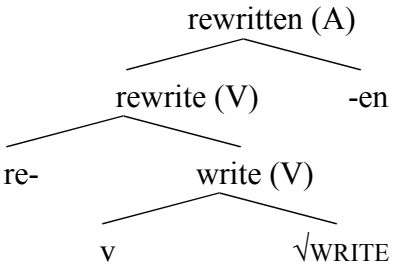
4.2.3 *Bracketing paradoxes*

This brings us back to our discussion of words like *understood* and *withstood*. While the combination of *under* and *stood* is surely below the parsing line, we suspect that in the case of forms such as *rewritten, unbent, untold, unborn, undeath, undone,* and *rebirth* the addition of the prefix is likely clearly above the parsing line (i.e. easily parsable—therefore, likely, not listed).

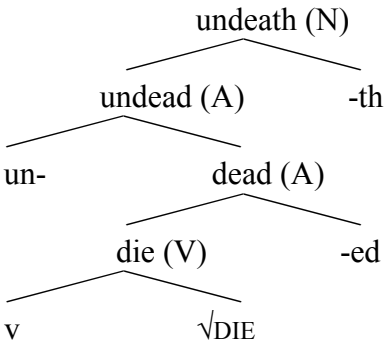
What is interesting about forms such as these is that, semantically speaking, the unproductive morphology is outside of the productive morphology. For example, *rewritten* is the irregular participle of productive and easily parsable *rewrite*, *unbent* is the irregular participle of totally transparent *unbend*, and *undeath* is the moribund nominalization of *undead* (20).

20) semantic/syntactic trees:

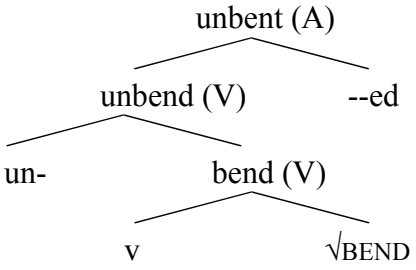
a) rewritten



b) undeath



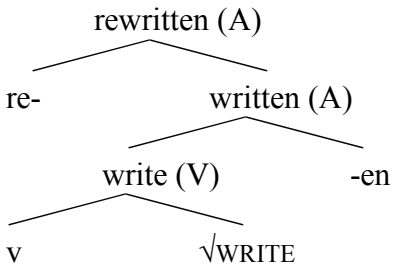
c) unbent



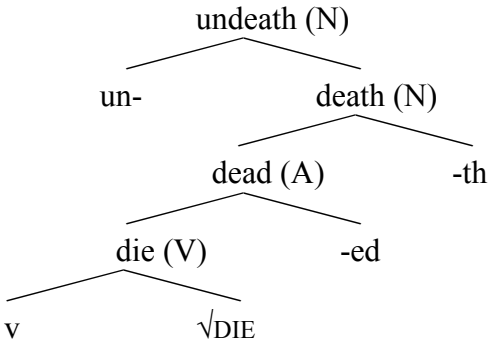
However, according to the Complexity-based Ordering hypothesis, these forms ought to be constructed as adding the prefix to the irregular stems. For example, *re* is affixed to *written*, *un* is affixed to *death*, and *un* is affixed to *bent* (21).

21) complexity-based trees:

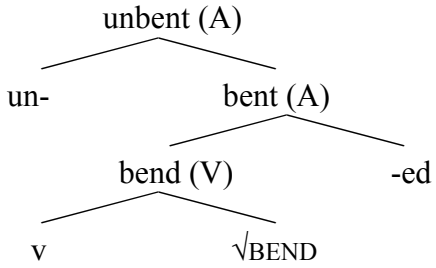
a) rewritten



b) undeath



c) unbent

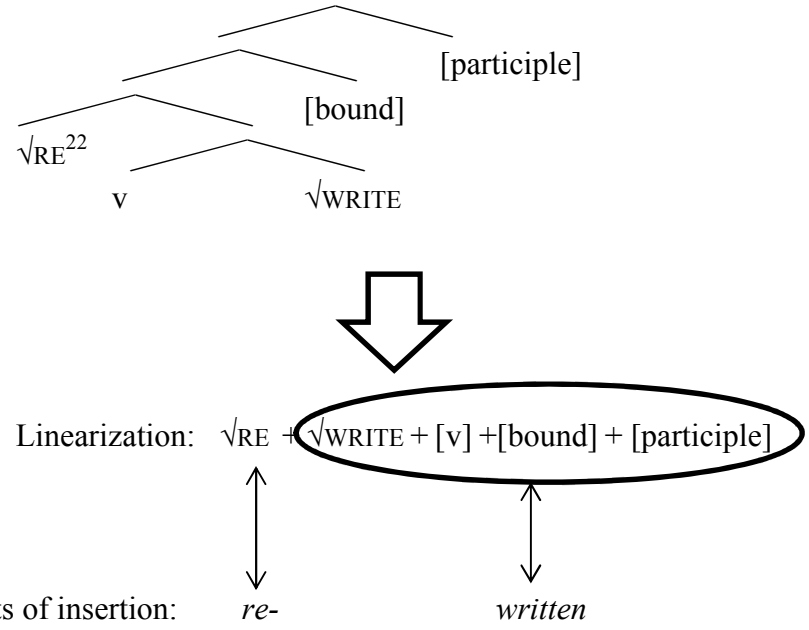


In this way, these forms are bracketing paradoxes not unlike *transformational grammarian* or *unhappier*.

These forms pose a containment problem for INT models and not for MIO models (i.e. the prefixes are syntactically below the stem allomorphy-conditioning affix, thus the prefixes ought to be "absorbed" into the irregular morphology). INT models have two unsatisfactory solutions to these data: (a) *undone, rewritten,* and *unbent* are listed (this seems very unlikely as they are transparent and productive), or (b) for some reason the prefixes are projected above the suffixes (counter to every morphosyntactic analysis of these types of forms that we are aware of). The post-linearization INT model that we propose here has no such problems. Indeed, this model predicts that these kinds of cases ought to exist. In this proposed model, it is irrelevant whether the prefixes are inside or outside of suffixes. All that matters is that the suffixes are adjacent to the stems, as we show in (22):

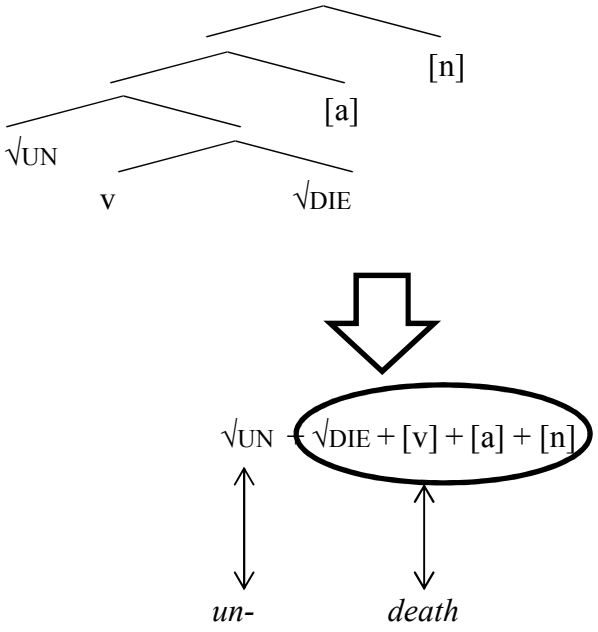
22) post linearization insertion derivations

a) rewritten

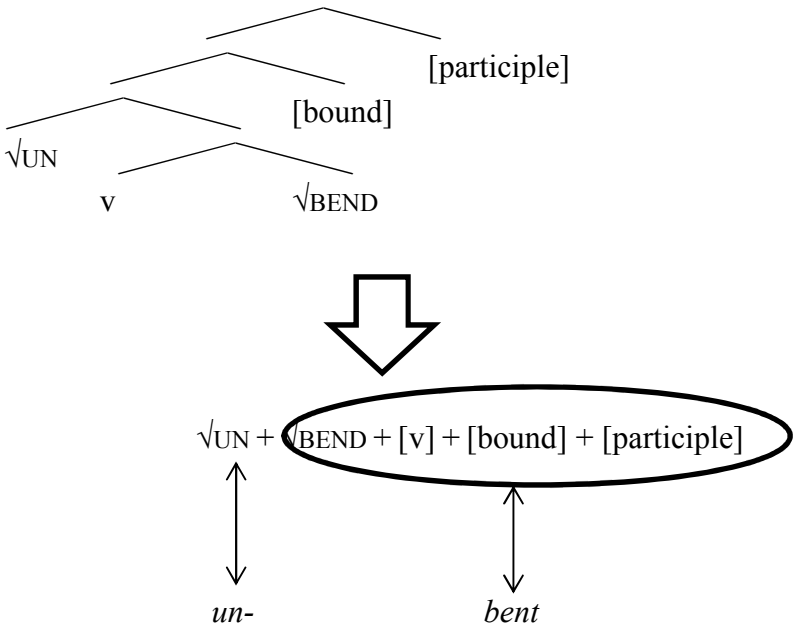


[22] We use for shorthand for whatever meanings are expressed by *re-*. It is not our intention to make any claims about derivational morphology with extragrammatical meaning such as *un-* and *re-*.

b) undeath



c) unbent

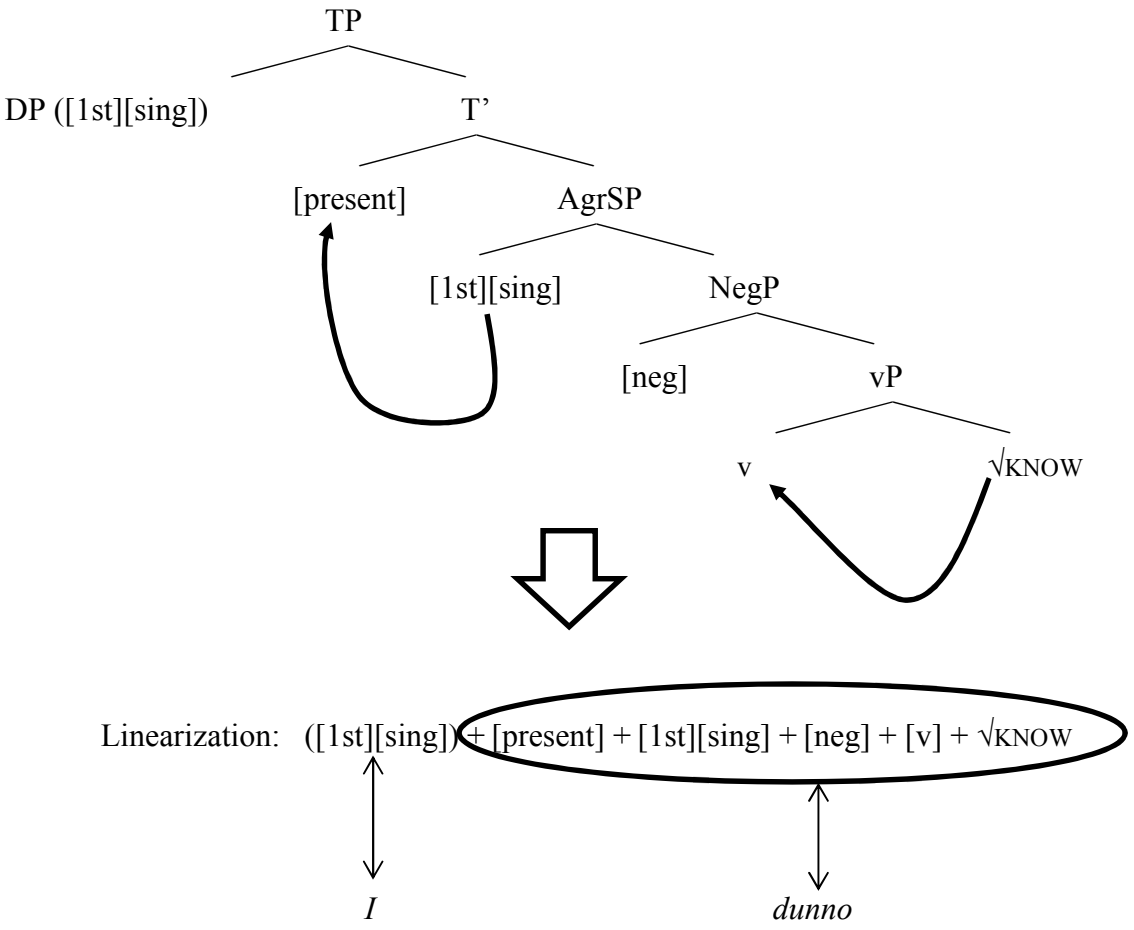


In this way, our account for these types of bracketing paradoxes mirrors Lieber's (1992) analysis, where hierarchical structure is irrelevant for the morphological licensing of affixation in *transformational grammarian* and *unhappier*—all that is relevant is adjacency to the stem. Similarly, not only can post linearization insertion account for these bracketing paradoxes, it predicts that they ought to exist.

4.2.4 *Contractions*

Yet another phenomenon that merits a brief mention is contractions that cross several syntactic nodes, such as English pseudo-modals. For example, *gonna, wanna, hafta, oughtta, I'm'a*, etc. Similar to these are contractions of frequent phrases, such as *dunno*, and irregular stem allomorphy triggered by clitics, such as *won't.* We definitely do not have the space here to plunge into the conditioning of *wanna* contraction (see Lakoff 1970, Lightfoot 1976, Postal & Pullum 1978), but we feel that we can safely hypothesize that, within RT, these forms are listed items. As listed items, they clearly offer more data that is more readily accounted for by INT models than by MIO models. For example, *dunno* replaces the string *do not know* which is at least five different syntactic nodes within the expanded VP (AgrS, T, Neg, v, and √) (23).

23)



Any RT account that treats *dunno* as a word of English (i.e. a Vocabulary item) rather than a phonological frequency effect (see Bybee & Schriebman 1999 for discussion) is either going to need an account that spans several syntactic nodes or stipulates head movement. Both Svenonius's (2012) *spanning* and the post linearization account proposed here can easily account for words like *dunno* parsimoniously without containment concerns. MIO models cannot.

We suggest that this type of proposal is an extraordinarily simple and elegant solution to stem allomorphy, borrowed morphology, and word/phrase blocking, though like the default

fusion in Siddiqi (2009)[23] we expect that there will be evidence that it needs to be constrained beyond the principle in (15). We will leave that to further research and development of such a hypothesis though.

5. CONCLUSION

In summary, we have argued that Embick's discussion of double-marking and containment does not conclusively decide in favor of MIO theories over INT theories on empirical or conceptual grounds. Rather, both theory types face serious challenges to one degree or another: MIO on the grounds that it forces (and is limited to) double marking (while nicely accounting for containment), and INT on the grounds of being problematized by containment (and being neutral with respect to double marking).

We have suggested that there is a third alternative within Realization Theory, involving insertion of Vocabulary after linearization, which is more in line with some recent approaches to DM which have conceptualized PF as a harmony-theoretic module of grammar: e.g. Haugen (2008, 2011)'s approach to prosodic morphology such as reduplication and mora affixation; Bye and Svenonius's (2012) "lexical insertion in two stages" approach to non-concatenative morphology generally (see also Zimmermann 2013); and Bermúdez-Otero's (2013) stratal OT approach which includes stem storage over readjustment. We hope that this line of inquiry will continue to be productive in future research within Realization Theories of morphology.

[23] We would like to take a second to recognize that, of all the models of insertion in RT presented here, Siddiqi's (2009) default fusion proposal is the only one that provides an account for the blocking of regular inflection inside nominal compounds but the permission of irregular inflection in the same position. Other MIO models, to our knowledge, have no viable account, nor do we see one available in any of the INT models discussed here, including the two we propose herein. This is because, in Siddiqi (2009), the fusion of the stem with the nominalizer and the inflection has the effect of percolating the nominalizer to the top of the dependent nominal (see Siddiqi 2009 for details). In all of the other models discussed here, the nominalizer remains embedded. However, Siddiqi (2009) requires a strict ordering that may not be otherwise motivated: fusion must occur before the conditions for grammaticality of compounding are checked, not after. Without this ordering and the already questionable default fusion of Siddiqi (2009), the permission of irregular inflection in nominal compounds in English appears to be data that requires an explanation in RT.

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