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THE MORPHOSYNTAX- PHONOLOGY CONNECTION

Locality and Directionality at the Interface

Edited by

Vera Griбанова and Stephanie S. Shih

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PRESYNTACTIC MORPHOLOGY OR POSTSYNTACTIC MORPHOLOGY AND EXPLANATORINESS IN THE BASQUE AUXILIARY

Karlos Arregi and Andrew Nevins

14.1 Overview

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

The two arguably most far-reaching goals of Kiparsky's chapter are to argue (i) that there are no opaque interactions in Basque (contrary to our claims in the book) and (ii) that the *Morphotactics* version of DM fails to properly characterize wordhood. Subsidiary goals include claims about redundancy and failure to capture generalizations in DM versus LM (Lexicalist Morphology), which should become clearer in our discussion of Kiparsky's specific LM analysis in this chapter.

We provide a counterpoint in this chapter specifically exploring these and other juxtapositions of the two frameworks vis-à-vis the morphological structure of the Basque auxiliary. As such, we dwell less on specific rhetorical remarks about processualism, some of which are tangential to the analytic questions in (i) and (ii) just mentioned, and that lose their force alongside remarks like that on p. 361: "a strictly constraint-based approach need *not* embrace parallelism and global constraint evaluation; it is compatible with a cyclic approach where words are built incrementally and interpreted phonologically and semantically at each

We thank Ricardo Bermúdez-Otero and Gereon Müller for very rewarding discussions of serial versus parallel models of morphosyntactic realization in this context, and Milan Rezac for very helpful comments on DM and Basque morphosyntax.

morphological step.” Similarly, in this chapter, we choose to focus less space directly on framework-comparing claims of the following nature (p. 370): “[T]he constraints do more work in OT than in DM because they generate the correct outputs directly and do not require movement processes to implement them”, which belie an unpacking of “generate the correct outputs directly,” as there is no specification of the workings of GEN. Instead, we focus on specific questions that arise with respect to characterizing the phenomena found across these auxiliary systems, and how comparisons like those raised in Kiparsky’s chapter sharpen questions of how to best implement them in ways that can provide explanations with a theoretical shelf-life and that can ideally generalize, without “brittleness,” across dialectal varieties.

Certain aspects of the alternative analyses that Kiparsky develops will receive less attention here than others. For example, we will not devote much space to respond to the claim that once Participant Dissimilation (as well as other constraints proposed in *Morphotactics*) is made into an Optimality Theory (OT) constraint, the specific rules that generate dialect-specific repairs can be erased (section 13.12 in Kiparsky’s chapter). At times, these parsimony-based comparisons between the frameworks are difficult to evaluate, as the DM analysis of Participant Dissimilation (in Chapter 4 of *Morphotactics*, and also in Arregi & Nevins 2007) accounts for a great deal of dialectal variation in the phenomenon in terms of small modifications to the Participant Dissimilation constraint as well as differences in the deletion rules triggered by the constraint. Kiparsky’s own analysis of this variation in Section 13.9 (which is necessarily sketchy) involves dialectal differences both in ranking of constraints such as MAX-2PERSON, and in constraint formulation (or perhaps reranking of two very similar versions of the same constraint). Once all the analytical details of the account of variation are spelled out, it is far from clear that one framework offers a simpler account of the facts than the other.¹

To a large extent, in what follows, we put aside some of the well-known rhetorical, broad-brush standbys about how “constraints do the work for free” that often recur in discussions of derivational vs. generate-and-filter approaches, and devote most of the focus exclusively to the specific details of the LM analysis as presented, and then turn to (i) and (ii) mentioned previously.

1. Kiparsky (p. 388) also suggests that Participant Dissimilation could be subsumed under a more general theory prohibiting misalignment between person prominence (first over second) and case prominence (ergative over absolutive and dative), but he notes that the Zamudio dialect “does not work this way, so at least that dialect requires” a constraint not grounded on misalignment of hierarchies. Zamudio is far from being an exception in this respect, and we argue against this type of account in Arregi & Nevins 2007. A particularly illuminating dialect is Bermeo, in which, according to Laka et al. 2008 (cited in Arregi & Nevins 2012:222), *all* combinations of first plural with second person clitics are banned (and repaired by deleting the first plural clitic), regardless of their relative positions in the hierarchies, rendering a hierarchy-alignment account otiose.

14.2 The LM analysis

In this section, we discuss the properties of the specific LM analysis presented in Kiparsky’s chapter and ways in which they differ from our own proposals. In the LM analysis, agreement affixes are generated on the relevant auxiliary forms, and compete as wholesale lexical items based on a parallelist model that compares their violation profile for markedness (e.g. NONINITIALITY, PARTICIPANT DISSIMILATION) and faithfulness (e.g. IDENT-F, MAXTENSE) 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 (see discussion following example (3) on p. 366 in Kiparsky’s chapter). In the DM analysis, this inviolability comes from a specific division of labor: merge and syntactic movement provide the bulk of the morpheme-ordering, whereas 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 instead having been violable in a grammar unlike the one provided for Basque.

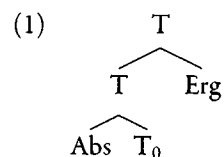
One of the points we wish to highlight in this response is the fact that a combination of assertions in Kiparsky’s chapter—that the tableaux represent the input to syntax (p. 369), and that hierarchical structure within words is not done within the syntax (pp. 367–368)—*force a necessarily presyntactic 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 chapter is not whether morphotactic well-formedness should be implemented with a purely constraint-based model versus a constraints-and-repairs model (section 13.12 in Kiparsky’s chapter), or whether opacity is the result of two interacting and serially-ordered modules (p. 392), 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 chapter (e.g. NONINITIALITY, PARTICIPANT DISSIMILATION) 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.

14.2.1 Case alignment, GEN, and faithfulness

One of the central constraints in the LM analysis is CASEALIGNMENT, discussed in detail in section 13.3 in Kiparsky's paper. In combination with constraints that ensure that T is in second position (ENCLISIS and NONINITIALITY), as well as inviolable order constraints on some morphemes (which are individually specified as prefixes and suffixes), it derives that absolutive markers surface before T, whereas dative and ergative markers (except in cases of Ergative Metathesis) are suffixal. We discuss here certain issues with the evaluation of the output with respect to this constraint, as we believe they can illuminate the nature of output candidates and their relation to the input (i.e. GEN) in LM.

In the specific case of monotransitive auxiliaries, CASEALIGNMENT favors structures such as (1), Kiparsky's (11a), in which the absolutive marker is structurally lower than the ergative marker.²



This is, for instance, the structure assigned to the auxiliary *n-a-su* in (2), Kiparsky's (9).³

- (2) Su-k ni-∅ ikus-i n-a-su.
 you.SG-ERG me-ABS see-PRF 1SG-PRS-2SG
 "You (Sg.Erg) have seen me (Abs)."

The effect of this constraint in this example (and in monotransitive auxiliaries more generally) can be illustrated by comparing the output candidates in (3), adapted from Kiparsky's example (8).

(3)

	NONINITIALITY	ENCLISIS	CASEALIGNMENT
Present + Abs.1Sg + Erg.2Sg			
☞ <i>n-a-su</i> 1Sg-T-2Sg		*	
* <i>s-a-t</i> 2Sg-T-1Sg		*	*

2. CASEALIGNMENT is a Mirror Principle-like constraint ultimately anchored in the Theta-hierarchy. See Kiparsky's Section 13.3 for details.

3. Although most examples in Kiparsky's chapter are from *Morphotactics*, we use here Kiparsky's own glossing conventions, which do not always coincide with our own. All examples in this chapter are from the dialect of Ondarru, which Kiparsky's chapter focuses on.

Grammatical *n-a-su* conforms to (1) and therefore wins over *s-a-t*, which, by reversing the hierarchical order of the absolutive and ergative markers, incurs a violation of CASEALIGNMENT.⁴

It is, however, not clear how candidate outputs with structures like (1) are built in the first place. Specifically, the analysis, as it stands, has no way of generating case labels (Abs, Erg) on agreement markers in these structures. The building blocks for these structures are the affixes in Kiparsky's (2); but, crucially for the LM analysis of Ergative Metathesis, these are unspecified for case. The input is specified for case (e.g. (3) above), but, in the Optimal Construction Morphology approach assumed by Kiparsky (p. 368), inputs are meaning targets that outputs are supposed to match (as evaluated by the constraints) and are thus not structures that GEN operates directly on to build candidate outputs. If neither inputs nor the affixes that candidate outputs are built on are the source of case labels on output structures, it seems that GEN is the only plausible source. Whether this is the right interpretation or not, it speaks against Kiparsky's claim that "the constraints do more work in OT than in DM because they generate the correct outputs directly. . . . The sparser machinery achieves not merely greater theoretical parsimony or elegance, but a more restrictive typology" (p. 370). In fact, it seems that GEN (or some other part of the LM analysis not developed yet) does play some role in generating even the simplest examples (e.g. *n-a-su* in (2)), which casts at least some doubt on the claim that LM analysis relies solely on the constraints and "sparser machinery" than DM.

A related aspect of the LM analysis that is not developed explicitly is the role of faithfulness constraints in the earlier parts of the paper. The constraints NONINITIALITY, ENCLISIS, and CASEALIGNMENT proposed by Kiparsky to account for simple examples such as (2) in this chapter are all markedness constraints (i.e. they evaluate outputs independently of how well they match the input). The analysis of more complex cases in later sections does include faithfulness constraints (e.g. MAX-2PERSON in section 13.9 in Kiparsky's chapter, and MAXTENSE, also introduced in section 13.9, but relevant for the LM analysis of *d*-Insertion in section 13.6). However, like any other OT analysis, faithfulness constraints are crucial even in the simplest cases, in order to ensure, for instance, that the auxiliary *n-a-su* in (2) with a second singular ergative marker is not the correct output for an input meaning target specified as Erg.3Pl

4. The string *s-a-t* can also be the form of a different candidate with the structure [_T Erg [_T T₀ Abs]] (Kiparsky's (11b)), in which the hierarchical order required by CASEALIGNMENT is preserved, but the order of the absolutive and ergative markers is reversed. This candidate loses to *n-a-su* because it violates NONINITIALITY with respect to the intermediate projection of T, in which T₀ is initial. See pp. 369–370 in Kiparsky's paper for details, as well as Section 14.4 in this chapter for interesting consequences that this way of evaluating NONINITIALITY has for Kiparsky's discussion of wordhood.

(cf. the analysis of *n-a-su* in (3)). Again, without an explicit description of crucial parts of the analysis, claims of theoretical parsimony or elegance are hard to evaluate.

14.2.2 Ergative Metathesis and *d*-Insertion

Whereas ergative marking is usually enclitic and absolutive marking proclitic, in auxiliaries with Ergative Metathesis, the proclitic position is filled by ergative instead of absolutive markers. Kiparsky’s analysis of Ergative Metathesis in section 13.5 (which finds a precedent in Albizu & Eguren’s 2000 OT account, discussed in Arregi & Nevins 2012:299–301) is based on NONINITIALITY and the underspecification of absolutive/ergative affixes in (2) in Kiparsky’s chapter with respect to case and linear order.

One of the most basic aspects of Ergative Metathesis that any analysis has to explain, and that has so far resisted a nonstipulative account (see Arregi & Nevins 2012:300–302 for relevant discussion), is the fact that it is blocked in the present tense, in which we typically find the exponent *d-* occupying the first position in the auxiliary (thereby satisfying NONINITIALITY). The proposed LM explanation (section 13.6 in Kiparsky’s chapter) is based on the idea that *d-* is specified as present tense (although it is not itself a T morpheme). Thus, a *d*-initial candidate satisfies both NONINITIALITY and MAXTENSE in the present tense, but a candidate with an auxiliary-initial ergative marker violates the latter constraint, under the assumption (implicit in Kiparsky’s chapter) that T itself is not specified for tense. This raises the question of why not *all* present tense auxiliaries have *d-*, because in fact, only present tense auxiliaries agreeing with third person absolutive arguments (or those used in sentences with no absolutive) have *d-*. In sentences with a participant absolutive argument (e.g. (2)), the pre-T position is dedicated to the absolutive marker, not *d-*. Kiparsky’s response to this potential objection is that *d-* is indeed a present tense marker, but that it also “selects a third person absolutive auxiliary” (p. 381).⁵

This analysis of *d-* as selecting a third person absolutive auxiliary faces some challenges having to do with its use in auxiliaries with no apparent matching third person absolutive argument, which Kiparsky addresses only partially. This includes unergative

5. Following Kiparsky’s comments on the role of selection in LM on his p. 368, we interpret the claim that *d-* selects a third person absolutive auxiliary as the requirement that *d-* be prefixed to a T specified as third person absolutive. However, Kiparsky’s own analysis of the exponence of T on pages 365 and 376 as dependent on the preceding morpheme seems incompatible with this. In particular, although the exponence of T depends on features of the absolutive argument (treated as a consequence of agreement between T and absolutives in *Morphotactics*), this sensitivity is dealt with in the LM analysis in terms of contextual allomorphy, that is, T itself is *not* specified for absolutive features. Thus, some component of the LM analysis of either T or *d-* must be wrong. On the relation between the exponence of T and absolutive arguments, see also section 14.2.4.

verbs with ergative subjects, analyzed by Kiparsky as covertly transitive (despite arguments to the contrary in Preminger 2012). However, present tense sentences headed by psych-verbs subject to the Person-Case Constraint (PCC) and Absolutive Promotion also have *d-*, as illustrated here by (4) and (5) (Kiparsky’s (32c) and (39c), respectively; see sections 14.2.3 and 14.3 for further comments on Kiparsky’s analysis of these phenomena).

- (4) Jon-ei gu-Ø/k, es d-o-tz-gu, (>dotzau)
 Jon-DAT we-ABS/ERG not PRS.3.SG-T-DAT.3.SG-CL.E.1PL
 gusta-ten.
 like-IMP
 “Jon doesn’t like us.”
- (5) Gu-ri su-Ø/k gusta-te d-o-su. (>su)
 we-DAT you.SG.ABS/YOU.SG.ERG like-PRF PRS-T-2SG
 “We like you (Sg).”

These sentences do not contain a third person absolutive argument (in fact, idiolectal variants contain a *nonthird* person absolutive argument); and unlike unergative verbs, it seems highly implausible to analyze these psych-verbs as having an implicit third person absolutive argument. In Kiparsky’s own analysis of our (5) in tableau (6) below, adapted from Kiparsky’s (42), the winning candidate has *d-*, yet no other element in the auxiliary, either in the input or the output, is specified as third person absolutive.

(6)

		PARTDISS	MAX-2P	NONINITIALITY	ENCLISIS	CASEALIGNMENT	MAXTENSE
Present + Abs.2Sg + Dat.1Pl							
*s-a-tz+ku	2Sg-T-Dat+1Pl	*					
*a-tz+ku	T-Dat+1Pl		*				
☞ d-o-su	Pres-T-2Sg				*		
*s-a	2Sg-T				*		*

See section 14.3 for discussion of other details of Kiparsky’s analysis of (5) and similar auxiliaries.

A potentially more problematic aspect of the analysis is the claim that the present tense specification of *d-* is what prevents Ergative Metathesis. Kiparsky adopts Donohue's (2004) claim that it is a marker of tense, based on Trask (1977, 1997).⁶ However, Donohue's (and Trask's) proposal is not specific to *d-*. In particular, Donohue also claims that *z-* is a past tense marker that fills the same templatic position as *d-* (Donohue 2004:25, 31–32). Outside Biscayan Basque, past tense *z-* has a distribution similar to present tense *d-*: in auxiliaries without Ergative Metathesis, it surfaces in forms agreeing with a third 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 past tense. As in Biscayan, Ergative Metathesis in these dialects occurs in auxiliaries that (i) agree with a third person absolutive, and (ii) also contain an ergative (see, e.g. the description of Ergative Metathesis in Barua, the standard dialect, in Donohue 2004:34–38). Under the analysis in Kiparsky's chapter, 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 third person past tense forms *s-a-n* and *s-i-n* in the intransitive paradigm in Kiparsky's (25), and the transitive third plural absolutive forms *s-ittu-n* and *s-itxu-e(-se)-n* in Kiparsky's table (20) (the Ondarru Basque data in Kiparsky's tables are from Yrizar 1992:vol. 1, 221–225; 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'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 prediction that this makes is that a similar prefix should also block Ergative Metathesis in the past tense, contrary to fact.

These differences in the analysis of Ergative Metathesis and *d-* insertion are independent of the particular framework they are couched in. As Kiparsky states on p. 379, the analysis in *Morphotactics* in terms of last resort insertion of dummy *d-* is "not in principle incompatible" with the LM framework, and a DM account in terms of a tense-specified auxiliary-initial *d-* that blocks Metathesis is conceivable. Fortunately, these differences can be settled by empirical argument, which in this case is provided by cross-dialectal examination of the distribution of auxiliary-initial exponents. Kiparsky's account has forced us to look more deeply at this variation, which as we stated in Chapter 7 in *Morphotactics*, continues to be one of our long-term objectives.

6. Contrary to Kiparsky's claim on p. 379, neither Trask nor Donohue account for the absence of Ergative Metathesis in the present in terms of blocking by *d-*. See Donohue 2004:34–38 for the details of her analysis of Ergative Metathesis.

14.2.3 The Person-Case Constraint and Absolutive Promotion

In this section, we discuss Kiparsky's proposed alternative analysis of the PCC in Basque (which we have argued to be distinct in nature from the classic PCC familiar from Romance and Greek, and indirectly related to the specific fact that Basque lacks absolutive third-person clitics) and of the syntactic repair operation, Absolutive Promotion, employed to circumvent the PCC in certain configurations.

The LM analysis of the PCC in section 13.7 in Kiparsky's chapter is based on his constraint (31), which bans a misalignment between person prominence scales and Theta-hierarchy scales. Notably, it must be narrowly restricted, as having, for instance, a third person ergative above a second person dative—a clear misalignment—would not cause a problem. In *Morphotactics* (Chapter 2), the exclusion of ergative arguments from the domain of PCC effects is accomplished by the fact that ergatives move to a distinct clitic-host domain—a fact directly tied to their morphosyntactic position in an analysis in which cliticization involves hierarchical syntactic movement, although not directly tied to their position in Kiparsky's statement of the PCC constraint. Of more general interest perhaps is the fact that it does not appear to be a violable constraint of the same nature as the other constraints on word-forms—that is, it may not actually be a constraint on the same module/level of representation as the other constraints discussed in the chapter. It does not appear in any of the tableaux of auxiliaries whose candidates could potentially violate it (e.g. tableaux (42) and (45) in Kiparsky's chapter), which makes us wonder whether it is in fact somehow tucked into the workings of a presyntactic GEN. The PCC constraint might, alternatively, be a constraint on feature combinations within words in the syntax. Kiparsky's chapter stresses the claim that this version of the PCC based on Haspelmath (2004) is "a constraint on argument structure or on Th-role combinations, not on the distribution of morphosyntactic case" (p. 383), which would seem to emphasize its affiliation within the workings of GEN, and therefore not participate in the constraints on word-form that involve morphosyntactic case. But then, to account for the difference between two different types of Basque varieties—one that bans *all* dative arguments with first/second person absolutive arguments, and another that makes a distinction between applicative datives and locative datives—Kiparsky appeals to the fact that the PCC can apply to two different levels of representation: one version applies to abstract Case (which is reducible to the Theta-hierarchy), and the other to morphosyntactic case (which is not reducible to the Theta-hierarchy). In our account, this is again structurally accomplished: the PCC applies within a given syntactic domain of clitic-host competition, and in some Basque varieties, locative datives cliticize to a lower position, without the need for duplication of the same constraint applying to different levels of case.

Kiparsky's analysis of the Absolutive Promotion repair in section 13.8 is formulated in an intriguing manner, in terms of the fusing of two distinct Theta roles, Cause and Theme (corresponding to ergative and absolutive case, respectively), specifically because of the cause-based semantics involved in psych-verbs, based on Donohue's (2004) analysis of these verbs in Basque. The idea is that this fused role usually surfaces as absolutive, but can surface as ergative instead when needed. Because no tableaux are provided, the question that arises here is how the analysis derives that Absolutive Promotion applies only in auxiliaries that would potentially violate the PCC. Is there some additional constraint that ordinarily requires that fused Cause and Theme roles in psych-verbs should show up as absolutive, which is violated specifically in Absolutive Promotion/PCC contexts, and if so, what is the nature of this constraint? This is not explained in the chapter.

Finally, it is worth remarking that if we were to adopt Kiparsky's alternative formulation of Absolutive Promotion, it wouldn't change the central questions of opacity and modularity per se, which we take to be the central pivots of the debate. It indeed might be interesting to have Absolutive Promotion (still a last resort!) come from the argument-structure of syntax-semantics mapping (still syntactic) instead of clitic landing sites, but in any event, Absolutive Promotion would remain housed earlier than the postsyntax, and the opacity arguments about how it interacts with later operations, such as Participant Dissimilation and Ergative Metathesis, would remain intact (see section 14.3 for further comments on these interactions).

14.2.4 The relation between T and absolutive and dative arguments

The shape of T in Basque auxiliaries depends to a great extent on the φ -features of the absolutive argument. In *Morphotactics* (Chapters 2–3), we proposed that this is due to (syntactic) agreement by T; but in the LM account, this sensitivity is analyzed in terms of contextual allomorphy (pp. 365, 376 in Kiparsky's chapter). According to Kiparsky, the allomorphy analysis, coupled with the structure for auxiliaries assumed in the LM account and morphological locality, has the "natural consequence" that the form of T "only ever depends on absolutive clitics; if there is no absolutive clitic in the auxiliary, T assumes its default shape." (p. 376)

This prediction of the LM analysis is in fact falsified by the Ondarru Basque paradigm in (1) in Kiparsky's chapter. First, features of the ergative clitic do have an effect on the form of T. For instance, both *n-a-su* and *n-au* crossreference a first singular absolutive argument, and the difference in the form of T (*-a-* vs. *-au-*) correlates with the features of the ergative (second singular and third singular, respectively). A similar ergative-anchored contrast is observed in the third singular absolutive forms *d-o-su* (second singular ergative) and *d-au* (third singular ergative). This allomorphy of T with respect to ergatives was one of the arguments in *Morphotactics* for a linear

adjacency-based account of contextual allomorphy. Second, the claim that when there is no absolutive clitic T appears in a default form ignores the fact that its shape in third person absolutive forms in Kiparsky's (1) is dependent on the number (singular vs. plural) of the absolutive argument (e.g. in the row for third singular ergative, the third singular absolutive form is *d-au*, whereas the third plural absolutive form is *d-oittu*). This fact is highly relevant, as it shows that the form of T is sensitive to features of the absolutive argument *even in the absence of a clitic in the auxiliary cross-referencing that argument*. This long-distance effect defies any constrained theory of locality in contextual allomorphy and thus argues for the agreement-based account in *Morphotactics*.

Further data bearing on this issue come from the phenomenon in the Lekeitio dialect referred to as "First Dative Impoverishment" in *Morphotactics* (Arregi & Nevins 2012:86–88), and mentioned by Kiparsky in footnote 10. In some auxiliary forms in this dialect, the auxiliary-initial position usually reserved for absolutive marking (or *d-*) in fact crossreferences features of the dative argument (and, correspondingly, no dative enclitic is present). Interestingly, although this makes the shape of T sensitive to the φ -features of the dative argument, it still shows sensitivity to those of the absolutive argument, as shown in *Morphotactics* (Arregi & Nevins 2012: pp. 156–162; see especially the discussion of the Lekeitio form *n-itu-Ø-s* in (102) on pp. 160–161). This is despite the absence of any other morpheme in the auxiliary agreeing with the absolutive. Like other forms discussed previously, these auxiliaries also present clear evidence that the shape of T correlates with φ -features of arguments not otherwise cross-referenced in the auxiliary, which strengthens the argument for an agreement analysis.⁷

As with other aspects of the Basque auxiliary system, these analytical choices are largely independent of the particular framework they are couched in, because agreement and contextual allomorphy are distinct basic processes that are indispensable in both DM and LM. The exponence of T is one of the most complex issues in the analysis of Basque auxiliaries, and is subject to a bewildering amount of cross-dialectal variation. It is only by proposing and comparing detailed analyses, such as those given in *Morphotactics* and Kiparsky's chapter, that we can hope to obtain an understanding of this phenomenon and its relation to others in Basque morphology.

7. In reference to these Lekeitio forms, Kiparsky states in footnote 10 that he has "not found any instances of actual dative agreement" in *Morphotactics*. We described these forms as having dative agreement because both the auxiliary-initial clitic and T cross-reference φ -features of an ostensibly dative argument (e.g. (90) on p. 87 in *Morphotactics*). Although the clitic itself that T is agreeing with is arguably absolutive (we agree with Kiparsky on this), the important point here is the case mismatch between the dative argument and the absolutive-looking morphemes crossreferencing it in the auxiliary (the proclitic and T). Kiparsky does not offer an analysis of this mismatch.

14.2.5 The bimorphemic analysis of dative markers

The LM account adopts a bimorphemic analysis of datives (pp. 365–366), which is ultimately relevant for the way that specific constraint evaluation works; in particular, whereas in *Morphotactics*, we appeal to principles of specificity to explain why ergative, but not dative, clitics metathesize to satisfy NONINITIALITY, Kiparsky employs the clever mechanism of positing that datives are bimorphemic, and hence cannot metathesize, as this would put the auxiliary in third, rather than second, position, resulting in too many violations of ENCLISIS, as illustrated in the tableau in (8) for the auxiliary *n-e-tz-an* in (7) (adapted from Kiparsky's (45) and (44a), respectively).

- (7) Liburu- \emptyset emo-n n-e-tz-an.
 book-ABS give-PRF 1SG-T-3SG.DAT-PAST
 "I gave him the book."

(8)

	NONINIT	ENCLISIS	CASEALIGN
Past + Abs.3Sg + Dat+3Sg + Erg.1Sg			
*e-tz+ \emptyset -t-an T-Dat+3Sg-Erg1Sg-Past	*		
*tz+ \emptyset -e-t-an Dat+3Sg-T-Erg1Sg-Past		**	
☞ n-e-tz+ \emptyset -an Erg1Sg-T-Dat+3Sg-Past		*	

Candidate *tz+ \emptyset -e-t-an*, with the bimorphemic dative *tz+ \emptyset -* before T, incurs two violations of ENCLISIS, one more than the winning candidate in which monomorphemic ergative *n-* precedes T. Kiparsky's footnote 5 addresses some of the objections to the bimorphemic analysis that were presented in *Morphotactics* (Arregi & Nevins 2012:130–131), but does not address all of them. In intransitive auxiliaries in Lekeitio and Zamudio (two Biscayan Basque varieties analyzed in *Morphotactics*), the first singular dative clitic is *-t/-na*, not *-st*, and the first plural dative clitic is *-ku*, not *-sku* (de Yrizar 1992:vol. 1, 97, 591). These do not have the initial fricative Kiparsky's bimorphemic analysis relies on.

More important, however, is the nature of the constraint evaluation that the bimorphemic analysis is supposed to help with. For example, what prevents proclisis of just one of the dative morphemes in (8)? In other words, what rules out candidates such as *tz-e- \emptyset -t-an*, in which only one half of the putative bimorphemic dative moves? A related, highly problematic detail has to do with Kiparsky's analysis of the third person dative allomorph *-ko* as a portmanteau (p. 365) instead of a bimorphemic form (precisely because the putative decomposition has no leg to stand on here). This is an interesting proposal, including the mention of a syntagmatic constraint that bans

this particular morpheme in the presence of any other clitic (p. 392). However, if as standardly assumed, a portmanteau is monomorphemic (which it must be, for this latter constraint to make sense), then *-ko*, unlike other datives, could potentially surface as a proclitic to satisfy NONINITIALITY, an incorrect result.

The larger issue is not the question of the decomposition of datives into two morphemes, which as we have pointed out, faces a number of challenges including the need to posit zero morphemes, portmanteaux, and the inevitable patches that would be required to prevent the metathesis of a dative morpheme that would not incur third-position violations; what is ultimately at stake is the fact that our appeal to case-specific constraints on metathesis in *Morphotactics* is independent of the surface form in specific dialects and argument-structural combinations, and generalize across all instances of the dative exponent.

14.3 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 chapter, this is in principle amenable to an OT-based analysis, 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, based on his LM account of the phenomenon offered in Section 13.10.

Within this discussion, we'd like to highlight the pertinence of the interaction between Absolutive Promotion and Participant Dissimilation (Arregi & Nevins 2012:346–349). This is one of the places that Kiparsky's reanalysis of opaque interactions founders, as can be illustrated by the tableau in (6) (adapted from Kiparsky's (42)), which involves Participant Dissimilation in a sentence headed by a psych-verb (5): the input has a first plural dative plus a second singular absolutive, but no dative clitic surfaces in the winning output. However, in addition to deletion of the dative clitic, the realization of the second singular argument is as an enclitic—even though it is the sole output argument. In *Morphotactics*, this involves the opaque interaction of Absolutive Promotion, whereby the otherwise absolutive participant Theme argument is promoted to ergative syntactically due to the presence of an Experiencer dative, followed by the fact that it triggers postsyntactic deletion of the dative clitic in its derived position (Participant Dissimilation).

In the LM analysis, the important question is how to guarantee that the sole remaining clitic will be an enclitic, rather than a proclitic. The way it's done in (6) is that candidate *s-a* loses because the analysis assumes that the T morpheme *-a-* is not specified

for present tense, violating MAXTENSE; and as a result, *d-o-su*, with enclitic realization and the putatively present-tense *d-* morpheme, wins out. But notice that the analysis in fact implicitly involves opacity, as the operation responsible for theta-collapse, whereby the theme is realized as an ergative due to the presence of a dative, is taking place even despite the surface absence of the dative clitic. There seems to be no way around this being a case of counterbleeding: deletion of the dative clitic, if it were to have taken place first, would bleed Absolute Promotion, and the result would always be *s-a*, contrary to fact. Compare this to a true intransitive present tense form with a single argument, which is in fact *s-a*. There is, however, no way to block *d-o-su* for this intransitive as the analysis stands; (6) seems to involve overapplication of Absolute Promotion, yielding the enclitic realization. This is because, as summarized previously, this enclitic realization is independent of the presence of a dative argument in the input. No matter what Kiparsky changes in the analysis, it seems to always predict either *s-a* or *d-o-su* as the form that would win for both intransitives and psych-verbs.

The reason that our analysis in *Morphotactics* is different is that even though on the surface, *d-o-su* involves a neutralization between absolute and ergative sources, we have clear derivational stages in which absolute and ergative are different. An “early” absolute that changes to ergative in the presence of a dative is realized with *d-o-su* (even if that dative isn’t present at the surface), and an absolute that never undergoes any change and never has any co-argument is realized with *s-a*.

To conclude this section, Kiparsky is right in criticizing the analysis in *Morphotactics* because of its “underutilization of rule ordering” (p. 392). Our rich modular architecture for the postsyntactic component predicts more opacity than we do in fact see in Basque auxiliaries. However, the LM analysis does not seem well-equipped to handle uncontested cases of opacity, such as the counterbleeding interaction between Absolute Promotion and Participant Dissimilation described previously. In the absence of alternative analyses that get the facts right, it seems that the derivational architecture assumed by DM provides the right tools to account for the attested cases of opacity, although it remains to be seen whether it can be constrained enough to rule out the unattested cases.

14.4 Does DM fail to characterize wordhood?

Kiparsky’s main argument for this point in Section 13.11 comes from modal particles, which do not count with respect to the NONINITIALITY requirement on T. *Morphotactics* used a distinction between X^{0max} and 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^{0max} , occurs after

Vocabulary Insertion of both relevant pieces. The relevant argument, therefore, is that X^{0max} generates too many constituents in a word, namely “as many X^{0max} heads as it has terminal X heads” (p. 393 in Kiparsky’s chapter), that are not counterparts of the lexical word in a lexicalist framework. 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, because the word internal structure of Basque auxiliaries contains sub-word constituents that the account makes crucial use of (via CASEALIGNMENT; see section 14.2.1).

Moreover, it’s not 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. 394 in Kiparsky’s chapter). Kiparsky (section 13.11) takes this to be a virtue of the LM analysis because the word domain in a lexicalist framework comes “for free.” However, the LM analysis of Basque auxiliaries based on this constraint in fact depends on a domain smaller than words; specifically, in (1) (Kiparsky’s (11a)), there are two nonterminal T nodes, and Kiparsky’s incremental cyclic account requires that NONINITIALITY be evaluated immediately on the lower one, which does not correspond to a word (see p. 369 in Kiparsky’s chapter for details). This account considerably weakens the argument for lexicalism in Kiparsky’s section 13.11 because NONINITIALITY effects are no longer based on word domains, and in fact apply at domains below the word, exactly as we claimed in *Morphotactics*.

Kiparsky’s arguments against the DM characterizations of wordhood relate to his objection to X^{0max} , a domain that we appeal to for the statement of NONINITIALITY, as mentioned previously. A particularly telling argument for T^{0max} in *Morphotactics* is the one based on Root Reduplication (Arregi & Nevins 2012:326–332), where there are two copies of T within the same M-Word. In the resulting word (*d-o-sta-s-endu-n*), the rightmost copy of T (*endu*) is four morphemes away from the left edge of the M-word, but it’s in second position within T^{0max} . Although it is not discussed in his paper, we assume that Kiparsky would again have to appeal to domains below the word level in order to account for the placement of this second copy of T.

Kiparsky’s major objection is based on the treatment of modal particles (e.g. evidential *ei*), which are outside the scope of the T^{0max} , but inside the M-Word. His criticism of constituency is based on alleged problems with word-level phonological processes (p. 395, including footnote 23); in particular, that modal particles on our analysis form a constituent with T^{0max} to the exclusion of ergative clitics and C. By contrast, Kiparsky contends that these modal particles adjoin to form a post-lexical word. We know of

no extant argument (phonological or otherwise) that the constituency of *ei n-a-su* (Kiparsky's (53)), with evidential *ei* and auxiliary 1Sg.Abs-T-2Sg.Erg, is *[ei [nasu]]* and not *[[ei na]su]*, as we have it in *Morphotactics*. However, we did not include enough discussion of stem-level versus word-level phonology in our section 3.6 on phonological processes, a criticism that we accept, and we find Kiparsky's suggestion of more variability in the application of lenition across word-level boundaries (footnote 23) to be an interesting direction to pursue, and more generally support the investigation into how postlexical phonological domains interact with what we call M-Words.

In fact, our main reason for sandwiching the Mod projection (which hosts these modal particles) between T and C in *Morphotactics* (Arregi & Nevins 2012:333–339)—rather than as external clitics—was to generate them where we see morphemes of this type cross-linguistically (according to Cinque 1999), with converging evidence from typology and semantic scope. 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? It is not sufficient to say that certain elements cliticize onto the auxiliary without saying where those elements come from within a syntactic tree. Related to this, the LM analysis contains no proposals about plural *-s* or past tense *-n*, both of which are in C in *Morphotactics* (Arregi & Nevins 2012:88–95, 103–108). With no commitment to these crucial elements, the comparison between LM and DM approaches to the structure of complex words is incomplete.

14.5 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 constraint on argument structure or on Th-role combinations, not on the distribution of morphosyntactic case" (p. 383) 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 First Dative Impoverishment): it is an output-based theory, but it 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 that 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 contains 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 in morphological theory as applied to systems like Basque is not likely to arise purely from comparison of which framework has fewer, partially overlapping modules—as both in fact do. Indeed, classic work in Kiparsky's own Lexical Phonology architecture is predicated on the idea of separate strata, each with overlapping but distinct principles and constraints. Instead, we contend that explanatory progress will result from comparison of which specific set of generalizations and proposals lead to a better understanding of the nature and limits of the constraints and repairs that characterize the closely related dialects discussed in *Morphotactics*.

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DIACHRONIC SOURCES OF ALLOMORPHY

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In approaches to phonologically conditioned suppletive allomorphy (PCSA) based on allomorph selection via subcategorization frames (Paster 2006, 2009, Bye 2007), it is a coincidence from the point of view of the grammar that PCSA often appears to be phonologically optimizing. This is not true in Optimality Theoretic (OT) approaches to PCSA (e.g. McCarthy and Prince 1993a, 1993b, Wolf 2008), where phonological markedness constraints drive the allomorphy (in the classic approach, this is achieved by ranking phonological (P) constraints over morphological (M) ones; hence “P » M”). Thus, the former approach has been criticized (Bonet et al. 2007:904, Mascaro 2007:721, Wolf 2008:108) for not explaining the apparent optimization. Embick (2010) calls this the “putative loss of generalization” (PLG) argument.

Three main defenses against the PLG argument have been advanced. First, examples of PCSA are cited (see Paster 2006) that seem neutral or even “perverse” in terms of well-formedness. These are hard to analyze in OT without stipulative, unnatural, language-specific constraints that undermine the claim that PCSA is driven by phonological well-formedness. A second defense is based on a detailed look at some putatively optimizing cases (see especially Embick 2010), showing that even where an established markedness constraint can be used to analyze PCSA, often stipulative “priority” constraints are needed for a complete analysis. Again this undermines the claim that PCSA is markedness-driven. A third defense (and the focus of this chapter) is that apparent optimization in some cases of PCSA is not coincidental but reflects historical changes. The forces behind the sound changes and other historical developments giving rise to PCSA can account for the apparent naturalness of synchronic patterns.

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