Abstractness and motivation in phonological theory*

Eric Baković

University of California, San Diego

October 26, 2008

1   Abstractness and its motivation

There can be no question, I think, that even the most basic work of phonological description and analysis involves some amount of abstraction from what can be directly observed, recorded, and measured. Our theories are likely to differ in their specifics, but abstraction in some way, shape, or form will be necessary. As Odden (2005: 258) aptly puts it: "Without generalizing beyond the directly observable, it would be impossible to make even the most mundane observations about any language. The question is therefore not whether phonology is abstract at all, but rather what degree of abstractness is required."

In this piece I adopt the standard textbook definition of 'abstractness' in generative phonological theory, "the degree to which a UR [= underlying representation] of a morpheme may deviate from its associated PRs [= phonetic representations]" (Kenstowicz & Kisseberth 1979: 179).[1] I do not distinguish 'phonetic representation' from '(phonological) surface representation' here, although I believe that there is a distinction to be made and that 'abstractness' in the relevant sense may exist between the two. I also follow standard texts in putting aside the inherent abstractness of representational elements themselves: distinctive features, discrete segmentation, syllabic constituent structure, rhythmic organization, and so on; there is also much to be said about this kind of abstractness, but I yield to both space considerations and the limitations of my expertise and personal interests in neglecting it.

I also adopt the perspective that a phonological analysis, independently of its 'degree' of abstractness, is (only) as adequate as the motivation and evidence that can be produced in favor of it and against substantive alternatives. Much like the notion 'degree of abstractness', the notions 'adequacy of motivation' and 'adequacy of evidence' are somewhat subjective and hence slippery matters, both influenced by and grounded in theoretical assumptions. This will be the focus of my remarks here.

---

*This piece probably would have benefitted enormously from questions, comments, and suggestions from others. For reasons not interesting enough to relate, I did not solicit such input. The views expressed herein, both right and wrong, are thus all mine — except, of course, where noted otherwise.

[1]See also more recent textbooks, e.g. Odden (2005), Ch. 9 and Hayes (2009), Ch. 12.

2   What counts as abstract?

Analyses that have traditionally been deemed 'abstract' in the relevant sense involve segments in the underlying representations of morphemes that differ from their correspondents in all of the surface alternants of those morphemes — or, in some cases, that do not even have correspondents in any surface alternant. Crothers (1973: 3) calls these abstract underlying forms "imaginary representations", representations that "contain at least one segment which is never directly realized phonetically, which either disappears entirely or which contains lexically features which do not ever together characterize a phonetic segment."

Consider, for example, Chomsky & Halle's (1968) proposal that the English morphemes *right*, *night*, and *might*, ending phonetically in [aɪt], end underlyingly in /ɪxt/. There are two examples of abstract analysis at play here: the claim of a relationship between 'lax' (or 'short') /ɪ/ and 'tense' (or 'long') [aɪ], and the claim that there is an underlying /x/ in these morphemes that is absent from all of their surface alternants, as it is from all surface forms of English. For present purposes, we'll take the former example of abstractness for granted and focus on the latter. Chomsky & Halle's primary concern with these words is that they have tense vowels not only in their basic forms but also in the derived forms *righteous* [ɹaɪtʃəs], *nightingale* [naɪɾɪŋgeɪl], and *mightily* [maɪɾɪliː]. These forms are expected to have lax vowels in these positions, following the more-or-less regular laxing pattern of *vice* [vaɪs] ∼ *vicious* [vɪʃəs] and *divine* [dɪvaɪn] ∼ *divinity* [dɪvɪnɪɾiː]. That they do not follow this pattern is taken to be sufficient motivation for a difference in their representation; they are assumed to have underlying lax vowels followed by /x/ and to undergo the following *ad hoc* rules in order.[2]

(1)   English tensing rules (Chomsky & Halle 1968: 234)

    a.   V ⟶ [+tense] / ___ x   (pre-velar fricative vowel tensing)
    b.   /x/ ⟶ Ø/ ___ C   (pre-consonantal velar fricative deletion)

So long as the tensing rule in (1a) follows the laxing rule that words like *righteous* are otherwise expected to undergo, the fact that such words have tense vowels is accounted for.

This analysis is clearly abstract in the relevant sense, given that the imaginary /x/ serves only to make the preceding vowel tense — itself a questionably abstract process — and is then deleted from all relevant surface alternants. More generally, though, what has counted as 'abstractness' in most if not all substantive theoretical discussions of the issue is this: if the analysis crucially involves a segment that does not surface intact *in any surface alternant of the set of morphemes directly relevant to the analysis*, it is abstract; otherwise, it is concrete.

3   What counts as motivation?

Odden (2005: 297) observes that "abstractness per se is not the issue; the proper question to be focusing on is what motivates an analysis." If motivation is the issue, then there must

---

[2]Chomsky & Halle cite a few other isolated irregular forms that can in principle be brought under the umbrella of one or both of the rules in (1), suitably generalized.

be a way to assess it — even if somewhat theory-dependently. In this section I review an example of an abstract analysis that may be familiar to readers of this journal, illustrating the sorts of considerations at stake in the difficult task of assessing motivation. My aim here is solely to scrutinize the forms of evidence that have been used to motivate this analysis.[3]

3.1   Spanish rhotics

Spanish is somewhat unusual among the languages of the world in contrasting two rhotics, a tap [ɾ] and a trill [r̄].[4] These contrast only in intervocalic position: *torero* [toɾéɾo] 'bullfighter' vs. *torrero* [toɾéɾo] 'lighthouse keeper'; *caro* [káɾo] 'expensive' vs. *carro* [kár̄o] 'car'.

The two rhotics are otherwise in complementary distribution. First, only the trill occurs word-initially: *rosa* [r̄ósa] 'rose', *ratón* [r̄atón] 'mouse'. Second, only the trill occurs syllable-initially after a consonant (effectively, after /n, l, s/): *honra* [ónr̄a] 'honor', *alrota* [alr̄óta] 'flax residue', *Israel* [isᶻr̄aél] 'Israel'. Only the tap occurs elsewhere: pre-consonantally and word-finally (that is, in syllable codas: *parte* [páɾte] 'part', *mar* [máɾ] 'sea') and as the second member of a complex onset (*tren* [tɾén] 'train', *obra* [óβɾa] 'deed').[5]

Harris (1969, 1983, 2001, 2002) analyzes the intervocalic contrast between the tap and trill as a quantitative one, between an underlying singleton rhotic (= the tap) and an underlying geminate rhotic (= the trill). Harris (1969: 50, 1983: 70) fully specifies this rhotic as a tap, while Harris (2001: 136, 2002: 84) leaves it underspecified for the feature(s) distinguishing the tap and trill. Somewhat arbitrarily, I present the latter analysis here.

This analysis is of course abstract in the relevant sense, since the underlying quantitative distinction cashes out phonetically as a tap vs. a trill, both of which are in every relevant respect singleton consonants (and both firmly in the onset of the following syllable). Without a doubt, trills are durationally longer than taps, but then again one would be hard-pressed to find a consonant as short as a tap. Besides, the quantitative analogy stops there. Taps and trills couldn't be more different articulatorily (Ladefoged & Maddieson 1996: 217, 231).

Perhaps most significantly, there are no quantitative distinctions on the surface in Spanish at all — no long vowels and, more importantly, no geminate consonants.[6] Hualde (2004: 388) considers this point and concedes, somewhat tongue-in-cheek: "I suppose an answer to this objection would be to state that the true generalization is instead that Spanish does not have geminates *except for (underlying) geminate rhotics*" (emphasis added). But simply because it is possible to state an appropriate exception to the generalization does not mean that there are no grounds for the objection — as I believe Hualde would agree.

---

[3]I must thank Adam Albright here for, long ago, supplying me with an extremely useful database of Spanish words coded with relevant information to check various empirical claims against.

[4]I follow Whitley (2003) and Hualde (2004, 2005) in using the IPA [ɾ] for the tap but adding the macron diacritic to better distinguish the trill [r̄] (Harris 1983 also uses the macron; others sometimes use a tilde). I also put aside the fact that there is significant phonetic variation in the production of both rhotics.

[5]I put aside for now the optional emphatic and 'more careful speech' trills that occur in subsets of these last two sets of positions, and return to the special case of morpheme-final prevocalic position in §3.6.

[6]A handful of exceptions are frequently noted, such as *innato* [innáto] 'innate' and *perenne* [perénne] 'perennial'. The former is a prefixed form, /in+nato/, with a 'fake geminate'; the latter is monomorphemic, but Harris correctly dismisses it as "an isolated exception" in other work (Harris 1984: 71).

These legitimate questions of abstractness aside, the underlying geminate rhotic becomes a surface trill by applying the rules in (2a,b) below in the order shown, as demonstrated in the sample derivation in (2c).[7] (The order is argued to follow from independent considerations; see §3.2.) Surface taps are derived by a context-free /R/ $\longrightarrow$ [ɾ] rule applying after these rules; this step is unnecessary if the rhotic is underlyingly specified as a tap.

(2)   Spanish trilling rules

    a. /R/ $\longrightarrow$ [r̄] / C $_\sigma$[__   (syllable-initial, postconsonantal rhotic trilling)

    b. /R/ $\longrightarrow$ Ø/ __ r̄   (pre-trill rhotic deletion)

    c. Derivation for *perro* 'dog': /peRRo/ $\underset{(2a)}{\longrightarrow}$ |peRr̄o| $\underset{(2b)}{\longrightarrow}$ [pér̄o]

Note that the rule in (2a) is stated in such a way as to accomodate syllable-initial trilling after /n, l, s/ as well as after another /R/. Word-initial trills, by contrast, must be derived independently under this analysis. Harris (1983: 70) states this as a separate rule, but Harris (2001: 137, 2002: 84) adds the word-initial context to the syllable-initial, post-consonantal context in (2a), creating a disjunctive environment. Harris (1969: 52) also considers a rule with a disjunctive environment, but in the discussion immediately following he (tentatively) collapses the word-initial case with an independent rule strengthening word-initial glides.

Naturally, the concrete alternative to this analysis is to propose an underlying contrast between the tap and the trill, with appropriate neutralization rules applying in all but intervocalic position where the contrast survives. Harris (1969, 1983, 2001, 2002) presents several arguments to motivate the abstract, underlying geminate analysis of the intervocalic trill; the most significant of these arguments are discussed and assessed in turn in the subsections below. (For reasons of space, I shall have to leave the remaining arguments unaddressed.)

3.2   The nondistinctiveness evidence

Harris (1983, 2001, 2002) makes the empirical claim that there is no phonetic distinction between examples like *salir rápido* /salir#r̄apido/ 'to leave quickly' and *salí rápido* /sali#r̄apido/ 'I left quickly' — that is, between a word-final tap followed by a word-initial trill and a word-initial trill by itself. Both are claimed to surface with a trill, [salir̄apiðo], which is taken to be independent evidence for the pre-trill rhotic deletion rule in (2b). This independent evidence is significant in the following two ways.

First, (2b) is now on more-or-less equal footing, motivation-wise, with (2a), for which the independent evidence is obligatory trilling syllable-initially after other consonants (/n, l, s/). Indeed, Harris (2002: 84) attempts to extend the evidence for (2b) to also encompass other consonants; specifically, /s#s/ is also claimed to be categorically simplified across word boundaries, such that *los serví* 'I served them' and *lo serví* 'I served it' both surface as [loserβí] — "unless pauses are unnaturally inserted between written words."

---

[7]Harris (1969: 55) proposes to derive the surface intervocalic trill by "a one-step transformation since it would be totally arbitrary to have to choose one of the two input [rhotics] to become [a trill], the remaining [rhotic] then being deleted as a second step." See §3.2 below for the key to Harris's later change of heart.

Second, the order between (2a) and (2b) can now be said to follow from the difference in their domains of application: (2a) is a word-level (= lexical) rule, while (2b) is a phrase-level (= postlexical) rule. This particular conclusion depends, of course, on two things: (a) that there is an inherent order between lexical and postlexical rules (we'll join Harris in assuming that there is), and (b) the strength of the evidence for each rule being assigned to the relevant level. The evidence for (2b) being a postlexical rule is precisely Harris's claim that it applies across word boundaries. The evidence for (2a) being a lexical rule, however, appears to depend entirely on whether it is collapsed with the rule responsible for word-initial trilling: Harris (1983) states word-initial trilling separately and (thus?) stipulates the order between (2a) and (2b); Harris (2001, 2002) collapses word-initial trilling with (2a) and thus concludes that it is a word-level rule, which precedes postlexical rules "by definition" (Harris 2002: 85).

Now, let us return to the supposed evidence for (2b)'s postlexical application. While it may be true that native speakers fail to reliably *perceive* a distinction between /ɾ#r̄/ and /#r̄/ (and I emphasize that this remains to be conclusively shown), Hualde (2004) contests the claim that native speakers never *produce* such a distinction. Hualde recorded and analyzed native speaker productions of various relevant examples, and found that productions of /ɾ#r̄/ were, on average, significantly longer than productions of /#r̄/; that is, while individual realizations of /ɾ#r̄/ may be nondistinct from /#r̄/, the two are on the whole kept distinct. (In fact, Hualde also found a more-or-less equally significant difference between /s#s/ and /#s/, as well as an even greater difference between /n#n/ and /#n/.)

Hualde (2004: 390) concludes from this experimental result that the geminate analysis of the trill is falsified, at least insofar as that analysis depends on the surface nondistinctiveness of /ɾ#r̄/ and /#r̄/. I find the evidence either way to be inconclusive. First, it may be that lexical (= word-internal) application of (2b) is obligatory/categorical while its application postlexically (= across word boundaries) is optional/gradient, a not uncommon state of affairs (*cf.* nasal place assimilation in English; Kiparsky 1985: 86). Second, perhaps the relevant duration comparison to be made is between /ɾ#r̄/ and word-internal, intervocalic trills — after all, it is these latter trills, not word-initial ones, that are argued to be derived from underlying geminates and that are therefore subject to (2b). If we were to find that word-internal, intervocalic trills are (on average) not significantly shorter or longer than /ɾ#r̄/, then (2b) might simply be optional/gradient across the board. In any event, the underlying geminate analysis does not live or die by the particular nondistinctiveness claim made by Harris (1983, 2001, 2002) and shown to be unsubstantiated by Hualde (2004).

3.3 The stress evidence

Closed penultimate syllables preclude antepenultimate syllable stress in Spanish words (e.g. *[ká.nas.ta]*, *cf. canasta* [ka.nás.ta] 'basket'). Harris (1983: 68) points out that Spanish words with antepenultimate stress and a trill in the onset of the final syllable are nonexistent (e.g. *[t͡ʃá.ma.r̄a]*, *cf. chamarra* [t͡ʃa.má.r̄a] 'coat'), and that Spanish speakers systematically reject such hypothetical examples. Harris argues that this fact can be accounted for by appealing to the underlying geminate rhotic representation of the intervocalic trill: as a geminate in e.g. /t͡ʃa.maR.Ra/, it closes the penult and thus precludes antepenultimate stress; the rules

in (2) then apply to reduce the geminate to a trill in the onset of the final syllable.

Though compelling, I share the view with other scholars that this evidence raises more questions than it answers. Antepenultimate stress is sufficiently rare in Spanish that one can point to several gaps that are just like *[ʧá.ma.r̄a] in relevant respects. Most notably, if the palatal nasal [ɲ], palatal lateral [ʎ], or postalveolar affricate [ʧ] occur in the onset of the final syllable, antepenultimate stress is also precluded, as shown in (3).

(3) Antepenultimate stress precluded by palatals / postalveolars in final onsets

    a. *[kám.pa.ɲa]   *cf. campaña*   [kam.pá.ɲa]   'campaign'

    b. *[ká.ba.ʎo]   *cf. caballo*   [ka.bá.ʎo]   'horse'

    c. *[és.tu.ʧe]   *cf. estuche*   [es.tú.ʧe]   'case'

These facts make sense from a historical perspective, as these segments derive from geminates or heterosyllabic clusters in Latin (see Penny 2002, among many others): Sp. /ɲ/ < L. /nn/, Sp. /ʎ/ < L. /ll/, Sp. /ʧ/ < L. /kt/ — and, not surprisingly and most significantly, Sp. /r̄/ < L. /rr/.[8] Latin, of course, also precluded antepenultimate stress when the penult was closed (more generally *heavy*, as Latin had long vowels that were lost in Spanish), and so the absence of the relevant proparoxytones is probably best viewed as an inherited fact — one that speakers are apparently aware of to some extent, but this can hardly be viewed as knock-down evidence for some synchronic grammatical reality.[9]

Harris claims that there are native speakers who reject *[ʧá.ma.r̄a]-type examples more strongly than they do unattested examples like those in (3) (Harris 1983: 144, note 22), and that there are some who reject the former but accept the latter (Harris 2002: 102). However, as Harris (2002: 102, note 41) partially admits, "not all informants have reliable intuitions regarding these cases" and, following up on a useful distinction made by Harris (1988: 19), "unsure judgments are to be expected: they involve the task of discriminating between 'unknown, peripheral, and inadmissible' vs. 'unknown and peripheral but admissible' categories of test items." This simply underscores the need to develop more reliable methods for gathering judgment data of this kind — not to mention the need to share our data-gathering methods so that they can be replicated and/or improved upon.

3.4   The position of contrast evidence

A cross-linguistic fact in favor of the geminate rhotic analysis of the intervocalic trill is that (undisputedly real) singleton/geminate contrasts often hold exclusively in intervocalic position, just like the contrast between the tap and trill. Harris (2001: 138, 2002: 87) goes further, arguing that independent evidence about syllabification in Spanish restricts the contrast between the tap (*qua* singleton) and trill (*qua* geminate) to intervocalic position.

---

[8]See also Lipski (1990: 168) and Hualde (2004: 391) for this historical argument.

[9]There is one proparoxytone with a voiceless velar fricative in the final onset: *cónyuge* [kóɲ.ʝu.xe] 'spouse'. Parallel examples with other voiceless obstruents are more common, but are overwhelmingly words of Greek origin; voiceless stops of Latin origin were also once geminates, reinforcing the conclusion in the text.

Briefly, the argument goes like this. The contrast is allowed intervocalically because syllabification of both the singleton (as an onset) and the geminate (as a coda + onset sequence) is possible. In all other contexts, the geminate is unsyllabifiable: because it would have to be adjacent to a consonant or a word edge (or both), a disallowed complex onset or coda would need to be formed in order to accomodate the geminate. The singleton, on the other hand, can be syllabified in all other vowel-adjacent contexts except V__C#, again because a disallowed coda cluster would need to be formed in this case. Word-initially and syllable-initially after a consonant, of course, the singleton is obligatorily realized as a trill; otherwise, it is realized as a tap (or optionally as a trill in emphatic speech; see note 6).

An arguably more explanatory account of the privileged positions of contrast between taps and trills in Spanish (and other languages) is offered by Bradley (2001a, 2001b: 124*ff*). Again, briefly: the rapid ballistic gesture required for the articulation of a tap is best implemented intervocalically, where its momentariness is also best perceived; this makes the intervocalic position the best position (and the only position, if there is to be only one) for a language to maintain a durational contrast between a tap and a trill. Bradley's account of course appeals more to functional considerations of contrast as opposed to the more formal considerations of syllabification appealed to by Harris; until the rather idle controversy between these two analytical approaches is generally settled or muted, however, there is sufficient doubt cast on Harris's argument to definitively conclude anything from it.

3.5 The cluster gap evidence

Harris (1983, 2002) offers what appears on the face of it to be a rather ingenious argument in favor of the underlying geminate rhotic analysis of the intervocalic trill. First, Harris (1983: 67) observes that, in word-internal, syllable-final position, the tap occurs before an unrestricted set of syllable-initial consonants — the only exception being /ɲ/, which does not cluster with any consonant (Harris 1983: 144, note 20). Second, Harris (2002: 101) observes that the other coronal sonorants /n, l/ do not regularly cluster with following coronal sonorants, while the tap does; e.g., *terno* [téɾno] 'suit', *perla* [péɾla] 'pearl'. In both cases, Harris concludes that a geminate rhotic — that is, a rhotic + rhotic cluster — fills an otherwise unexplained gap in the overwhelmingly prolific clustering potential of the tap.

Harris (1983: 67) suggests that attempting to "account explicitly for this gap … will require an ad hoc addition to the grammar, against the universal judgment that ad hoc additions are to be avoided whenever possible", while Harris (2002: 101) states that such a gap would be "an isolated quirk". But how is it an isolated, ad hoc quirk for it to be stated in the grammar that clusters of identical consonants are disallowed? Indeed, the geminate rhotic forces one to add a quirky, ad hoc exception to this near-perfect generalization, as Hualde (2004: 388) points out (and as discussed above (2) in §3.1 above).

Furthermore, Harris (2002: 101) appears to undermine his own argument for the generality of (2a) with the claim that the coronal sonorants /n, l/ only exceptionally cluster with following coronal sonorants: the /n, l/ + rhotic clusters in e.g. *honra* [ónr̄a] 'honor' and *al-rota* [alr̄óta] 'flax residue' are dismissed as "idiosyncratic examples" of otherwise disallowed coronal sonorant clusters. The similarly singular example of *Israel* [isᶻr̄aél] 'Israel' rounds

out the 'independent evidence' that (2a) applies to anything more than the hypothetical explanandum at issue: the second half of a geminate rhotic.

3.6   The morpheme-final trill evidence

Regular nouns in Spanish fall into one of three classes, which I will refer to here as the *o*-class, the *a*-class, and the *e*-class. Members of the *o*-class and *a*-class have singulars that end in [o] and [a], respectively (e.g., *forro* [fór̄o] 'lining', *gorra* [gór̄a] 'cap'), and plurals are simply formed by adding an [s] suffix to the form of the singular (*forros* [fór̄os] 'linings', *gorras* [górras] 'caps'). The *e*-class consists of two phonologically-defined sets of nouns: those whose singular forms end in a single dental and alveolar consonant (which are phonologically licit word-finally) and those that end in other consonants or consonant clusters (which are not phonologically licit word-finally) followed by [e]. I refer to the former set as 'licit' and to the latter set as 'illicit'. Plurals of nouns in both sets end in [es] as opposed to the [s] of *o/a*-class plurals; the difference between the two sets is thus only evident in the singular.

(4)   'Licit' and 'illicit' *e*-class nouns

	licit			**illicit**			
sg	*animal* [animál]	*amor* [amór]	*pan* [pán]	*padre* [páðre]	*llave* [ʎáβe]	*valle* [báʎe]	*torre* [tór̄e]
pl	*animales* [animáles]	*amores* [amóres]	*panes* [pánes]	*padres* [páðres]	*llaves* [ʎáβes]	*valles* [báʎes]	*torres* [tór̄es]
	'animal(s)'	'love(s)'	'bread(s)'	'father(s)'	'key(s)'	'valley(s)'	'tower(s)'

The presence vs. absence of a final [e] in the singulars of *e*-class nouns is directly attributable to the licit vs. illicit nature of the final consonant or consonant cluster of these nouns: if it is a licit word-final consonant, then there is no final [e]; if it is an illicit word-final consonant or consonant cluster, then a final [e] is necessary. This raises the question of the final noun on the far right in (4): does the singular end in an [e] because the trill is an illicit word-final *consonant*, or because it is an illicit word-final consonant *cluster* — a geminate?

Harris (1969: 51) only lists illicit *e*-class forms with final clusters, thus tipping the scales in favor of the latter conclusion. Further support comes from the fact that the dental and alveolar consonants [ð, l, n, ɾ, s] are licit word-final consonants, and [r̄] should otherwise be a natural member of this class — but then so should [t] be, and it clearly is not; e.g., *bote* [bóte] 'boat' can be comfortably listed as a member of the illicit set of *e*-class nouns. Harris (2002: 96) excludes [t] by stipulating that the class of licit word-final consonants is "a special subset ⓒ that contains anterior coronals that are voiced or continuant or both", a convoluted description that may as well also stipulate the exclusion of the trill.[10]

---

[10]There are exceptional cases of otherwise illicit word-final consonants (e.g., *club* [klúβ] 'club', *carnet* [kaɾnét] 'ID card'), but there are also exceptional word-final clusters (e.g.,*vals* [báls] 'waltz') that in other cases behave as expected (e.g., *embalse* [embálse] 'dam, reservoir'). The fact that there are no exceptional word-final trills (save those due to optional trilling; recall note 5) thus does not point us in either direction.

Related to this general point is the absence of morphemes that end in a tap when phrase-final or preconsonantal, alternating with a trill when intervocalic; e.g. hypothetical, nonexistent [amóɾ] ∼ [amóɾ̄oso] ∼ [amóɾ̄es] (*cf.* existing *amor* [amóɾ] 'love' ∼ *amoroso* [amoɾóso] 'loving' ∼ *amores* [amóɾes] 'loves'). According to Harris (1983: 69), the absence of such alternations is explained by the geminate analysis: morphemes ending in /RR/ are expected to be followed by /o, a, e/ just like other cluster-final morphemes. As just noted, however, this evidence would be more persuasive if the alternative that [r̄] is simply not a member of the (redefined) "special subset Ⓒ" of licit word-final consonants were not also available.

3.7   The fake geminate evidence

Regular verbs in Spanish fall into one of three conjugation classes, where each class is defined in part by the 'theme vowel' that often appears between the verb root and inflectional suffixes: *a*, *e*, or *i*. Within the *e*-class, there is a handful of verb roots that, exceptionally, do not have a theme vowel in future or conditional forms. Some relevant examples are given in (5).

(5)   Typical and exceptional *e*-class verbs

	typical		**exceptional**			
infinitive	*correr*	/koRR-e-R/	*poder*	/poD-e-R/	*querer*	/keR-e-R/
	[koɾ̄éɾ]	run-TV-INF	[poðéɾ]	be-able-TV-INF	[keɾéɾ]	want-TV-INF
	'to run'		'to be able'		'to want'	
1sg future	*correré*	/koRR-e-R-é/	*podré*	/poD-R-é/	*querré*	/keR-R-é/
	[koɾ̄eɾé]	run-TV-FUT-1SG	[poðɾé]	be-able-FUT-1SG	[keɾ̄é]	want-FUT-1SG
	'I will run'		'I will be able'		'I will want'	

The typical set of *e*-class verbs is exemplified here by the verb *corr-e-* 'run', which expresses the theme vowel in both the infinitive *correr* and the first person singular future *correré* forms. The exceptional set is exemplified by the verbs *pod-e-* 'be able' and *quer-e-* 'want', both of which express the theme vowel in the infinitive *pod-e-r* and *quer-e-r* but not in the first person singular future *pod-_-r-é* and *quer-_-r-é* forms.

The point of immediate interest concerns the last of these forms, *quer-r-é*. The verb root here ends in a tap, as is clear from the infinitive *quer-e-r* (compare *corr-e-r* with a root-final trill), and the future suffix immediately following it is also a tap, as is clear from *corr-e-r-é* and *pod-r-é*. The juxtaposition of these two taps apparently results in a trill: *quer-r-é* = /keR-R-é/ ⟶ [keɾ̄é]. Since this pair of underlying taps surfaces as a trill, it appears to be not too much of a leap to say that all trills are derived from pairs of underlying taps.

This argument appears convincing until one considers the following. First, *quer-e-* is the only verb relevant to this argument, and in fact, the only *e*-class verb root with a final tap.[11] Second, the set of exceptional verbs that behave like *pod-e-* and *quer-e-* in (5) is extremely small; Harris (1969: 96) lists five, and suggests that the list is exhaustive. Furthermore, all

---

[11] By comparison, there are many *a*-class and *i*-class verb roots with final taps such as *ador-a-* 'adore', *aclar-a-* 'clarify', *sobr-a-* 'be left over', *mor-i-* 'die', *abr-i-* 'open', *podr-i-* 'rot' ...

five of these verb roots exhibit "some irregularity in addition to the future stem" (Harris 1969: 97). It thus also appears to be not too much of a leap to simply list the various exceptional forms of these few verbs; evidence for a pattern deserving of explanation is simply not sufficient to warrant any conclusions beyond this small set of forms.

3.8 Summary

I have critiqued here the bulk of the evidence that has been brought out in favor of the abstract geminate rhotic analysis of the intervocalic trill in Spanish.[12] My conclusion in each case is that the evidence is not what it's cracked up to be. First, Hualde (2004) has shown that the claim that /ɾ#r̄/ and /#r̄/ are 'nondistinct' is empirically unsupported, undercutting the independent evidence for rule (2b). Second, the supposed evidence from antepenultimate stress preclusion is best understood as an inherited fact from Latin, and besides, more explicit and reliable methods for establishing the significance of the 'knowledge' that native speakers appear to have regarding this kind of fact need to be deployed.

Third, Bradley (2001ab) has demonstrated that the limitation of the tap/trill contrast to intervocalic position should be explained with reference not to syllable structure, but to contrast-relevant information regarding articulation and perception. Fourth, the fact that rhotics cluster with any following consonant *except other rhotics* is completely expected given the independent fact that no consonant in Spanish can cluster with itself — in fact, this more obvious latter generalization must be compromised if rhotic + rhotic clusters are allowed.

Fifth, the fact that trill-final noun roots must be followed by one of the class vowels *o, a, e* (e.g. *forr-o, gorr-a, torr-e*) is just as much an indication that the trill is not a licit word-final consonant as it is an indication that it is (at a necessarily abstract level of analysis) not a licit word-final cluster. And finally, the existence of a single, independently exceptional tap-final verb root (*quer-e*) that forms a trill in combination with the tap of the future and conditional suffixes is far from sufficient evidence for the claim that all intervocalic trills are derived from rhotic + rhotic sequences. I conclude from all this that the existing motivation for this analysis is mildly suggestive at best, quite apart from the issue of abstractness.

4 The irony of abstractness

Kiparsky's (1968) Alternation Condition was designed to rule out most if not all forms of abstractness as defined in §2. But of course, abstractness is simply a special case of Kiparsky's (1973) broader notion of *opacity*. Specifically, analyses considered 'abstract' are special cases of *counterbleeding* opacity: the process ensuring that the abstract element does not surface intact *fails to bleed* any process that takes crucial advantage of the element's abstract property. Because the motivation for the application of these latter processes (= the abstract property) is not present on the surface, the unbled processes appear to have applied without motivation — to have *overapplied*, to use McCarthy's (1999) useful terminology.

---

[12]See Bradley (2001b: 62*ff*) and Hualde (2004: 392) for critiques of two of the main remaining arguments.

In a rule-ordering approach, 'abstract counterbleeding opacity' is achieved by ordering the rule neutralizing the abstract element after any rule that is contextually sensitive to the element's abstract property. In a recent defense of this approach, Vaux (2007: 32) claims that the acquisition of opaque interactions between rules under this approach is simple: each rule is independently motivated in completely transparent contexts — i.e., contexts in which the rules do not interact with each other — and the observable result of their opaque interaction motivates their ordering. The requirement that each rule be independently motivated is a handicap for abstract analyses, however: the abstractness of the relevant element by definition does not reveal itself except by virtue of its effect on the behavior of other rules and of its impact on other types of generalizations in the language. The existence of the rule neutralizing the abstract element must thus be entirely inferred from these effects and impacts, throwing the question of motivation into sharp relief.

Kiparsky (1973) argues that opaque interactions in general are marked, on the very reasonable grounds that they are difficult to learn. For example, in the case of counterbleeding opacity, the lack of surface motivation for the application of an unbled processes should make the acquisition of that process more difficult than it would otherwise be. This is why independent motivation is important, particularly in the case of abstract counterbleeding opacity where the motivation for the other process is also less than clear. In general, then, abstractness should be more difficult to acquire than other forms of opacity — calling into question the need for an independent Alternation Condition to curtail it.

Now of course, the better and more plentiful the motivation for an abstract analysis, the easier it will be to acquire — but this is presumably true only to the extent that such motivation is unambiguously interpretable by the learner, which is not guaranteed to correlate with the extent to which the motivation is convincing to linguists. This is the cloud hanging over the heads of virtually all of Harris's arguments meant to motivate the geminate rhotic analysis of the intervocalic trill in Spanish.[13] On the basis of this (largely inconclusive) evidence, the learner is expected to come to the abstract conclusion that $[\text{V}\bar{\text{r}}\text{V}] = /\text{VRRV}/$ — as opposed to the alternative, concrete conclusion that $[\text{V}\bar{\text{r}}\text{V}] = /\text{V}\bar{\text{r}}\text{V}/$, with which none of the relevant evidence is in principle incompatible. This seems to me to be an extraordinarily unnecessary leap for the learner (and the linguist) to make, at least in this case.

But this leap is not without testable consequences for learnability. Even granting some or all of the evidence in favor of the abstract analysis, the fact remains that abstractness is opacity and as such should be difficult to learn in some quantifiable sense. I'm not aware of any research addressing the question in this way, but the available evidence does not seem promising: millions of native Spanish speakers around the world appear to learn the near-complementary distribution of the tap and trill (or their phonetic equivalents in various dialects) without notable difficulty,[14] and (most tellingly) there does not appear to be any

---

[13]And let's be clear about Harris's frequently stated commitment to the program of accounting for "a speaker's internalized linguistic competence" (Harris 1969: 4), "speakers' competence in their native language" (Harris 1983: 4), and "the linguistic competence of Spanish speakers" (Harris 2002: 81).

[14]Note that the acquisition of a distribution between two phonemes is technically separate from the order of acquisition of those phonemes. Harris (2002: 84) cites Meneses *et al.* (2000), a paper unavailable to me at this time, on the order of acquisition between the tap (earlier) and the trill ("considerably later").

trend among 'innovative' dialects of Spanish to reanalyze the distribution in some concrete way, as Kiparsky argues frequently happens with opaque interactions over time.

The 'irony' in this final section's heading refers to the way in which Kiparsky's notion of opacity has become the weapon of choice among phonologists of the rule-ordering persuasion, who vehemently argue — not quite following Kiparsky's actual position on the matter — that language is rife with opacity and that ordered rules provide a uniquely complete way of handling it.[15] What's ironic is that ordered rules make abstractness (*qua* special case of opacity) almost too easily possible — so much so that, apart from the relatively contained 'abstractness controversy' literature spawned by Kiparsky (1968), unquestionably abstract analyses are not sufficiently tested by appropriate evidence. The question to be asking, I think, is not "how opaque/abstract is phonology?", but rather "what does it take to learn opacity, or abstractness, or phonological phenomena in general?" Only with this question in mind can we hope to find appropriate evidence to decide between alternative analyses.

References

Baković, Eric. 2007. A revised typology of opaque generalisations. *Phonology* 24, 217-259. [ROA-850, Rutgers Optimality Archive, http://roa.rutgers.edu/.]

Bradley, Travis G. 2001a. A typology of rhotic duration contrast and neutralization. In M. J. Kim and U. Strauss (eds.), *Proceedings of the 31st North East Linguistics Society*, 79-97. Amherst, MA: GSLA Publications.

Bradley, Travis G. 2001b. *The Phonetics and Phonology of Rhotic Duration Contrast and Neutralization.* Ph.D. dissertation, The Pennsylvania State University. [ROA-473, Rutgers Optimality Archive, http://roa.rutgers.edu/.]

Chomsky, Noam, and Morris Halle. 1968. *The Sound Pattern of English.* Cambridge, MA: MIT Press.

Crothers, John. 1971. On the abstractness controversy. University of California (Berkeley) Department of Linguistics, Project on Linguistic Analysis *Reports*, Second Series, No. 12, CR1-CR29.

Harris, James W. 1969. *Spanish Phonology.* Cambridge, MA: MIT Press.

Harris, James W. 1983. *Syllable Structure and Stress in Spanish: A Nonlinear Analysis.* Cambridge, MA: MIT Press.

Harris, James W. 1984. Autosegmental Phonology, Lexical Phonology, and Spanish nasals. In M. Aronoff and R. T. Oehrle (eds.), *Language Sound Structure*, 67-82. Cambridge, MA: MIT Press.

Harris, James W. 2001. Reflections on *A Phonological Grammar of Spanish.* In J. Herschensohn, E. Mallen and K. Zagona (eds.), *Features and interfaces in Romance*, 133-145. Amsterdam: Benjamins.

---

[15]See Baković (2007) for a recent challenge to this view.

Harris, James W. 2002. Flaps, trills, and syllable structure in Spanish. *MIT Working Papers in Linguistics* 42, 81-108.

Hayes, Bruce. 2009. *Introductory Phonology.* Malden, MA: Wiley-Blackwell.

Hualde, José I. 2004. Quasi-phonemic contrasts in Spanish. V. Chand, A. Kelleher, A. J. Rodríguez, and B. Schmeiser (eds.), *Proceedings of the 23rd West Coast Conference on Formal Linguistics*, 374-398. Somerville, MA: Cascadilla Press.

Hualde José I. 2005. *The Sounds of Spanish.* Cambridge: Cambridge University Press.

Kenstowicz, Michael, and Charles W. Kisseberth. 1979. *Generative Phonology: Description and Analysis.* San Diego, CA: Academic Press.

Kiparsky, Paul. 1968. How abstract is phonology? Ms., MIT. [Published in 1973 as Part 1 of 'Phonological Representations', in O. Fujimura (ed.), *Three Dimensions of Linguistic Theory*, 5-56. Tokyo: TEC Company, Ltd.]

Kiparsky, Paul. 1973. Abstractness, opacity, and global rules. Part 2 of 'Phonological Representations', in O. Fujimura (ed.), *Three Dimensions of Linguistic Theory*, 5-56. Tokyo: TEC Company, Ltd.

Kiparsky, Paul. 1985. Some consequences of Lexical Phonology. *Phonology Yearbook* 2, 85-138.

Lipski, John M. 1990. Spanish taps and trills: phonological structure of an isolated opposition. *Folia Linguistica* 24, 153-174.

McCarthy, John J. 1999. Sympathy and phonological opacity. *Phonology* 16, 331-399.

Meneses, Sue, Lucille Herrasti, and Francisco Arellanes. 2000. Adquisición de las consonantes líquidas en español. Paper presented at *VI Encuentro Internacional de Lingüística en el Noroeste*, Universidad de Sonora, Hermosillo, México.

Odden, David. 2005. *Introducing Phonology.* Cambridge: Cambridge University Press.

Penny, Ralph. 2002. *A History of the Spanish Language*, 2nd edition. Cambridge: Cambridge University Press.

Vaux, Bert. 2007. Why the phonological component must be serial and rule-based. In B. Vaux and A. Nevins (eds.), *Rules, Constraints, and Phonological Phenomena*, 20-60. Oxford: Oxford University Press.

Whitley, M. Stanley. 2003. Rhotic representation: problems and proposals. *Journal of the International Phonetic Association* 33, 81-86.