

Presyntactic morphology or postsyntactic morphology and explanatoriness in the Basque auxiliary

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Abstract: We respond here to Kiparsky’s ‘Thoughts on Arregi & Nevins 2012’ (Kiparsky 2013), an Optimality-Theoretic Lexicalist Morphology (LM) reanalysis of the Basque auxiliary systems that were characterized within a Distributed Morphology (DM) analysis in our book ‘Morphotactics’. The two arguably most far-reaching goals/arguments of Kiparsky’s paper are (i) to show that there are no opaque interactions in Basque (contrary to our claims in the book) (ii) that the Morphotactics version of DM fails to characterize wordhood. We respond to both of these claims. We observe, more generally, that a common argument against DM is that it is redundant to have a syntax and a postsyntax which partially overlap but are distinct modules. However, when one holds the LM analysis up to the light, it necessarily contains a syntax and a presyntax. As such, progress in the field will arguably come not from a comparison of which framework has ‘fewer’, partially overlapping modules—as both in fact have two—but which specific set of generalizations and proposals lead to a better understanding of the nature and limits of the constraints and repairs that characterize these closely related dialects.

1. Overview

Kiparsky’s remarks on our work (‘Thoughts on Arregi & Nevins 2012’) (Kiparsky 2013) constitute a thought-provoking and inventive treatment, and his paper is to be commended for the care and diligence with which it presents an Optimality-Theoretic reanalysis of the Basque auxiliary systems that were characterized within a Distributed Morphology (DM) analysis in our book *Morphotactics* (Arregi and Nevins 2012).¹

The two arguably most far-reaching goals/arguments of Kiparsky’s paper are (i) to show that there are no opaque interactions in Basque (contrary to our claims in the book) (ii) that the Morphotactics version of DM fails to characterize wordhood. Subsidiary goals include claims about redundancy and failure to capture generalization in DM vs LM (an unexpanded acronym used in the paper which we assume means ‘Lexicalist Morphology’), which should become clearer in our discussion of Kiparsky’s specific LM analysis below.

We will focus on the questions above, although we note that his paper contains a few rhetorical remarks about processualism, ultimately tangential to the analytic questions in (i) and (ii) above, which lose their force alongside remarks like that on p. 5: “I actually prefer a setup where morphemes are combined incrementally and each combination is immediately assigned a phonological, semantic, and argument-structural interpretation, but adopting it here would just introduce an unnecessary distraction.” Similarly, in the present review, we choose not to respond to framework-comparing claims of the following nature (p. 5): “And

*We thank Ricardo Bermúdez-Otero and Gereon Müller for very rewarding discussions of serial vs. parallel models of morphosyntactic realization in the current context, and Milan Rezac for very useful comments on DM and Basque morphosyntax.

¹Unless otherwise noted, all references to page, example, and section numbers are to Kiparsky’s paper.

[the LM constraints] do more work than [the constraint in Morphotactics] because they generate the correct outputs all by themselves without movement processes to implement them”, which belie an unpacking of “generate all by themselves”, as there is no specification of the workings of GEN.

To a similar extent, we will not devote much space here to respond to claims (such as Section 10 of his paper) that once Participant Dissimilation (see our version of it in *Morphotactics*, Ch.4) is made into an Optimality Theory (OT) constraint, the specific rules that generate dialect-specific repairs can be erased. This is only true if each dialect happens to contain exactly the right inventory of repaired forms, and the LM theory, like all list-based theories without principles of generation, fails to characterize recurrent, possible, shared, and impossible inventories of repaired forms across the dialects. For instance, the analysis of Participant Dissimilation in Ondarru in Section 7 is too general, as it rules out all combinations of 1Pl and 2nd person, but, as can be seen in Table (1) in the paper, not all of them in fact are (e.g. *saittuau* for 1Pl ergative with 2nd absolutive). Furthermore, the particular list of forms affected by this phenomenon, as well as the repair, varies from dialect to dialect. It is hard to compare *Morphotactics*, which includes an account of this variation, with the LM analysis as presented in Kiparsky’s paper, which does not.

Hereafter we put aside such well-known rhetorical standbys that run through discussions of derivational vs. generate-and-filter approaches, and focus exclusively on the details of the LM analysis as presented, before finally coming back to (i) and (ii).

2. The LM analysis

We now discuss the properties of the specific LM analysis presented in Kiparsky’s paper. In the LM analysis, agreement affixes are generated on the relevant auxiliary forms, and compete as wholesale lexical items based a parallelist model that compares their violation profile for markedness (e.g. NONINITIALITY, *1PL/2) and faithfulness (e.g. IDENT-F, MAX-2P) constraints. Naturally, the action happens when the former outrank the latter; in this way, very much paralleling Grimshaw’s (1997) analysis of Spanish clitic clusters in OT, ‘the best’ auxiliary form often happens to be ‘the wrong’ auxiliary form.

With respect to morpheme order, in addition to violable constraints (e.g. NONINITIALITY, ENCLISIS) the LM analysis also contains a number of *inviolable* constraints determining that certain morphemes are prefixes or suffixes ((3) on p. 4). In the DM analysis, this inviolability comes from a specific division of labor: merge and syntactic movement provide the bulk of the morpheme-ordering, while only very limited, morphotactically-motivated *operations* can reorder *specific morphemes*. To the extent that the LM analysis must make a division between inviolable and violable morpheme-order constraints, one might ask whether the LM architecture itself is missing certain generalizations about what could be a possible LM grammar with certain morpheme-order constraints violable in a grammar unlike the one provided for Basque.

One of the points we wish to highlight in this response is the fact that a combination of assertions in Kiparsky’s paper—that the tableaux represent the input to syntax (p. 6), and that hierarchical structure within words is not done within the syntax (p. 9)—*force a necessarily presyntactic word-formation component with a certain set of principles responsible*

for generating the hierarchical structure of words. We contend one of the ultimately more far-reaching questions raised by this paper is not whether morphotactic well-formedness should be implemented with purely constraint-based model versus a constraints-and-repairs model (Section 10), or whether opacity is the result of two interacting and serially-ordered modules (pp. 13–15), but rather whether, once one spells out all of the details of Gen and Eval that guarantee the right set of winning candidates that enter the syntax a presyntactic morphology module, would be in fact more explanatory than the combination of syntactic and postsyntactic operations we have explicitly set forth in *Morphotactics*.

Many of the constraints in Kiparsky’s paper (e.g. NONINITIALITY, *1PL/2) are OT versions of specific principles in *Morphotactics*; we are in fact quite pleased to see that the insights captured in our proposal can be straightforwardly maintained in a wholly different architecture. As such, we largely focus on the specific analytical differences.

2.1. Case Alignment and faithfulness

One constraint whose status in the LM analysis is not clear to us is CASEALIGNMENT. On the one hand, it seems to be doing the job of a faithfulness constraint, since candidates that don’t violate it have the expected internal structure depicted in (7). Verbs with Ergative Metathesis deviate from this expected structure in response to the higher ranked NONINITIALITY markedness constraint. The interaction between these two constraints is similar to the typical interaction between faithfulness and markedness in OT.

On the other hand, CASEALIGNMENT doesn’t appear to be defined as a faithfulness constraint, as it doesn’t require identity between input and output forms. Rather, it imposes a certain isomorphism between the internal hierarchical structure of the output and a thematic hierarchy. As far as we can tell from the text and the tableaux in Kiparsky’s paper, the input contains neither internal hierarchical structure nor thematic information, which makes it irrelevant for CASEALIGNMENT (i.e. only properties of output candidates are relevant).

This raises a more general question about the inputs in the LM analysis: What is the source of these inputs? In OT, an input is either a lexical item (or concatenated lexical items), or (especially in Stratal OT; see Bermúdez-Otero 1999, Kiparsky 2000) the output of some other module. However, it is not clear that the inputs used in the tableaux in the LM analysis fall under either category. They’re not lexical items, since the true lexical items seem to be the affixes in (2), other affixes (e.g. present tense *d-*), and auxiliary heads (e.g. *-ittu-*) (for instance, Footnote 13 on p. 12 refers to the absence of 3rd person absolutive/ergative affixes as a fact about the lexical inventory). That is, lexical items are pairings of phonological and morphosyntactic features in the LM analysis, but the inputs in the tableaux do not contain phonological features (or at least, they are not represented as containing them), so they’re not lexical items. This leaves the option that these inputs are outputs of some other module. The only other module discussed in Kiparsky’s paper is syntax, but p. 6 makes the claim that it’s the *output* of the tableaux that is used by the syntax, so the syntax couldn’t possibly be providing the inputs. This makes the status of the input in the LM analysis rather unclear.

2.2. Ergative Metathesis

The account of Ergative Metathesis presented in Kiparsky’s Sections 5–6 has several analytical problems. The analysis is based on three hypotheses: (i) the interaction of two constraints (NONINITIALITY>ENCLISIS), (ii) absolutive/ergative morphemes are unspecified for case, and (iii) *d-* encodes present tense. In the past tense, NONINITIALITY forces proclisis of the ergative, which otherwise is placed after T due to ENCLISIS. In the present tense, the tense specification of *d-* allows satisfaction of NONINITIALITY without the need for proclisis, and the ergative clitic can thus satisfy ENCLISIS in post-T position. Hypothesis (ii) is crucial, as it allows case-neutral prefixes/proclitics to realize either absolutive or ergative morphemes. This account seems to derive generalized enclisis of both ergatives and absolutives in Basque auxiliaries, a wrong result.

Consider first past tense monotransitive auxiliaries with a 1st or 2nd person absolutive. For instance, the past auxiliary form with a 1Sg absolutive and a 2Sg ergative, whose realization is *n-inddu-su-n* (Table (15)), has the following structure in the LM analysis, with an absolutive proclitic and an ergative enclitic (Kiparsky’s paper does not provide an explicit analysis for past tense *-n*, which we omit here):



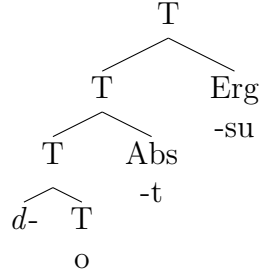
However, the candidate **su-inddu-a-n* (or **su-inddu-t-n*; both would surface without the leftmost *-u-*) with the following structure, is just as optimal:



Both satisfy NONINITIALITY (albeit with a different clitic; note that the formulation of this constraint on p. 5 is formulated in terms of distance from T to the left edge of the word, so it doesn’t have to be T’s sister that shields it from first position), to the detriment of a single ENCLISIS violation, and both satisfy CASEALIGNMENT in an identical way (importantly, this constraint is formulated in terms of c-command, not linear order).

A similar preference for enclisis also rears its head in present tense monotransitives with a 1st/2nd person absolutive. Consider the form *n-a-su* in Tableau (9): a candidate that is missing there is **d-o-t-su* (or *d-a-t-su*) with the following structure:

(C)



This is a better candidate than *n-a-su* (9b), since it satisfies all the constraints (including CASEALIGNMENT). In addition, in a way similar to the past tense example above, **s-a-t*, with an absolutive enclitic and a higher ergative proclitic, is just as good a candidate as *n-a-su* (although, as shown above, **d-o-t-su* is better than both).²

Two specific claims in the LM analysis lead to these wrong results. First, the present-tense property of *d-* overrides any proclisis in the present tense. Second, no constraint or lexical property ensures that absolutives and ergatives have different properties in terms of placement with respect to T. In fact, these assumptions jointly overlook the fact that, whenever present, absolutives are always in proclitic position.³ Avoiding the generation of a right-branching tree as in (B) seems to essentially require a presyntactic module with very precise constraints on what it can generate, but as shown above, the LM analysis of this presyntactic module falls short of this. Note that Kiparsky explicitly says that the hierarchical structure within words should not be accomplished syntactically (p. 9): “This entire apparatus [in *Morphotactics*] appears to be an artifact of having the pieces of the auxiliary assembled by syntactic movement. The lexical analysis has no movement, hence no puzzle about what moves where when and why. Instead, [CASEALIGNMENT] provides a cross-linguistically motivated unifying principle for the constituency of the agreement morphemes, which together with their morphological orientation (prefix, suffix, or unspecified) generates their order in the auxiliary.”

The assumption that the present tense specification of *d-* is what prevents Ergative Metathesis raises other serious questions. Kiparsky adopts Donohue’s (2004) claim that it is a marker of tense, based on Trask (1977, 1997). However, Donohue’s (and Trask’s) proposal is not specific to *d-*. In particular, she also claims that *z-* (*s-* in Biscayan; see below) is a past tense marker (Donohue 2004:25, 31–32). Outside Biscayan, past tense *z-* has a similar distribution as *d-*: in auxiliaries without Ergative Metathesis, it surfaces in forms agreeing with a 3rd person absolutive. That is, the past tense has a prefix *z-* that seems to perform the same function as *d-* in the present, but this doesn’t block Ergative Metathesis in the

²Ricardo Bermúdez-Otero (personal communication) has suggested that these issues might be avoided under a serially-optimizing model, such as a Harmonic Serialism version of Kiparsky’s LM that performs optimization morpheme-by-morpheme.

³Milan Rezac (personal communication) has brought to our attention forms in certain dialects in which canonically absolutive arguments are crossreferenced in enclitic position. They seem to occur in very specific environments, including PCC-like contexts (with a 1st or 2nd person theme and a dative argument) and non-indicative moods. The mechanisms underlying these forms are not very well-understood, and the phenomenon is seriously underdescribed. For instance, in the non-indicative forms, we do not know whether the theme argument crossreferenced by enclisis has absolutive or ergative case, which would provide crucial clues as to their syntax. See Rezac 2013 and Hualde et al. 1994:119–120 for description of the known facts and relevant discussion.

past tense in these dialects. As in Biscayan, Ergative Metathesis in these dialects occurs in auxiliaries that (i) agree with a 3rd person absolutive, and (ii) also contain an ergative (see, for instance, the description of Ergative Metathesis in Batua in Donohue 2004:34–38). Under the analysis in Kiparsky’s paper, it is not clear why *z-* does not block Ergative Metathesis in all forms in the past tense.

Due to a general (historical) phonological process of place neutralization, the Biscayan counterpart of *z-* is *s-*. Although *s-* in Biscayan has a more limited distribution than its counterpart in other dialects, it is present in a few forms. See for instance the 3Pl absolutive forms *s-ittu-n* and *s-itxu-e(-se)-n* in (15) (the corresponding forms in *Morphotactics* have \emptyset ; this sort of idiolectal variation is common). Thus, the problem also arises in the Biscayan variety analyzed in Kiparsky’s paper. We think part of the problem here is that the distribution of *d-* and similar prefixes in Basque finite verbs is much more complex than is made out to be in Kiparsky’s paper. This can also be seen in the treatment of dummy *g-* in the paper (Footnote 7 on p. 4, and example (25) on p. 14). This prefix is used in all applicative intransitives in the indicative, including the past tense (de Yrizar 1992:vol. 1, 221; note that this work is cited in Kiparsky’s paper, but it does not appear in the list of references). Although this is not stated explicitly, the comments in Footnote 7 and the glosses in example (25) suggest that it is treated as a present tense prefix in the analysis, just as *d-* is. Its appearance in past tense forms is therefore unaccounted for.

We’d like to emphasize that the main issue is not the assumption that *d-* marks present tense, but the claim that this is what blocks Ergative Metathesis in the present tense. The specific wrong predictions that this makes are that (i) *d-* blocks proclisis—even of absolutives—in all present tense forms, and (ii) a similar prefix also blocks Ergative Metathesis in the past tense.

2.3. The PCC and Absolutive Promotion

The analysis of the PCC and Absolutive Promotion is presented in such a way that it is very hard for us to evaluate it, let alone compare it to the one in *Morphotactics* (Chapter 2). First, Absolutive Promotion is a repair to the PCC in intransitive sentences. However, Kiparsky’s paper never makes an explicit connection between the two. In particular, forms with Absolutive Promotion are discussed in Sections 6 and 7 (in the latter, Absolutive Promotion is not mentioned, even though the auxiliary in (27c) analyzed in (29) involves this process), but not in relation to the PCC, which is only discussed later on in Section 8. Even for the reader familiar with *Morphotactics*, this organization of the discussion makes it very confusing.

The PCC analysis in Section 8 is not specific enough as it stands either. It is based on the constraint in (32), but it is not clear to us whether this is a constraint on word-forms, that is, a constraint on the same module/level of representation as the other constraints discussed in the paper. It does not appear in any of the tableaux of auxiliaries that could potentially violate it (i.e. (24) and (29)), which makes us wonder whether this is the case. It might, for instance, be a constraint on feature combinations within words in the syntax. Without any explicit claim to this effect, we can’t evaluate the validity of (32).

Kiparsky’s paper stresses the claim that (32) is “a general constraint on argument structure or Th-role combinations, not the distribution of case” (p. 17). We therefore don’t

understand the claim that the extension of the PCC to all datives in some dialects is “not a problem for the lexical account, at least descriptively, since the case/agreement morphemes agree in case with the arguments they license.” (p. 18) If the PCC has nothing to do with case, how is this case-based extension at all possible?⁴

Another question that the analysis of the PCC gives rise to has to do with the relation between this constraint and Absolutive Promotion: how exactly does the analysis derive that Absolutive Promotion applies only in auxiliaries that would potentially violate the PCC? This is not explained in the paper. In addition, the explicit analyses of forms with Absolutive Promotion in (24) and (29) are not consistent with each other: the Theme is encoded as ergative in the input in (24), but as absolutive in (29). This only adds to the reader’s confusion about this part of the account.

The fact that Absolutive Promotion is a PCC-repair also speaks to the claim in the paper that the PCC is about Th-roles, not case. In auxiliaries with Absolutive Promotion, an ergative theme is combined with a dative recipient/experiencer, but this violates (32). The reason the PCC is made dependent on case in *Morphotactics* is precisely that one of its repairs enacts a change in case, not Th-roles.

The discussion of PCC and Absolutive Promotion raises other minor issues. First, the tableau in (29) makes it quite clear that, as shown above, the analysis wrongly derives generalized enclisis of absolutives. Given an input identical to (29) except for the absence of a dative clitic, the result is **d-o-su*, but the actual form is *s-a(s)* (de Yrizar 1992:vol. 1, 221). Second, the discussion under (24) of the *Morphotactics* analysis of forms with both Absolutive Promotion and Ergative Metathesis is based on a mistaken description of that analysis. According to Kiparsky, we “propose that [-tz] occurs in auxiliaries that contain a clitic in C” (p. 14). The actual proposal in *Morphotactics* is that -tz is the allomorph of this clitic in the context of T specified as [+have] (pp. 123, 125; the relation between this and Ergative Metathesis and Absolutive Promotion is discussed on p. 352). As is generally the case in DM, this allomorphy is determined at Vocabulary Insertion, and thus occurs after Ergative Metathesis. Thus, Kiparsky’s claim that this allomorphy is determined before Ergative Metathesis in *Morphotactics* is mistaken. For further comparison between the DM and LM analyses of these phenomena, see Section 3 on opacity below.

2.4. The relation between T and absolutive and dative arguments

The characterization of possible forms of T in the present on p. 2 is oversimplified. First, the form appearing after 1Pl and 2nd person absolutive/nominative markers is -*aittu*-, not -*ittu*-. Second, 1Sg absolutives are not always followed by -*a*- (see *n-au*, *n-ab-e* in Table (1)). Third, the claim that “when there is no nominative it appears in a default form” hides

⁴The rhetoric under (32) makes it appear that this extension is marginal: “Sentences like “I approached the boat (Dat.)” are reportedly OK in Basque ... though not for A&N’s Ondarru consultant. ... some speakers or dialects apparently extend PCC effects to all datives”. This extension is in fact well documented in many dialects. See Rezac 2009 (cited on p. 75 in *Morphotactics*) for a fairly complete description of the known dialectal variation in this respect. The total absence of absolutive/dative forms with a 1st or 2nd person dative from the description of all Biscayan varieties in de Yrizar 1992 is also telling: he justifies not including them because their use is either very limited or nonexistent in most varieties (vol. 1, p. 1), which we take to mean that it is so under any interpretation of the dative (directional/locative or recipient/experiencer).

the fact that in the 3Sg and 3Pl columns in Table (1), T can take one of three forms: *-o-*, *-au/ab-*, or *-oittu-*. It's not clear what it means for all three exponents to be default, and Kiparsky's paper is silent on what determines their distribution.

More generally, the paper does not contain an explicit analysis of the sensitivity of the shape of T to the features of the absolutive argument. Is it agreement, as in *Morphotactics*, or contextual allomorphy? The only explicit claim made in the paper is that the form of T is “determined” by the absolutive argument (p. 2). The choice of analysis might have consequences for the account of dialects with auxiliary forms in which proclitics and agreement crossreference dative arguments. Footnote 10 claims that on the LM account, these forms are “predicted” and that the DM account contains an artifact in claiming that dative arguments trigger agreement. In fact, the argument in *Morphotactics* comes from the doubled pronominal argument (e.g. *gu-ri* on p. 83), which retains dative case. Descriptively speaking, the shapes of both the proclitic and T are affected by a dative argument. The LM account in Kiparsky's paper includes no way to deal with case mismatches between the case on arguments and the case on corresponding clitics/agreement; once implemented, it will contain an OT equivalent of the Dative Impoverishment rule posited in *Morphotactics* (p. 87), and a weakening of Kiparsky's claim on p. 18 that “for the lexical account, at least descriptively ... the case/agreement morphemes agree in case with the arguments they license.”

2.5. The bimorphemic analysis of dative markers

The LM account adopts a bimorphemic analysis of datives (p. 3), which is ultimately relevant for the way that specific constraint evaluation works. Footnote 3 addresses some of the objections to this analysis in *Morphotactics* (pp. 130–131), but does not address all of them. First, in intransitive auxiliaries in Lekeitio and Zamudio, the 1Sg dative clitic is *-t/-ra*, not *-st*, and the 1Pl dative clitic is *-ku*, not *-sku* (de Yrizar 1992:vol. 1, 97, 591). These do not have the initial fricative the bimorphemic analysis relies on. Second, Kiparsky claims that the underlying form of the first plural enclitic is *-gu* in all dialects, including Zamudio, which justifies his claim that dative *-sku* also derives from */-tz-gu/* in this dialect (Footnote 3). As an ergative marker, it surfaces as *-u* due to phonological lenition. Although there is ample evidence for the lenition process mentioned in this footnote,⁵ this cannot be the

⁵Kiparsky describes this phonological lenition process as involving *g- → y- → ∅*, which, furthermore, “applies fairly generally in Zamudio”. Lenition of voiced stops is quite general across Basque dialects, not just Zamudio. The intermediate step, if any, is [ɣ] (a velar fricative or approximant), not *y* (which is the orthographic representation of a palatal fricative or approximant). For relevant discussion of the phonology of voiced stops, see pp. 178–180 (including Footnote 90) in *Morphotactics* and references cited there.

The paper contains other minor misunderstandings about Basque phonology. Footnote 1 contains two mistakes: “Palatalization of *-tt-* as in *saittut → sa(i)txut*, regular in A&N's data, is restricted to the Gaminde area in Yrizar.” Orthographic *tt* is a palatal stop, and orthographic *tx* is an alveopalatal affricate (these are standard orthographic conventions; see pp. xix and 15–16 in *Morphotactics*), so *tt → tx* can hardly be described as palatalization. The change is a generational one (younger speakers have replaced *tt* with *tx*) and quite general at least in Biscayan (see p. 16 in *Morphotactics*). Second, the reference to “the Gaminde area in Yrizar” is puzzling: Iñaki Gaminde's fieldwork is extensively referred to in de Yrizar 1992. All references to Gaminde on vol. 1, p. 222 (and elsewhere) in this work are to this linguist's field work, not to any particular “area”. Furthermore, the change *tz → s* in dative markers is claimed to involve Voicing Assimilation (p. 3). It does not: both *(t)z* and *(t)s* are voiceless. Orthographic *(t)z* is laminal, and *(t)s* is

correct analysis for 1Pl *-u* in Zamudio. The arguments that this clitic is *-u* in Zamudio, but *-gu* in Ondarru and Lekeitio, are on pp. 131 and 178–180 (including Footnote 92) in *Morphotactics*.

In the LM analysis, bimorphemic datives can't be used to satisfy NONINITIALITY because they incur too many violations of ENCLISIS (p. 14).⁶ However, what prevents proclisis of just one of the morphemes? In other words, what rules out candidates *tz-e-∅-t-an* and *∅-e-tz-t-an* in (24)? A related issue has to do with Kiparsky's analysis of the 3rd person dative allomorph *-ko* as a portmanteau (p. 3). This is an interesting proposal, including the mention of a syntagmatic constraint involving it (p. 15). If, as standardly assumed, a portmanetau is monomorphemic, *-ko*, unlike other datives, could potentially surface as a proclitic to satisfy NONINITIALITY, a wrong result (e.g. the auxiliary in (25) is *g-a-ko*, with post-T *-ko* and no present-tense-specific prefix to prevent proclisis).

3. Is there Opacity in the Basque Auxiliary?

Several of the rule interactions discussed in Chapter 6 in *Morphotactics* involve transparent (not opaque) orders. As pointed out in Kiparsky's paper, this is in principle amenable to an OT-based account, although, in the absence of explicit accounts of these interactions in the LM analysis, this is hardly more than a tentative statement. One of the main cases for opacity made in *Morphotactics* has to do with the interaction between Absolutive Promotion and Ergative Metathesis, which Kiparsky claims is not in fact opaque (p. 21), based on the LM account of the phenomenon offered in Section 6. To this, we'd also like to add the interaction between Absolutive Promotion and Participant Dissimilation (*Morphotactics*, p. 346–349). The relevant surface forms are opaque, since they involve displacement of a Theme clitic to ergative position (Absolutive Promotion), followed by deletion of the dative clitic (Participant Dissimilation) triggering the displacement. The LM analysis also offers an alternative account of this phenomenon in Section 7. Since both phenomena involve Absolutive Promotion and the PCC, whose LM-based account, as argued above, is not explicit enough, we take it that our claim for opacity in these forms has not been countered. Thus, Kiparsky's claim that DM's theoretical power is excessive (p. 21) is not warranted.

In fact, the analysis of these phenomena in Kiparsky's paper and the one in *Morphotactics* (pp. 346–352) are strikingly similar. In the latter, the syntax determines that the Theme is an ergative clitic. This provides an input to Noninitiality-triggered Metathesis of this clitic in the postsyntactic component. In Kiparsky's analysis in (24), the Theme is similarly encoded as ergative in the input, and NONINITIALITY enforces proclisis. This entails that this input is the output of some other component that forces promotion of the Theme to ergative position. Although it is not clear to us what this other component may be, the two analyses make the identical claim that the output form is the result of the interaction of two separate modules: one yields Absolutive Promotion as output, which is taken as input to a

apical. The place neutralization involved in this phonological change is part of a more general one discussed on p. 15 in *Morphotactics*.

⁶The LM analysis in fact also derives that datives can't satisfy NONINITIALITY due to the undominated ordering constraint in (3a). This seems redundant. As noted on p. 10, the obligatory post-T position of datives follows directly from (6) and their bimorphemic structure, so including datives in (3a) is unnecessary.

separate module that enacts Ergative Metathesis. This derives opacity as a result of serial interaction of modules, which is one of the main claims made in *Morphotactics* and resonates with similar claims by Kiparsky in other works. Although the similarity between the two analyses is extremely interesting, other parts of the paper make us doubt whether this is the correct interpretation of the claims made in the LM analysis. In particular, the analysis in (29), which involves Absolutive Promotion, is based on an input where the Theme is encoded as absolutive. In this specific part of the paper, it seems that Absolutive Promotion is handled in the same component as Participant Dissimilation and Ergative Metathesis, which contradicts the assumptions behind the LM account of (24). To conclude, the claim that Basque auxiliary morphology does not display opacity effects awaits a more explicit account of the PCC and Absolutive Promotion in the LM framework.

4. Does DM fail to characterize wordhood?

Kiparsky’s main argument for this point comes from evidential particles, which do not contribute to the second position requirement. *Morphotactics* used a distinction between the $X^{0\max}$ and the M-Word. LM expresses the latter in terms of postlexical cliticization, a move with a straightforward equivalent in DM, whereby phrase-level cliticization of this sort, outside the $X^{0\max}$, occurs after Vocabulary Insertion of both relevant pieces. The relevant argument, therefore, is that $X^{0\max}$ generates certain constituents, such as [do] in [dosu], that are not ‘words in any useful sense’. This remark seems akin to claiming that [and Bill] in [John [and Bill]] is a ‘bogus constituent’ because it cannot move in isolation; syntactic structures always contain items that correspond to constituents that may not show independence. Just like not everything in syntax follows from certain sequences of words but not others being a phrase (XP), not everything in morphology needs to follow from some sequence of morphemes but not others being a word. This is in fact apparent in the LM analysis, since the word internal structure of Basque auxiliaries in (7) contains sub-word constituents that the account makes crucial use of (via CASEALIGNMENT).

Moreover, it’s not completely clear to us whether the LM analysis of Basque verbs can completely dispense with word-like units below the lexicalist level of word. Unlike *Morphotactics*, the LM account defines NONINITIALITY in terms of words: T in a finite verb cannot be the leftmost morpheme within the word (p. 5). In Section 9, this is taken to be a virtue of the LM analysis, since the word domain in a lexicalist framework comes ‘for free’. However, as shown above, the LM analysis of Ergative Metathesis based on this constraint runs into several problems. In particular, it wrongly predicts the possibility of forms where a higher ergative morpheme precedes T and a lower absolutive morpheme follows T (see the tree for **su-inddu-a-n* in Section 2.2 above). Because NONINITIALITY doesn’t care about the hierarchical position of the initial morpheme within the word, ergative *su* satisfies it, even though, due to CASEALIGNMENT (which does care about hierarchical relations), it’s in a high position within the word. One could easily imagine a simple fix to NONINITIALITY whereby it would require T to be noninitial *within its minimal projection*, i.e. it could only be satisfied if the sister of terminal T were to its left. As desired, this constraint, in conjunction with CASEALIGNMENT, would rule out any verb with a prefixal ergative and a suffixal absolutive. However, it would also considerably weaken the argument for lexicalism

in Section 9, since NONINITIALITY effects would no longer be based on word domains.

The specific argument for $T^{0\max}$ in *Morphotactics* is the one based on Root Reduplication (pp. 326–332). In the resulting word (*d-o-sta-s-endu-n*), the rightmost copy of T (*endu*) is 4 morphemes away from the left edge of the M-word, but it’s in second position within $T^{0\max}$. Kiparsky asserts that $T^{0\max}$ is the domain for impoverishment and morpheme ordering in *Morphotactics*. It’s not: the domain for these is M-Word, as demonstrated by the interaction between ergative clitics (adjoined to C) and items inside $T^{0\max}$ in Participant Dissimilation in Chapter 4 (e.g. 1Pl absolutive deletes in the context of 2nd person ergative) and Ergative Metathesis in Chapter 5 (ergative clitic becomes sister of T).

The criticism of constituency is based on alleged problems with word-level phonological processes (p. 20). We know of no argument (phonological or otherwise) that the constituency of *badosu* is [ba[dosu]] and not [[bado]su], as we have it in *Morphotactics*. Arguably, however, we did not include enough discussion of stem-level vs. word-level phonological processes in our Section 3.6 on phonological processes, a criticism which we accept.

Our main reason for sandwiching Mod between T and C in *Morphotactics* (pp.333–339), was to generate them where we see these morphemes cross-linguistically (according to Cinque 1999). The LM analysis, on the other hand, presents no analysis of Mod, other than to say that they are proclitics on the finite verb. Where are they generated? Related to this, the LM analysis contains no proposals about plural *-s* or past tense *-n*, both of which are in C in *Morphotactics* (pp. 88–95, 103–108). With no commitment to these crucial elements, the LM/DM comparison about the structure of words is incomplete.

5. Conclusion

Kiparsky’s paper has allowed us to understand better many aspects of our own analysis, as formalization in different frameworks allows one to see what is crucial in an analysis and what is substitutable by other devices. It seems that his assertion that Ergative Metathesis is blocked in the present tense due to the feature specification of *d-* is untenable without massive, potentially even more stipulative changes to both analyses. Perhaps relatedly, his statement of the PCC in which “it is a general constraint on argument structure or on Th-role combinations, not on the distribution of case” (p. 17) seems overly simplistic in light of Rezac’s (2011) discussion of French locative *y* as a replacement for the dative argument as a repair strategy. In short, the LM analysis, as promising as it may be, will eventually need to address the applicability of PCC repairs to languages beyond Basque, in which the details of the formulation of the PCC matter as much for the constraint as for its inventory of (im)possible repairs. The LM analysis in its present form is also somewhat inscrutable as to case mismatches (such as those involved in Absolutive Promotion and Dative Impoverishment): it is an output-based theory, but seems to restructure the input in ways that still mismatch the case on the corresponding arguments.

There are many criticisms levelled against DM—not all of them are contained within the specific paper of Kiparsky’s we are responding to here. One argument against DM is that it is redundant to have a syntax and a postsyntax which partially overlap but are distinct modules. However, when one holds the LM analysis up to the light, it necessarily contains a syntax and a presyntax, the latter of which still contain some unclarities in to how the

input *to* the presyntactic tableaux anticipates and constrains the set of inputs, as mentioned for Absolutive Promotion in particular. As such, progress will arguably come not from a comparison of which framework has fewer, partially overlapping modules—as both in fact do (and indeed, classic work in Kiparsky’s own classical Lexical Phonology architecture is predicated on the idea of separate strata, each with overlapping but distinct principles and constraints) —but rather which specific set of generalizations and proposals lead to a better understanding of the nature and limits of the constraints and repairs that characterize these closely related dialects.

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