Synchronic explanation

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The aim of this article is to show that synchronic cognitive constraints are responsible for some restrictions on human speech sound patterns; not all markedness asymmetries can be ascribed to mechanisms of diachronic change. We identify evidence for synchronic constraints in sound patterns that are desirable from a performance perspective yet are not attested. We also discuss recent experiments that provide evidence for psychologically and even neurophysiologically active restrictions; these patterns can be distinguished from statistical generalizations across the lexicon. We also argue that there is evidence that language learning is determined by innate predispositions. Finally, we examine the methodology behind choosing a synchronic or diachronic account for a particular sound pattern when both potentially offer an explanation.

1 Introduction

In an extreme view, all human speech sound patterns are due to restrictions on diachronic change. In such an approach, the phonological component is able to output any structure, respecting the formal properties of its objects and relations. The only reason that languages show systematic similarities is because there are particular grammars that are not learnable or unstable in diachronic transmission. This approach to explanation relies on restrictions in learnability and language acquisition, particularly language transmission and how it is perceived and articulated; hence, it can be called 'diachronic explanation'.

The most extensive and sustained advocacy of diachronic explanation can be found in John Ohala's work (see e.g. Ohala 1983, 1995). Barnes (2002) and Blevins (2004) present more recent theories. We acknowledge the contribution of Ohala and Barnes' work, but in order to sufficiently focus this article we will concentrate on Blevins' (2004) theory – Evolutionary Phonology. In any case, we intend the central points we make about Evolutionary Phonology to apply equally to other theories with a diachronic perspective on synchronic grammar.

The alternative to diachronic explanation approaches is that there are non-trivial cognitive restrictions in the phonological component. In this view, the lack of certain sound patterns is due to the phonological component's inability to generate them. We will call this 'synchronic explanation'.

We argue for synchronic explanation: i.e. that there are synchronically active restrictions on the phonological component. We examine two types of evidence. One is grammars (or parts of grammars) that are learnable, but never generated. Section 2 focuses on place of articulation in neutralization and epenthesis. We argue that synchronic neutralization to and epenthesis of [k] is unattested, yet is desirable for diachronic and performance reasons.

The other type of evidence, discussed in section 3, involves demonstrations of active synchronic restrictions. This evidence comes in four forms. First, the phonetic motivations of sound changes persist long after those changes have been phonologized. Second, sound changes can be optimizing in a ways that suggest that they interact with an active synchronic grammar. Third, synchronic constraints actively regulate linguistic behavior and do so in ways that indicate they are a thing apart from generalizations across the lexicon. Finally, language acquisition itself is apparently constrained by innate constraints on possible grammars.

To make one thing clear, we do not advocate an extreme 'synchronic explanation' view whereby every facet of every sound pattern is due to restrictions in the phonological component. The role for diachronic explanation is explored in section 4, and argued to account for the typological frequency of grammars and sound patterns. For example, the fact that of the voiced stops [g] is the least frequently attested cross-linguistically is not something that the phonological component necessarily should account for: the role of the phonological component is to be able to generate grammars both with and without [g]. Instead, performance issues such as confusability in perception and difficulty in articulation are responsible for [g]'s relative low frequency. We then address the long-standing issue of whether a grammar's lack of attestation is due to synchronic restrictions or diachronic pressures; we argue that in many cases both should be relevant.

2 Learnable grammars that cannot be generated

There are mismatches between diachronically desirable changes and synchronically attested sound patterns. Section 2.1 focuses on types of neutralization and epenthesis that are attested diachronically but never synchronically. Section 2.2 argues that a series of natural diachronic changes can easily lead to synchronically unattested grammars, a point recently made by Kiparsky (2004). Building on Kiparsky's work, we argue that synchronically active phonological restrictions are needed to account for such cases, as attested sound changes would otherwise produce them. In short, these restrictions show that language's synchronic grammars are not merely the residue of the sound changes they have undergone, but actively limit the results of diachronic change.

2.1 Absolute restrictions

No language has epenthetic [k]. Epenthesis refers to a situation where an output segment has no corresponding input segment. In purely *output* terms, there is no language that has wordforms with the shapes […V-CV…] and […V-<u>k</u>-V…], where both forms contain a common morpheme (this particular pairing describes a case of medial onset-driven epenthesis; there are many other epenthesis environments which can be illustrated by other equivalent pairings).

Similarly, no language has place neutralization in stops that yields [k]. In input→output terms, there is no input segment that differs in major Place of Articulation from [k] (i.e. is not dorsal) and surfaces unfaithfully as [k], unless some incidental process such as assimilation or dissimilation interferes. Restricting attention to syllable-final PoA neutralization, in output-only terms there are no wordforms with the shapes […Vk-CV…] and […Vt-V…] where the leftmost morpheme is the same in both forms.

Neither of the assertions about [k] is new (e.g. Trubetzkoy 1939; Jakobson 1941; Lombardi 2002; de Lacy 2002, and many others). However, they have been challenged in some recent work, so apparent counter-examples will be examined in section 2.3.

2.1.1 *Synchronic explanation.* There are a number of synchronic theories that can account for a lack of epenthetic [k]. For example, an OT constraint system with an output constraint *[dorsal] and no constraint that favors dorsals over other places of articulation prevents epenthetic [k]. Putting aside the effects of assimilation and dissimilation, tableau (1) demonstrates this impossibility, using the theory of markedness constraints presented in de Lacy (2002, 2004). The motivation for epenthesis is the constraint ONSET, which requires a syllable-initial consonant; it interacts with faithfulness constraints in the ranking ONSET, MAX » DEP. No candidate violates any IDENT constraint as faithfulness to input features is irrelevant in epenthesis; output constraints will fully determine output feature specification. Consequently, the only constraints that matter here are those that distinguish between different places of articulation.

(1)

/a/	*dorsal	*{dorsal,labial}	*{dorsal,labial,coronal}
(a) <u>k</u>a	*!	*	*
(b) <u>p</u>a		*!	*
☞ (c) <u>t</u>a			*

The candidate with [k] epenthesis is harmonically bounded by the candidate with [t] epenthesis: there is no ranking of the constraints that produces epenthetic [k]; all rankings of these constraints favor the candidate with [t] epenthesis (Samek-Lodovici & Prince 1999). The crucial condition that makes this result valid is that there is no constraint that favors dorsals over all other places of articulation.

The added complexity with Place of Articulation is that glottals incur even fewer violations than coronals: i.e. [ʔ] doesn't violate any of the constraints in (1). Consequently, some languages have epenthetic [ʔ]. However, [ʔ] can be eliminated by other constraints (e.g. a ban on high sonority syllable margins – de Lacy 2002), so effectively promoting coronals to least marked status in some languages.

The same point can be made for neutralization. With an input /ap/ and a constraint that forces /p/ to surface unfaithfully, there is no ranking of the constraints above that will force /p/ to become [k]. Again, /p/→[t] is a harmonic bound for /p/→[k].

(2)

	/ap/	*{dorsal}	*{dorsal,labial}	*{dorsal,labial,coronal}
	(a) ap		*!	*
	(b) ak	*!	*	*
☞	(c) at			*

So, given a choice of epenthetic [k] or [t] and neutralization to [k] or [t], [t] will always win. It is uncontroversial that synchronic theories are capable of implementing such a restriction. Other proposals include using fixed rankings of constraints (Lombardi 2002) and lack of specification in an autosegmental approach (see Paradis & Prunet 1991 and references cited therein).

2.1.2 *Diachronic explanation.* Are phonological constraints like those just presented necessary? If languages with [k] epenthesis are unlearnable or very unlikely to survive diachronic transmission intact, epenthetic [k] would be unattested for purely performance reasons. Therefore, there would be no direct justification for proposing that the phonological component is unable to generate epenthetic [k] – it could be that there are constraints capable of producing epenthetic segments with any place of articulation – e.g. freely rankable *dorsal, *labial, *coronal. However, there is evidence that languages with [k]-epenthesis and neutralization to [k] are desirable from a performance point of view.

Epenthesis of [k] could develop in language change through misperception. Suppose a learner misperceives a vowel-vowel transition as having an intervening consonantal constriction. Exactly how this misperception happens is not of interest here – the fact that consonant epenthesis occurs means that (in a diachronic explanation approach) there must be some performance factor that motivates the learner to store the form with an inserted segment. What is of interest is how the learner decides which consonant to insert.

Some languages have stop epenthesis. Famously, Axininca Campa has epenthetic [t] (Payne 1981, Spring 1990). [t] is inserted at a variety of vowel-vowel junctures, exemplified in (3). Epenthetic consonants are underlined; reduplicants are double-underlined.

(3) *Axininca Campa [t] epenthesis (Payne 1981)*

(a) Root+suffix juncture

/i-N-√koma-i/ → [iŋkomati] 'he will paddle'

cf /i-N-√tʃ^{h}ik-i/ → [iɲtʃ^{h}iki] 'he will cut'

(b) Suffix+suffix juncture

/i-N-√tʃ^{h}ik-aː-i/ → [iɲtʃ^{h}ikaːti] 'he will cut again'

/√na-√RED-wai-ak-i/ → [nata-nata-waitaki] 'I will continue to carry it'

(c) Minimal word augmentation

/√t^{h}o/ → [t^{h}ota] 'kiss, suck'

c.f. /non-t^{h}o+RED/ → [nont^{h}onont^{h}o]

Why does Axininca Campa insert [t] and not [p], [tʃ], or [k]? From a learning point of view, the answer must be that [t] has some perceptual or articulatory property that makes it more desirable than these other options. The reason is possibly acoustic: coronals coarticulate less with surrounding vowels than labials and dorsals, and are therefore more readily perceptually separated from the flanking vowels. As such, they provide a clear break between vowels (assuming a possible motivation for inter-vocalic epenthesis is to minimize vowel overlap, perceptually).

Whatever the reason for [t] epenthesis, the issue is why [k] is not chosen in Axininca Campa, and in fact is *never* chosen in any language. There is a good deal of evidence that [k] has properties that can make it more desirable than [t] from a performance point of view. In diachronic change, there are many languages in which *t has become [k]: Lynch et al. (2002:54) note that "across the languages of the world the sound change *t* to *k* is hugely more common than *k* to *t*". This diachronic change is found in Hawaiian, Luangiua, colloquial Samoan and several other Oceanic languages (Blust 1990; Lynch et al. 2002:ch.4) as well as Fort Chipewyan Chipewyan (Haas 1968). The *t > k change happened in all phonological environments. For example, the Proto-Eastern Polynesian word for 'man, people' is *taŋata; in Hawaiian, it is [kanaka] (Clark 1976; Pukui & Elbert 1986).

Blevins (2004§5.4) discusses motivations for the *t > k change, observing that [k] has the longest VOT of all stops, and so proposing that "velars will sound, to the child, like good tokens of Category 1 stops [i.e. stops with long VOT and high amplitude]." There is an added subtlety here: Blevins observes that cases of *t > k have occurred when the parent language has no contrast between [t] and [k] (i.e. the change is diachronically non-neutralizing). This is probably irrelevant to their choice in epenthesis environments, especially because even languages without a [t]~[k] contrast do not epenthesize [k] (see discussion below).

An alternative motivation lies in the fact that dorsals' articulation, and consequently their acoustics, vary with the backness of adjacent vowels, while for labials and especially coronals the vowels' articulations and acoustics vary with the consonants' place instead. These facts show why a dorsal would be preferred over a coronal or a labial in a language where place contrasts are deteriorating: the dorsal is phonetically more placeless than the others. Learners would opt for it instead of the other places if what they perceived were simply the presence of an oral closure without strong place cues. In other words, failing [ʔ], [k] is the most placeless consonant available; for a learner wanting an epenthetic stop but wishing to deviate least from the perceived speech signal, [k] is an excellent option.[1]

[1] The motivations for [t] epenthesis and [k]/glottal epenthesis must be different. We suggested that the benefit in [t] epenthesis is that it minimizes vocalic overlap by having a consonant that is coarticulated least with the surrounding vowels; the motivation therefore means that the learner's desire to make the vowels as perceptually distinct as possible overrides the desire to accurately reproduce the perceived speech signal. In glottal and [k] epenthesis, the opposite holds. Glottals (particularly [h]) are significantly affected by their environment, and so is [k]. So, glottal/[k] epenthesis would separate vowels perceptually, yet allow minimal deviation from the perceived speech signal by continuing the vowel articulations as much as possible.

In any case, the diachronic change of *t > k indicates that there is some performance aspect of [k] that favours it over [t]. Therefore, [k] could be a better choice than [t] in epenthesis. As Blevins (2004:128) states "Coronal segments may have unique properties, but so do labials and dorsals". Therefore, there is no reason why epenthesis should consistently discriminate against [k].

A similar point can be made for neutralization. Suppose a learner heard a morpheme in two different environments: [at] and [a.t-o]. The learner might confuse [t] with [k] in the word-final environment with the consequence that there is now a pair of related wordforms [ak] and [a.t-o] in his/her grammar. If the misperception was general enough so that every [t] in onset corresponded to a [k] in a coda (while some onset [k]s corresponded to coda [k]s), in input→output terms the grammar would have to generate /t/→[k] neutralization.

The issue for neutralization, then, is whether misperception of [t] as [k] is possible solely in a limited environment such as the coda (or at least word-finally). This is evidently the case in Peruvian Spanish. Pre-consonantal stops in loanwords are realized as [k]: e.g. [peksi] < "Pepsi", [xikler] < "Hitler". José Elías (p.c.), a native speaker of the dialect, reports that a diachronic change of pre-consonantal [p] to [k] is also partially in place: many people do not distinguish between [akto] "act" and [apto] "apt", with both realized as [akto]. Similarly, words like *abstracto* "abstract" and *optional* "optional" are usually pronounced as [akstrakto] and [oksjonal]. Finally, Elías observes that when speakers of his dialect learn English as a second language, words like "street" tend to be pronounced as [estri], but word-final [t] can be realized as [k]: i.e. [estrik]. All of these cases can be ascribed to misperception of [t] as [k] in pre-consonantal and word-final environments, showing that *t > k is possible in just these positions. However, Peruvian Spanish does not have alternations that show /t/→[k]; there are no wordform pairs where a morpheme has [t] in an onset and [k] in a coda.

If misperception is responsible for *t > k in codas, then one would expect a situation in which a child learned a morpheme [at] as [ak], but retained the [t] in pre-vocalic environments [at-o], resulting in a /t/→[k] coda neutralization. However, no such system is attested.

In contrast, synchronic alternations involving coda neutralization to [t̠] do occur (e.g. Basque codas have /ogi-ʔapur/ → [ot.ʔapur] 'bread-crumb' – Hualde 1991). Neutralization to [t] occurs in restricted environments such as reduplicants, in Taiwanese (e.g. /k-k'ak-RED/ → [lak-k'it̠] 'crack open' – Li 1985) and Cantonese (/l-çap-RED/ → [lap-çit] – Yip 1982). Kim-Renaud (1986) reports that even /h/ surfaces as [t] in codas: e.g. /tʃoːh-k^{h}o/ → [tʃoːt-k^{h}o] 'good and', cf. [tʃoːh-ʉni] 'as (it's) good'.

In short, the argument here is that diachronic evidence shows that [k] has at least some properties that are more desirable than [t] in terms of performance; evidence from loanword adaptation and second language acquisition agrees. If performance properties are what influences sound patterns, then one would expect epenthesis of [k] and neutralization to [k]. However, neither phenomenon is attested. This, then, is a Competence-Performance mismatch: a sound pattern is favoured by performance factors, but the phonological component is unable to generate a grammar that reflects those performance pressures.

An important issue lurks in the preceding discussion: the majority of epenthetic elements are glides and glottals and most neutralization involves debuccalization. Is the lack of [k]-epenthesis therefore accidental, due to the infrequency of stop epenthesis generally? Languages with [t] epenthesis are few – Axininca Campa is a clear case; other languages include Māori (de Lacy 2003 and references cited therein) and Odawa Ojibwa (Piggott 1993; Lombardi 2002); some other languages have coronal epenthesis (of [n] and rhotics). In short, [t] epenthesis is not the most common type, but still attested. However, the lack of [k] epenthesis is still telling: there are situations where languages seem poised to have [k] epenthesis, but shy away from doing so, discussed in section 2.2.

2.2 Many diachronic rights never make a synchronic wrong

It is an easy matter to identify a series of natural diachronic changes that would produce an unattested language. Therefore, synchronically active phonological restrictions are needed to constrain diachronic change when it would produce a synchronically impossible grammar. The following argument builds on Kiparsky's (2004) work.

Kiparsky's argument refers to sonority-driven stress in Gujarati, described and analyzed in de Lacy (2002, 2006). Stress is usually penultimate (e.g. [apwána] 'to give', [ekóteɾ] '71'). However, it will fall on an [a] elsewhere if the penult is not [a] (e.g. [tádʒetəɾ], *[tadʒétəɾ] 'recently'). de Lacy (2002) proposes synchronic constraints that favour more sonorous stressed vowels over less sonorous ones, where the vowel sonority hierarchy is | a 〉 e,o 〉 i,u 〉 ə |. A diachronically-based account would not appeal to such a markedness hierarchy, and have no related constraints. Instead, Kiparsky suggests that in a diachronic account the "intrinsic acoustic prominence of sonorous vowels may be reinterpreted as stress in sound change". Suppose a language has stress on the word-final syllable. If a listener heard a word such as [pakí], [a]'s greater intensity and duration might mislead the listener into thinking that it bore stress: i.e. [páki]. In contrast, in a word such as [piká], [i] has less intensity and duration than [a], so there is no motivation for the listener to misperceive stress on [i]; the same applies to [pikí]. The resulting language would have 'sonority-driven stress' by having final stress in words like [pikí] and [piká], but non-final stress in words like [páki].

However, a natural sound change could easily change the stress facts. For example, Kiparsky observes that *a could become [ə] in all environments – stressed and unstressed – as it did in a sound change from Sanskrit. If this change happens in a daughter language of Gujarati, *[tádʒetəɾ] would become [tə́dʒetəɾ]. The problem with this form is that in synchronic terms a form like [tə́dʒetəɾ] has stress retracting to an antepenultimate schwa even though there is a more sonorous vowel – i.e. [e] – in penultimate position. No such stress system exists (see de Lacy 2004 for a typology). In short, a natural sound change results in a language that reverses the markedness relation between [ə] and mid vowels on the sonority hierarchy.

Diachronic mechanisms therefore cannot account for the resistance to changes that would affect the sonority of the vowel without also affecting its ability to bear stress. That is, the diachronic explanation approach could allow the *a>ə change without necessarily requiring stress to shift back to the penult, so losing the sonority condition.

In contrast, the Competence theories presented in Kenstowicz (1997) and de Lacy (2004) mean that it is impossible to construct a grammar in which schwa attracts stress away from a more sonorous vowel. These theories therefore predict that a sound change like *a>ə in a language with sonority-driven stress will necessarily alter the stress in words that have undergone the change: i.e. the *a>ə change must be simultaneous with the change in stress position to [tədʒétəɾ] – there is no stage in the language's history which would have [tə́dʒetəɾ].

A relevant case involving diachronic changes that ties in with the discussion of [k] epenthesis above is found in Eastern Polynesian languages. In the Eastern Polynesian language Māori, [t] is epenthesized to avoid a PrWd-initial onsetless syllable in suffixation. An example is given in (4a). Evidence that the passive is /ia/ and gerund /aŋa/ is found in environments where the underlying form of the root has a final consonant (b) and where the suffixes can syllabify with the preceding vowel (c) or can form a foot (d).[2]

(4) *[t] epenthesis in Māori, a conservative Central-Eastern Polynesian language*

	UR	Alone	Passive	Gerund
(a)	/mahue/ 'leave'	[mahue]	[mahue-t̲ia]	[mahue-t̲aŋa]
(b)	/hopuk/ 'catch'	[hopu]	[hopuk-ia]	[hopuk-aŋa]
(c)	/hika/ 'kindle'	[hika]	[hi.kai.a]	[hika-ŋa]
(d)	/toː/ 'drag'	[toː]	[toː-ia]	[toː-aŋa]

The passive and gerund undoubtedly existed in proto-Polynesian: the morpheme has reflexes in all the major subgroups – Tongic, Samoic, and Eastern Polynesian (Pawley 1966; Clark 1976§3.2ff) (in some languages it marks ergative rather than passive). All closely studied languages show allomorphy that is very similar to Māori's, so it is likely that epenthesis occurs in all these cases.

Proto-Central Eastern Polynesian (PCE) had *p, *t, and *k, and no *ʔ.[3] Consequently, PCE had a Māori-like situation: it had the passive and gerund, and very likely epenthesis of [t]. Subsequent developments of *k>ʔ occurred in Tahitian and Hawaiian, and *t>k in Hawaiian.

If PCE had [t] epenthesis, and *t became [k] in Hawaiian, one would expect the epenthetic consonant to be [k] in Hawaiian: i.e. PCE **alofat̲ia* should become HAW [alohak̲ia]. However, it in fact appears with a glottal stop: [alohoaʔ̲ia] (Pukui & Elbert 1986; Elbert & Pukui 2001§6.6.3). The conundrum is therefore why learners converted surface [t]s that had underlying /t/s to [k]s, while surface epenthetic [t]s were replaced by [ʔ]. The development of Hawaiian presents a prime situation where [k] could be epenthetic, and should be, but is not.[4]

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[2] For the conditions surrounding passive formation in Māori and evidence for epenthesis, see de Lacy (2003), which builds on a great deal of previous work cited therein.

[3] All daughter languages of Proto-Central Eastern Polynesian contrast three places of articulation for stops. The most common are [p t k].

[4] My thanks to 'Oiwi Parker-Jones for his help with Hawai'ian. As in Māori, underlying final consonants surface: /malah-ia/ → [malahia] 'ache', /paul-ia/ → [paulia] 'finish', /inum-ia/ → [inumia] 'drink'. When /ia/ can be incorporated into the same PrWd (i.e. with short bases), the [i] deletes post-vocalically: /ale-ia/ → [aléa] 'swallow' (Pukui & Elbert 2001:84). Bases that end in /a/ and long bases force the passive into its own PrWd, so epenthesis occurs: /wela-ia/ → [{wéla}{ʔ̲ía}] 'burn', /aloha-ia/ →[{alóha}{ʔ̲ía}] 'love'.

There is a straightforward synchronic explanation for the Hawaiian situation: the phonological component is not capable of generating a grammar with epenthetic [k], as in section 2.1.1. The only option for the Hawaiian speakers is to have epenthetic [ʔ].

In short, the development from Proto-Central Eastern Polynesian to Hawaiian presented a situation where [k] epenthesis is expected. The fact that Hawaiian has epenthetic [ʔ] instead of [k], whereas all non-epenthetic PCE [t]s became [k], indicates that there are restrictions on the result of language change: if the phonological component is unable to create epenthetic [k], the fact that [ʔ] is the epenthetic consonant in Hawaiian is explainable.

To be clear about the points made in the last two sections, we are not claiming that there is no role for diachronic explanation. Instead, we are asserting that no synchronic grammar can map underlying /t/ to surface [k], even though sound changes such as that which occurred in PCE can readily turn a *t into [k] . This sound change must therefore be the byproduct of misperception during learning and not of constraint reranking between the protolanguage and the present day. We discuss the division of labor between diachronic and synchronic explanation in section 4.

2.3 Evidence in epenthesis and neutralization

The preceding discussion is based on the proposal that [k] is never epenthetic or the output of place neutralization, unless processes such as assimilation and dissimilation interfere. A broader claim is that epenthesis and neutralization never produce dorsals. This claim is not new, and has been supported by typologies in Lombardi (2002) and de Lacy (2002, 2006). While epenthesis of and neutralization to glottal and coronal place of articulation is well attested, no cases involving dorsals or labials survive close scrutiny. The aim here is not to refute every counter-example but to identify general themes that lead to misidentification of dorsals as epenthetic and the output of neutralization. In some cases the putative epenthetic segment is a morpheme; other cases involve suppletion, and in many cases of nasal neutralization the output is phonologically glottal but is realized phonetically with velar or post-velar constriction.

The canonical source of evidence for epenthesis or neutralization comes from synchronic alternations that present similar phonotactic environments and apply across different morpho-syntactic contexts. For example, epenthetic [t] in Axininca Campa is found intervocalically in junctures between root+suffix and suffix+suffix, and to avoid sub-minimal words (see (3)). A case of epenthetic [k] would have to meet the same standard: it would have to occur in a well-defined phonological environment, and not be restricted to a limited morpho-syntactic environment.

The proposed cases of epenthetic [k] and neutralization to [k] do not meet this standard. Ostensibly ‘epenthetic’ segments are often morphemes, as in the Dravidian language Koɖava. Ebert (1996:9) reports that “euphonic [k] is inserted between roots ending in a vowel or [n] and a following [a]”, with the additional proviso that [k] voices after nasals. Examples are given in (5); [ɯ] is epenthesized after root-final consonants. Voiceless stops are banned after nasals, so accounting for the ‘euphonic’ [g] in (d) and (e).

(5) Koɖava euphonic [k] (Ebert 1996)

(a) /kuɖi-a/ → [kuɖika] 'let's drink' (c.f. [kuɖi] 'drink')

(b) /aɭa-ate/ → [aɭakate] 'without sitting down' (c.f. [aɭa] 'sit')

c.f. /ʌɭɯd-ata/ → [ʌɭɯdate] 'don't write', *[ʌɭɯdkate] (c.f. [ʌlɯdɯ] 'write')

(c) [koɖɯkate] 'do not give!'

(d) [tingadɯ] 'let him eat'

(e) [kanga] 'see you!'

The reasoning for treating [k] as epenthetic in [aɭakate] is presumably that (a) the bare root is realised as [aɭa], not *[aɭak], and (b) the suffix is [ate] (not *[kate]), as can be seen in [ʌɭɯd-ate].

However, a major problem with treating [k] as epenthetic relates to the environment that triggers its insertion. If [k] were truly epenthetic, it should be inserted for phonotactic reasons, such as a requirement that syllables have onsets. However, epenthesis after /n/-final roots is prosodically unnecessary: /kan-a/ could surface as *[ka.na] since this form satisfies ONSET; yet [g] is epenthesized: [kan-g-a]. Moreover, euphonic [k] is severely restricted in its distribution. It can only appear between a verb root and suffix. For example, /√kond-ɯn-avʎ̃/→[kondɯnavʎ̃], *[kondɯnkavʎ̃] 'one who killed'; /√ʌɭɯd-ɯn-ʌ/→[ʌɭɯdɯnʌ], *[ʌɭɯdɯnkʌ] 'I wrote'. In short, [k] does not behave like epenthetic consonants in other languages.

If [k] is a morpheme, a good deal of sense can be made of its distribution. Like other morphemes, /k/'s distribution can be morpho-syntactically restricted to the verb root + suffix juncture. With [k] as a (perhaps semantically empty) morpheme, the only challenge is to explain why it does not appear after *every* verb stem. The answer is that [k] is deleted when it would create a phonotactic problem. If there is a conflict between realising a verb root segment or [k], the verb root segment wins. For example, /ʌɭɯd-k-at̪e/ does not surface as [ʌɭɯdkat̪e] because the cluster [dk] is banned in Koɖava (as is [dg]). In contrast, /k/ does surface in /tin-k-ad/ → [tingadɯ] because homorganic NC̭ clusters are permitted on the surface.

Treating /k/ as a morpheme has ample precedent: many languages have semantically contentless 'thematic' morphemes (e.g. Attic Greek – Lupas 1972). The final reason for treating [k] as epenthetic in Koɖava is that glides are epenthesized in other hiatus situations (e.g. [elːi-j-uː] 'whereever', boɳɖu-w-aː] – p.9).

Other languages cited as having epenthetic dorsals are also amenable to reanalysis as morphemes; the tell-tale sign is their extremely restricted morphological distribution. For example, Marlett (1981:56) reports that [k] is epenthesized in Seri, but it only occurs after the [t] in the morpheme /tm/ and between the morphemes /t/ and /m/. Trigo (1988:59ff) argues that a [ɣ] that appears with certain reduplicants in Murut is epenthetic: e.g. /RED+aŋkup/ → [gaɣaŋkup] {*no gloss*}, /RED+insilot/ → [giɣinsilot] 'toothpick' (c.f. [bu-βulud] 'ridges in which tuberous crops are planted') (Prentice 1971:121). Note that [ɣ] reduplicates as [g] – this is due to the fact that voiced stops and voiced fricatives are in complementary distribution – voiced stops are forced to spirantize intervocalically. However, [ɣ]'s appearance is unpredictable. It only occurs with some vowel-initial reduplicants; others employ infixation: e.g. /RED+ulampoj/ → [ulalampoj] {*no gloss*},

/RED+indimo/ → [in<u>di</u>dimo] 'about five times'. If [ɣ] appears to satisfy a general prosodic requirement, it is difficult to see why it should only appear for some roots and not others. If [ɣ] is a morpheme that selects for particular morphosyntactic environments, its restricted distribution finds a ready explanation. In summary, while several languages have been claimed to have epenthetic velars, on close inspection the velars behave like morphemes rather than elements whose motivation is merely phonotactic (see Paradis & Prunet 1994 for relevant discussion).

The claim that there are no epenthetic [k]'s is couched in a broader claim that only coronals and glottals can be epenthetic (derivable from the constraints presented in section 2.1.1). The same problems as identified for putative epenthetic dorsals above can be seen for putative epenthetic labials. For example, Blevins (2003) proposes that Southern Oromo has an epenthetic [m]. It is found in reduplication of frequentative verbs: e.g. [<u>eːm</u>-eːge] 'he waited long', [<u>tam</u>-tataːniː] 'they stayed and stayed', [<u>fuːm</u>-fuːgite] 'she raised some children' (Stroomer 1987). However, the [m] has a highly restricted morphological distribution: it is only found with frequentative reduplication. The other factor is the phonological unnaturalness of the motivation for epenthesis – [m] is 'epenthesized' to fill a coda, acting against NOCODA. There are other reasons why coda epenthesis can happen (e.g. to make a stressed syllable bimoraic), but [m] appears even when a consonant from the base could have been copied: e.g. [<u>ham</u>-harkifte] 'he pulled frequently', *[<u>har</u>-harkifte]. The [m] in Southern Oromo has all the hallmarks of a morpheme – it is morphologically restricted and appears in phonologically unmotivated environments. Following Alderete et al.'s (1999) analysis of similar cases of reduplicant prespecification, the Southern Oromo [m] can be analysed as a morpheme: i.e. /RED-m-tataːni/ → [<u>ta</u>-m-tataːni].

Other cases of epenthesis are due to morphological suppletion. Suppletive morphemes have more than one underlying form; the underlying form that surfaces is selected for its output well-formedness in different environments (Mascaró 1996). An example is found in Buriat, which has been cited as having an epenthetic voiced dorsal (Poppe 1960; Rice 2004). The exact featural content of the 'epenthetic' consonant varies depending on the environment: it is velar [g] before front vowels, uvular [ʁ] between back vowels, and uvular [ɢ] after front and before back vowels (Poppe 1960). Poppe (1960:20) states that a [g]/[ʁ]/[ɢ] is epenthesized at stem-suffix junctures in certain situations of vowel hiatus. However, there are several problems with this proposal: (a) the 'inserted' consonant varies – it is sometimes dorsal and sometimes the palatal glide [j], (b) the phonological environment for the consonant is not consistent among different suffixes, and (c) deletion is the default strategy for hiatus-resolution, not epenthesis. The alternative account presented here is that the morpheme is suppletive: it has more than one underlying form, one of which happens to have a /g/.

The allomorphs of the reflexive possessive (RP) in (6) illustrate the issues (Poppe 1960:46). Harmony affects vowel quality.

(6) Buriat Reflexive Possessive (RP) allomorphy

(a) After consonants (except for c) = /aː/

[gar-aː] 'hand' [arteːl-eː] 'own *artel*' [nom-oː] 'book'

(b) After long vowels and diphthongs = /gaː/

[taχaː-ʁaː] 'hen' [gaχai-ɢaː] 'pig' [dyː-geː] 'younger brother'

(c) After suffixes ending in [ŋ] = /gaː/

[noχoi-ŋ-goː] 'of the dog' [aχ-iːŋ-gaː] 'of the elder brother'

(d) After a short vowel = /jaː/

[aχa-jaː] 'elder brother' [mori-joː] 'horse' [modo-joː] 'tree'

The proposal that the underlying form of the RP is /aː/ and [g] is epenthesized faces significant difficulties. One is why [g] is inserted in [taχaː-g-aː] 'own hen', but [j] appears in [aχa-j-aː] – note the almost featurally identical environments. The environment for epenthesis in (6b) and (6d) is inter-vocalic; however, the environment in (6c) is "after suffix-final [ŋ]", which is not clearly a natural prosodic environment to expect epenthesis. The [g] in (6b) and in (6c) is not clearly an epenthetic consonant; [j] has an equal claim to epenthetic status from (6d). Finally, other morphemes show quite different patterns: e.g. the genitive is [iːn] after stem-final short vowels, [ai] after stem-final consonants, [n] after diphthongs, and [gai] after long vowels (Poppe 1960:42ff).

The problems disappear when these different realizations are analyzed as suppletive allomorphs of the RP morpheme. The reflexive possessive can have two distinct underlying forms: /aː/ and /gaː/. The choice of allomorph depends on phonological conditions. After most final consonants, the /aː/ allomorph is chosen to avoid unnecessary violations of NOCODA (cf. [nom-oː], *[nom.goː]). After a suffix [ŋ], /gaː/ is selected because [ŋ] is an undesirable onset (i.e. [noχoi-ŋ.-goː], *[noχoi.ŋ-oː]). Vowel hiatus is banned in Buriat, so a consonant must appear after a short vowel; to avoid a [g], the /aː/ allomorph is selected and the short vowel breaks: i.e. the final /a/ in /aχa/ breaks into a vowel and glide counterpart [aj] resulting in [aχajaː]. While short vowels can undergo deletion, reduction, and a variety of other phenomena in Buriat, long vowels and diphthongs cannot. The high rank of faithfulness to long vowels prevents them from splitting to form an onset, so [gaː] is selected instead: [dyː-geː], *[dyːjeː].

To summarize, mono-segmental morphemes and suppletion can both introduce apparently epenthetic segments. However, these cases leave tell-tale signs: they happen in morphologically restricted environments, and may not occur in phonologically consistent environments. Valid cases of epenthesis show relative freedom in the morphological environments they appear in. For example, Axininca Campa's epenthetic [t] appears between any root and aas following suffix, between any suffixes, and after subminimal roots. In contrast, Koɖava's [k] only appears between a verb root and certain suffixes. Valid cases of epenthesis also appear for phonological reasons such as the need to avoid vowel hiatus, to increase the weight of stressed syllables, and so on.

Finally, cases have been cited that do not qualify as synchronic epenthesis. For example, Vaux (2003) claims that epenthetic [b ʃ ʒ] occur in various dialects of Basque, citing Hualde & Gaminde (1998). The data derive from a comparison of how different dialects respond to a vowel hiatus involving a stem + the singular determiner [a].

However, Hualde & Gaminde (1998:42) state that their data show "the output for each of the historical (or, if one wishes, 'underlying') sequences". They make no claim that the consonants are produced by synchronic epenthetic processes. In all cases, it is clear that the 'epenthesis' is influenced by its environment: [b] appears between only /u+a/ sequences in four of the dialects while [ʒ] appears between /i+a/ and there is no epenthetic consonant for the other hiatus situations (/a+a/, /e+a/, /o+a/). At the very least the restrictions indicate the effect of assimilation. Assimilation and dissimilation often affect the form of epenthetic consonants; they can assimilate in PoA and manner to adjacent segments (see Rosenthall 1994§4.2 for a general discussion). An example of both manner and PoA assimilation is found in Dakota: epenthetic consonants are [j] before the front (i.e. coronal) vowels [i e], and [w] before back (i.e. dorsal) vowels [a o u] (Shaw 1980:90). Similarly, the epenthetic consonant in Brahui assimilates to low back vowels in dorsality, voice, and continuancy, resulting in [ɣ] (e.g. [lumːa-ɣ-aːk] 'mother {masc.pl}') (Elfenbein 1997).

A number of analyses of loanwords have been argued to involve epenthesis of dorsals and labials. For loanword adaptation to involve true synchronic epenthesis, the foreign words would have to be correctly perceived (i.e. correctly mapped to the native language's phonetic categories), correctly mapped to phonological primes, and serve as an input to the grammar, which then modifies it. In other words, loanword adaptation involves synchronic epenthesis only if competence mechanisms are the only relevant factor in loanword adaptation. For example, Māori [kɪɾíhimète] is borrowed from English [kɹ́ɪsməs]. In the Māori borrower's grammar, was there ever a mapping in which /kɹ́ɪsməs/ → [kɪɾíhimète]? Work by Silverman (1992), Dupoux et al. (1999), Peperkamp & Dupoux (2003), and Peperkamp (in press) argues otherwise: foreign words can be misperceived, and this misperception may be a source of adaptation (also see Yip 2002; Kang 2003; Broselow to appear; section 3.2 below). So, errors can be made 'pre-phonology', and so phonological processes need not be responsible for every change in linguistic transmission. If this latter view is correct, loanword adaptations cannot be trusted to give evidence for competence mechanisms, and therefore for epenthesis. In no cases which we have observed are there synchronic alternations to support the claim that loanwords have true epenthesis (i.e. output segments with no corresponding input segment).

For neutralization, a common phenomenon that seems to support the idea that dorsals can be the output of neutralization is that nasals are often reported as neutralizing to [ŋ]: e.g. Huallaga Quechua (Weber 1989), Genovese (Ghini 2001:173), Kagoshima Japanese (Haraguchi 1984; Trigo 1988), Seri (Marlett 1981:20), Yamphu (Rutgers 1998), Makassarese (Aronoff et al. 1987), and the San Marcos dialect of Misantla Totonac (MacKay 1994:380). However, de Lacy (2002, 2006) argues that close examination of several cases shows that the reported 'ŋ' is phonologically glottal, called [N] here; this proposal is similar to Trigo's (1988) analysis of such nasals as placeless (at least for part of the derivation). For example, in the Nepalese language Yamphu oral stops become [ʔ] before another glottal: e.g. /mo-dok-ha/ → [modo<u>ʔh</u>a] 'like those' (p.48), *[modo<u>kh</u>a]; /læː<u>t-h</u>e-ma/ → [læː<u>ʔh</u>ema] 'to be able to do' (Rutgers 1998:48). Nasal stops also assimilate to glottals, and the result is reported as 'ŋ': /pen-ʔi/ → [peNʔi] 'he's sitting';

/hen-heː-nd-u-æn-de/ → [heNheːndwende] 'can you open it?' (p.44). If Yamphu [N] is really velar [ŋ], this assimilation is inexplicable – one would expect stop assimilation to result in [k] before [ʔ]: i.e. /læːt-he-ma/ → *[læːk.he.ma]. From a broader perspective, assimilation of PoA always results in agreement of PoA features. Therefore, the nasal that appears before [h] in Yamphu must be phonologically [glottal].

Gutturals – glottals, pharyngeals, and uvulars – can force an adjacent vowel to have a retracted tongue root ([RTR]). For example, Arabic verb stems must have a low vowel next to a guttural in the imperfect: e.g. [faʕal] 'do {perfect}', [ja-fʕal] 'do {imperfect}', *[faʕul] cf. [katab] 'write {perfect}', [jaktub] 'write {imperfect}' (McCarthy 1994). The generalization holds for uvulars [χ ʁ], pharyngeals [ħ ʕ], and glottals [ʔ h], but not for velars. Similarly, Miogliola's vowels must be RTR when followed by a tautosyllabic moraic glottal nasal [N] (Ghini 2001:ch.4). Coda [N] is usually non-moraic (e.g. [fe$^{\mu}$N] 'fine'), but becomes moraic when a consonant follows (e.g. [fɛ$^{\mu}$N$^{\mu}$d͡z]) or after a stressed vowel. The only vowels allowed before moraic [N] are the RTR vowels [ɛ œ a ɔ]; ATR [i y e æ ɑ o u] are not allowed. As velars never cause vowels to lower, it must be the case that Miogliola [N] is post-velar – i.e. glottal.[5]

[N] has the same distribution as other glottals. It is common for glottals to be banned from onset position. For example, Chamicuro and Macushi Carib do not allow [h] in onsets (Parker 1994), and Buriat's [N] is allowed only in codas while [n] and [m] appear in onsets (Poppe 1960). If Buriat [N] is actually a velar [ŋ], it is difficult to explain why [k], [g], and [x] can all appear in onset position but [N] cannot. In fact, there is no language that bans velars like [k g x ɣ] in onsets but allows them in codas; glottals excepted, every PoA that is allowed in codas is also allowed in onsets (Goldsmith 1990, Beckman 1998, de Lacy 2006§3.2.3). So, the fact that Buriat's 'ŋ' is only allowed in codas indicates that it is really glottal [N]. Miogliola [N] has the same distribution, consistent with its vowel-lowering behaviour mentioned above (Ghini 2001§5.1).

[N] also alternates with other glottals. For example, [ɦ̃] appears in Aguaruna onsets, but is realized as [N] in codas: [suŋkuN] 'influenza' c.f. [suŋ.ku.ɦ̃-ãn] 'influenza+accusative' (D. Payne 1990:162). If this [N] is really velar [ŋ], the motivation for the alternation is unclear; alternations involving [h] in other languages produce other glottals or coronals (e.g. Korean – Kim-Renaud 1986), not velars.

To summarize, a clear case of an epenthetic dorsal or neutralization to dorsal PoA has yet to be identified. Putative examples can be reanalyzed as involving morphemes, suppletion, glottal nasals, or assimilation.

3. Active synchronic restrictions

In extreme diachronic explanation, there is no role for active synchronic restrictions. The alternations and phonotactic restrictions in languages' synchronic phonologies are due to learning restrictions, and so the resulting language has essentially arbitrary statements of

[5] An alternative analysis is that [N] nasalizes the preceding vowel, and nasal vowels must be [–high]. However, Ghini does not report any such nasalization, and vowels do not lower before other nasals (e.g. [ɪɲ.d͡ʒénw] 'naïve', *[ɪɲ.d͡ʒɛ́nw]).

sound distributions which are encoded in the representations of and paradigmatic relations between morphemes and words.

However, in this section we argue that a sound change's phonetic motivation can remain active in the resulting language's synchronic phonology after it has been phonologized. We also describe a sound change that is optimizing. Turning back to synchrony, we show that restrictions on sounds' distributions are psychologically and even neurophysiologically active and also distinguishable from statistical generalizations across the lexicon. Finally, we demonstrate that language learning is determined by innate predispositions.

3.1 Phonetic persistence

Tonogenesis in Athabaskan provides clear evidence that a sound change's phonetic motivation remains active even after the sound change has been phonologized. The persistence of the phonetic motivation does not necessarily follow from an extreme diachronic explanation: although there are phonetic motivations for language change, there is no need for those motivations to persist *after* the change has been phonologized.

In Proto-Athabaskan, glottalic and non-glottalic stops, affricates, nasals, and glides contrasted at the ends as well as the beginnings of stems (Krauss 2005). Conservative languages spoken in Alaska and along the Pacific Coast in northern California and Oregon maintain these contrasts at the ends of stems but in most of the rest of the family, this contrast has been replaced by a tone contrast on the preceding vowel in stems ending in stops and affricates – henceforth just "stops". This contrast has also been lost in stem-final stops in a few peripheral Alaskan languages without being replaced by a tone contrast.

The development of tone from an earlier laryngeal contrast in an adjacent consonant is an extremely common sound change (Hombert, Ohala, & Ewan 1979), particularly in the language families of East and Southeast Asia. It can occur because one of the phonetic correlates of a laryngeal contrast in consonants is differences in the fundamental frequency (F0) of adjacent vowel. These F0 differences become tone contrasts in the vowels and replace the original laryngeal contrast when the other phonetic correlates of that contrast are lost from the consonants.

Two properties of tonogenesis in Athabaskan are relevant to the argument that a sound change's phonetic motivation can remain active after it's been phonologized. First, some Athabaskan languages have developed a high tone in stems that originally ended in a glottalic stop and a low tone in stems that ended in a non-glottalic stop, while in others the opposite tones have developed in these two kinds of syllables. Second, glottalic and non-glottalic sonorants still contrast stem-finally in the daughter languages, as does glottal stop with its absence, and the same tones have always developed in these stems as in stems that ended in glottalic and non-glottalic stops in the protolanguage. Because these consonantal contrasts have been retained, the tones that develop on preceding vowels are redundant rather than contrastive in stems ending in sonorants, unlike stems ending in stops.

Kingston (2005) argues that both high and low tones can develop directly from the original glottalic stops because these consonants may be pronounced in two different ways. Glottalic consonants are distinguished from non-glottalic ones by a constriction of the glottis that is tight enough to curtail or even cut off air flow through the glottis. The glottis is closed by contracting the interarytenoid and lateral cricoarytenoid muscles while relaxing the posterior cricoarytenoid muscles, and the constriction is tightened by the forceful contraction of the thyroarytenoid muscles, which stiffens the inner bodies of the vocal folds and causes them to press firmly against one another. If this is all the speaker does, the voice quality of adjacent vowels will be creaky and its F0 will be low because the folds' outer covers remain slack. However, if the speaker also contracts the cricothyroid muscle at the same time, the folds' outer covers will be stretched and the voice quality in the adjacent vowel will be tense and its F0 high instead. The two muscles can be contracted independently because they are innervated by separate nerves: the superior laryngeal nerve innervates the cricothyroid and the recurrent laryngeal nerve the thyroarytenoid.

Thus, in languages where a low tone developed from an original glottalic stop, speakers contracted the thyroarytenoid alone when pronouncing glottalic stops, while in languages where a high tone developed instead, they also contracted the cricothyroid. Whether just the thyroarytenoid is contracted or the cricothyroid is, too, doesn't appear to depend on any other articulation, whether laryngeal or oral, so it appears to be the speaker's choice to contract just one of these muscles or both of them. The large and growing body of evidence of language-specific phonetic differences shows that speakers commonly exercise the freedom to make such choices.

What's particularly interesting about these choices is their persistence in the history of each of the Athabaskan languages that have developed tone, for the choice speakers of a particular protodialect made about how to pronounce the glottalic stops at a time when these stops still contrasted with non-glottalic stops continues to determine how the glottalic sonorants are pronounced by their present-day descendents. This persistence is particularly striking since nothing appears to stand in the way of speakers changing how they pronounce the glottalic sonorants sometime between the present day and the time when the contrast was lost in the stops and transferred to tone in the preceding vowel. That is, if it were once possible to choose whether to constrict the glottis in such a way as to either lower or raise F0, then it should have remained possible to do so, and for speakers of a tonal Athabaskan language to adopt the pronunciation of glottal constriction that has the opposite effect on F0 and tone in the preceding vowel sometime during that language's subsequent history. The result would be that stems which originally ended in glottalic stops in the protolanguage would have one tone, while those that end today in glottalic sonorants or glottal stop would have the opposite tone. This has never happened. It has not happened because when the sound change was phonologized, the phonetics of the pronunciation of glottal constriction were, too. Doing so has constrained glottalic sonorants and the glottal stop to be pronounced in the same way throughout the subsequent history of each tonal Athabaskan language as its own glottalic stops were when the sound change was actuated.

To summarize, the phonological restriction on tone and its phonetic motivation necessarily co-exist. Their co-existence is expected in a theory where synchronic phonological constraints are directly related to phonetic conditions. In an approach to sound change that severs the product of sound change from its phonetic motivatioon once it's been phonologized, there is not only no need to maintain the link between the phonologized output and its phonetic motivation but indeed an overt denial that any link can be maintained.

3.2 An optimizing sound change

Proponents of diachronic explanations have also claimed that sound change is never optimizing. Undoubtedly in many cases it is not, but in at least some cases it is. The case discussed here – the on-going split in the pronunciation of the diphthong [aɪ] in southern American English – is particularly interesting because the optimization apparently conflicts with well-grounded phonetic expectations. As we'll see, there are independent and conflicting phonetic reasons to expect exactly this development.

As documented in Thomas (2000) and Moreton (2004), [aɪ]'s pronunciation is shifting toward a more extreme diphthong – F1 is lower and F2 is higher in its offglide – before voiceless obstruents and toward a monophthong elsewhere.[6] Thomas and Moreton also show that listeners are significantly more likely to identify a following obstruent as voiceless when diphthongization is more extreme.

What is striking about this split is that its direction is exactly opposite what is expected on phonetic grounds. In the transition to voiceless obstruents, formants are frequently cut off early because the glottis opens and voicing ceases before the oral constriction is complete. Voicing's continuation into the oral constriction of voiced obstruents permits formants to reach more extreme values at the end of a vowel preceding such consonants. An important consequence is that F1 is typically higher at the end of a vowel before a voiceless than a voiced obstruent; this difference is in fact so reliable that listeners use it as a cue to the obstruent's voicing (Fischer & Ohde 1990). How then can diphthongal offglides become acoustically more extreme precisely in the context where the phonetics leads us to expect they'd become less extreme, while becoming less extreme in the context where the phonetics encourages more extreme acoustic values?

After considering and rejecting a number of alternatives, Moreton offers the following answer to this question: diphthongs are hyper-articulated before voiceless obstruents because voiceless obstruents themselves are hyper-articulated; specifically, they are produced with more extreme and faster articulations than voiced obstruents, and their hyper-articulation spreads to the preceding vowel. The spreading of hyper-articulation compensates for and indeed undoes the cutoff of formant frequencies by the early cessation of voicing before voiceless obstruents. Hyper-articulation is also optimizing in that it not only maintains but exaggerates the voicing contrast. In doing so, it reflects the high ranking of the phonological constraint IDENT[voice] in circumstances where it appears the language may be on the verge of transferring the burden of conveying this contrast from the consonant to the preceding vowel.

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[6] Moreton reports that the offglides of other complex nuclei [ɔɪ, aʊ. ɛɪ] are also more extreme before voiceless obstruents than elsewhere, but these nuclei are not (yet?) becoming monophthongs in southern American English when they don't occur before voiceless obstruents.

3.3 Active synchronic constraints

In this section, we turn to the evidence that the constraints proposed to account for synchronic sound patterns are psychologically and even neurophysiologically active. This evidence shows that they are also not mere statistical generalizations across the lexicon. These constraints influence the on-line categorization of sounds and the syllabification of segment strings.

Moreton (2002) presented listeners with two sets of stop-sonorant-vowel stimuli. The two sets differed in only the stop, which ranged incrementally from [d] to [b] in the first set and from [g] to [b] in the second. In both sets, the sonorant ranged incrementally between [l] and [w]. Listeners identified the members of the first set as beginning with "gl", "gw", "dl", or "dw" and the members of the second set as beginning with "gl", "gw", "bl", or "bw". One of the responses to the first, "dl", is an onset that is phonotactically prohibited in English, while one of the responses to the second, "bw" is an onset that is statistically very rare but not prohibited by any constraint.[7] Moreton showed that both [dl] and [bw] have a zero frequency of occurrence as onsets in the 18.5 million words in the London-Lund corpus of written and spoken British English. On statistical grounds alone, then, "dl" and "bw" responses are predicted to be equally disfavored. The results were quite different. In responses to the first stimulus set, if listeners identified the stop as "d" they were more than three times less likely to identify the sonorant as "l" than if they identified the stop as "g". In responses to the second stimulus set, they were actually more than one and a half times more likely to identify the sonorant as "w" if they identified the stop as "b" rather than "g", contrary to what might be expected if statistical rarity inhibited "b" responses.[8] These results show that it is possible to distinguish a zero that is the result of a phonotactic prohibition from one that is the consequence of an accidental gap.

What is the source of speakers' knowledge of this distinction? In a theory with a non-trivial phonological component, it can be ascribed to innate an synchronic constraint that bans [dl], but not [bw]. In contrast, for an extreme diachronic explanation the zero frequencies of [dl] and [bw] have the same source, the process of diachronic transmission. Each occurs with zero frequency because in the course of the language's history nothing has caused either to arise. There is no ongoing psychologically active constraint that restricts either structure, and nothing – not even lexical frequency – that can account for the difference in their preference.

In a followup experiment in which a vowel was inserted before the stop-sonorant-vowel string in the first stimulus set, the bias against "l" when the stop was identified as "d" disappeared. This result shows that the difference between [dl] and [bw] obtained in the first experiment is not merely a perceptual interaction between the two segments but a

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[7] This generalization is supported by the fact that loanwords with [bw] exist in some English dialects (e.g. [bwana] 'boss'), but no dialect has loanwords with initial [dl].

[8] Hallé, Segui, Frauenfelder, & Meunier (1998) show that French listeners find it difficult to distinguish [d, t] from [g, k] before [l], and Hallé, Best, and Bachrach (2003) show that Hebrew listeners, whose language permits [dl] and [tl] in onsets, are much better at this discrimination than French listeners. This difference shows that the constraint against onset [dl, tl] is low-ranked relative to the constraint preserving the contrast between coronals and dorsals in Hebrew but high-ranked in English and French.

consequence of their syllabification. The sequence [d.l] is perfectly acceptable in English if the two segments are not both in the onset.

Other phonotactic prohibitions have also been shown to influence on-line phoneme categorization (Moreton 1999; Moreton & Amano 1999; Coetzee 2004, 2005). In all these cases, it is possible to debate the nature of the phonotactic constraint that determines listeners' responses, but the results of these experiment leave little doubt that their responses are directly governed by quite specific constraints on how sounds may legally combine and not on statistical generalizations across the lexicon. The constraints are specific both to sequences they prohibit and the languages in which they apply.

Results reported in Kirk (2001) show that on-line syllabification is also directly influenced by phonological knowledge of language-specific allophony, phonotactic constraints, and general constraints on the affiliation of segments to syllables. Kirk used the word-spotting task, a procedure in which positive trials consist of strings of sounds that are not themselves words but which contain words. For example, the stimulus in a postive trial might be [vukwajn], which contains the word *wine* [wajn]. The remainder of the string in such trials, here [vuk], is also not a word. In negative trials, no contiguous subset of the string's segments is a word. Listeners must determine as quickly and as accurately as possible whether the string contains a word, and if so say it aloud. This task is a fundamentally more naturalistic probe of how listeners parse the signal than either the syllable monitoring task (Mehler, Dommergues, Frauenfelder, & Segui 1981; Cutler, Mehler, Norris, & Segui 1986) or having subjects divide words into syllables (Treiman 1983; Treiman & Danis 1988; Treiman & Zukowski 1990) because it involves finding words in longer strings, something listeners do ordinarily when listening to speech.

In demonstrating that English allophony influences on-line syllabification, Kirk presented listeners with two variants of stimuli like the example, one in which the voiceless stop before the word was aspirated and the other in which it was unaspirated, [vuk^{h}wajn] vs [vukwajn]. Because the embedded word always began with a non-nasal sonorant, English phonotactics permit listeners to syllabify the [k^{h}] or [k] with the following segment. Kirk predicted that they would be more likely to syllabify the aspirated [k^{h}] with the following sonorant than the unaspirated [k], because voiceless stops in English are consistently aspirated only at the beginnings of syllables. If [k^{h}] is syllabified with the sonorant and [k] is not, and syllabification determines where listeners think words begin, then *wine* should be harder to spot after [k^{h}] than [k]. As predicted, listeners detected *wine* much slower and less accurately when the preceding voiceless stop was aspirated than unaspirated.

To determine whether phonotactics influenced syllabification independently of allophonics, Kirk pitted the two against one another by comparing the speed and accuracy with which listeners detected a word beginning with [l], e.g. *lunch*, following aspirated coronal vs. non-coronal stops, in such strings as [vit^{h}lʌntʃ] vs [vik^{h}lʌntʃ]. On the one hand, the phonotactics prohibit a coronal stop from appearing in an onset before [l], which predicted that *lunch* should be detected faster and more accurately after [t^{h}] than [k^{h}] because the strings would be syllabified differently, [vit^{h}.lʌntʃ] vs [vi.k^{h}lʌntʃ]. The preceding stops' aspiration, on the other hand, indicates that they're in the syllable onset, which predicts that *lunch* should be detected equally slowly and inaccurately after [k^{h}] as well as [t^{h}], because the strings would be syllabified identically, [vi.t^{h}lʌntʃ] and [vi.k^{h}lʌntʃ]. The findings showed clearly that phonotactics take priority over allophonics in determining syllabification: listeners detected *lunch* significantly faster and more accurately after [t^h] than [k^h].

Kirk's remaining experiments showed that listeners first parse segments into syllables on their way to recognizing words even when neither allophony nor phonotactics dictates a particular syllabification. They use constraints requiring that onsets be maximized and that segments affiliate with an adjacent stressed syllable to group segments exhaustively into syllables. Onset maximization was tested by comparing the speed and accuracy with which listeners detected words such as *smell* vs. *mad* in the strings [vusmɛl] and [vusmæd]. As predicted if listeners maximize onsets, *mad* was detected slower and less accurately than *smell*. To ensure that this result reflected onset maximization and not some difference between the targets, Kirk ran another experiment in which listeners had to detect words beginning with [+voice] stops such as *bag* in the strings [zebæg] vs. [zesbæg]. As [+voice] stops are typically pronounced voiceless unaspirated at the beginnings of words in English, a preceding [s] could syllabify with them without violating the language's phonotactics. Again, the results showed that listeners maximize onsets: *bag* was detected slower and less accurately in [zesbæg] than [zebæg].

In the experiments testing the effects of attraction to stress of single intervocalic consonants, words such as *east* would be presented in the strings [gwəvˈist] vs. [gwˈɛvˌist].[9] If [v] is attracted into the main stressed syllable [gwˈɛv.ˌist], a syllable boundary coincides with the left edge of the word [ist] *east*, and would make the word easier to pick out if listeners first parse strings into syllables in hypothesizing where words begin and end. In contrast, in [gwə.vˈist] the lack of a syllable boundary before *east* leaves no phonological cues for the morpheme's edge. As predicted, *east* was detected slower and less accurately in [gwə.vˈist] than in [gwˈɛv.ˌist]. The same pattern of results was obtained when the initial stressed syllable contained a tense rather than a lax vowel, [gwˈevˌist] rather than [gwˈɛvˌist]. This finding shows that it was stress rather than the prohibition against lax vowels in open syllables that attracted the consonant into the first syllable in such strings. It also shows that attraction to stress, i.e. weight to stress, takes priority over a constraint requiring that syllables have an onset.

Using quite different techniques, Dupoux and his colleagues present equally compelling evidence that native language phonotactics determine on-line syllabification, even to the point that they cause listeners to hear segments that aren't present in the signal (Dupoux, Kakehi, Hirose, Pallier, & Mehler 1999; Dehaene-Lambertz, Dupoux, & Gout 2000; Dupoux, Pallier, Kakehi, & Mehler 2001). These studies rely on phonotactic differences between French and Japanese in what consonants can cluster together: French but not Japanese permits such clusters as [bz] or [gm]. The phonotactic constraint is, moreover, an active component of Japanese's grammar in that it forces epenthesis of [ɯ] to break up such clusters in loanwords.

Dupoux et al. (1999) had French and Japanese listeners respond whether the vowel [ɯ] was present in a continuum of stimuli from [ebɯzu] to [ebzo], where the [ɯ] was progressively shortened down to nothing. French listeners' "yes" responses decreased monotonically from 100 down to 0% as the vowel shortened, while Japanese listeners' "yes" responses did not drop below 70% even for the stimuli from which the entire vowel

[9] The stress marks are placed before the vowels rather than the syllables precisely because the syllabification is at issue in these strings.

had been removed. The phonotactic prohibition in Japanese against clusters such as [bz] creates a perceptual illusion: where a vowel must occur, Japanese listeners hear one, even if there's actually no vowel there at all. Japanese listeners were also much poorer than French listeners at speeded discrimination of stimuli which differed in whether a vowel intervened between two consonants, e.g. [ebɯzo] vs. [ebzo].

Dupoux et al. (2001) showed that these effects reflect a phonotactic constraint and not just the facts that the vowel [ɯ] occurs between two consonants in many Japanese words, particularly in many loanwords. Listeners transcribed and made lexical decision judgments for non-word strings containing illegal CC clusters that either have a single lexical neighbor with [ɯ] between the consonants (e.g. the string [sokdo] has the neighbor [sokɯdo] "speed"), or a single lexical neighbor with some vowel other than [ɯ] (e.g. the string [mikdo] has the neighbor [mikado] "emperor"). If the illusion simply reflects the possibility that the string is a word once a vowel has been added to it, then listeners should transcribe [mikdo] with its missing vowel and identify it as a word as readily as [sokdo]. If they instead imagine that an [ɯ] is present because the grammar supplies it as the epenthetic vowel to repair violations of a constraint banning clusters such as [kd], then they should instead identify [sokdo] as a word more often than [mikdo]. They should, moreover, transcribe [mikdo] with [ɯ] between the two consonants as often as [sokdo], even though [mikɯdo] isn't a word. In conformity with this alternative prediction, listeners inserted [ɯ] into their transcription of [mikdo] strings nearly as often as into [sokdo], despite the absence of any corresponding word [mikɯdo]. They also they identified [mikdo] strings far less often as words than [sokdo] strings, despite the existence of the word mikado. Finally, response times in lexical decision to [mikdo] strings were as slow as those to the corresponding non-word strings [mikɯdo] while those to [sokdo] strings were as fast as those to the corresponding word strings [sokɯdo]. All these results support the phonotactic constraint explanation and not the lexical similarity explanation. The phonotactic constraint must introduce the illusory vowel before any lexical item is activated because [ɯ] was inserted into the transcriptions of [mikdo] strings nearly as often as into [sokdo] strings, despite the lexical competition from the [a] in [mikado], and [mikdo] was thus identified as a nonword as slowly as [mikɯdo].

This conclusion is driven home by the behavioral and neurophysiological data reported by Dehaene-Lambertz et al. (2000). French and Japanese listeners heard four repetitions of strings such as [igɯmo] or [igmo] followed either by a fifth repetition of the same string or by a string that differed in the presence or absence of the vowel [ɯ] between the two consonants. Listeners labeled the fifth stimulus as the "same" or "different" from the first four as quickly as possible. French listeners responded correctly to different trials far more often than Japanese listeners, 95.1% vs. only 8.9%. Responses were also significantly slower in different trials compared to same trials for the French but not the Japanese listeners. Like the accuracy data, the absence of any RT difference in the Japanese listeners' responses suggests they usually didn't notice that the fifth stimulus was different in the different trials.

An event-related potential (ERP) obtained in the earliest interval following the moment when the fifth stimulus deviated from the four preceding stimuli on different trials was significantly more negative in voltage than that obtained on same trials for French but not Japanese listeners. Dehaene-Lambertz et al. interpret this ERP as arising when the brain detects the sensory mismatch between the different stimulus and the sensory memory of the preceding reference stimulus.[10] Just as the behavioral response shows that the Japanese listeners seldom consciously heard any difference between [igɯmo] and [igmo] strings, this early ERP shows that their brains didn't notice any difference either.

Kirk's results and those of Dupoux and his colleagues show that on-line syllabification is as strongly influenced by language-specific constraints as is on-line categorization. Like the categorization results reported by Moreton and others, they also cannot be attributed to statistical generalizations across the lexicon. All the knowledge about their languages that the participants in these experiments used in producing these results instead takes the form of psychologically active categorical constraints in their synchronic phonologies.

3.4 Innateness

A pattern or behavior can be identified as innate if it emerges when there is no external stimulus or model, as does vocal babble in deaf infants. Optimality Theory's account of language learning assumes that in the initial ranking all markedness constraints outrank all faithfulness constraints (e.g. Smolensky 1996; Davidson, Jusczyk, & Smolensky 2004), and that the learning of a particular language is a process of demoting specific markedness constraints below competing faithfulness constraints. Demonstrating that markedness constraints are ranked above faithfulness constraints for infants who are too young to be learning the ambient language's sound patterns would confirm the hypothesis that this ranking is innate, assuming that markedness constraints do outrank faithfulness constraints in the initial state. This result would also demonstrate that the particular constraints observed to be ranked in this order are themselves innate.

Davidson et al. (2004) have demonstrated both these points. They used the head-turn preference procedure to determine whether 4.5 month old infants prefer to listen to sequences of the form *... on pa ompa ...* or *... on pa onpa ...*, where the assimilation in place of the nasal in *ompa* obeys a markedness constraint requiring a nasal to agree in place with a following stop, while its failure to assimilate in *onpa* instead obeys a conflicting faithfulness constraint preserving the nasal's place of articulation. The results show that infants at this age significantly prefer to listen to the sequence than obeys the markedness rather than the faithfulness constraint. The fact that there is any preference at all shows that infants are capable of distinguishing between [m] and [n] in this context, and thus undermines any attempt to explain assimilation as a failure to detect the nasal's place of articulation (cf. Blevins 2004:118-119). This preference also cannot be a result of these 4.5 month old infants' having learned that nasals assimilate in place to following consonants in the ambient language, because 6 month old infants do not significantly prefer to listen to sequences that conform to the ambient language's phonotactics; such preferences don't emerge before 9 months (Jusczyk, Friederici, Wessels, Svenkerud, & Jusczyk 1993; Jusczyk, Luce, & Charles-Luce 1994; Jusczyk, Houston, & Newsome

[10] This ERP's timing and cortical topography closely resembles the mismatch negativity (MMN) obtained whenever the brain detects that the current stimulus is auditorily different from the immediately preceding one.

1999; Mattys & Jusczyk 2001). This result confirms the prediction that markedness constraints are ranked above competing faithfulness constraints in the initial state as well as the hypothesis that this ranking and the constraints themselves are innate.

Certainly, this initial ranking need not be preserved in the grammar of the ambient language that is eventually learned. Nonetheless, in the most parsimonious account, the grammar is composed of the same constraints as govern infants' responses at 4.5 months, and no more markedness constraints have been demoted below faithfulness constraints than is necessary to account for the surface distributions of sounds in that language.

3.5 Summary

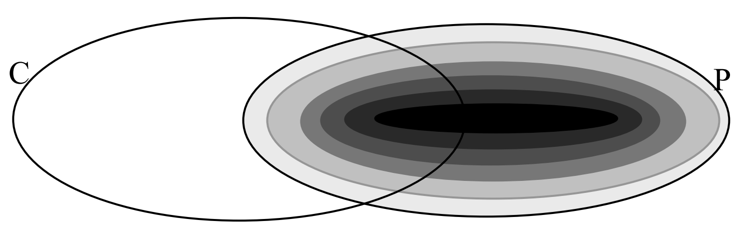
In this section, we have presented positive evidence in support of the claim that there are active synchronic restrictions: a sound change's phonetic motivation remains active after it's been phonologized, sound changes can be optimizing in a way that reflects synchronic constraints, the constraints attributed to synchronic phonologies are psychologically and neurophysiologically active, these constraints are not mere statistical generalizations across the lexicon, and the constraints of which synchronic phonologies are composed and their initial ranking are innate. This evidence points to a rich, on-going interchange between the phonetics and phonology and to a contentful synchronic phonology.

4 The roles of diachronic and synchronic explanation

We have argued that there are non-trivial phonological constraints that restrict the form of synchronic grammars; we therefore reject extreme versions of diachronic explanation which seek to ascribe every observed markedness asymmetry to learnability differences. However, we accept that diachronic change influences languages; in this section, we discuss the roles of both diachronic and synchronic change.

In diagram (7), C ∪ P contains all the grammars that the phonological component could generate if it was given a free hand (while abiding by the restrictions of its formatives' and relations' formal properties). C contains grammars that the phonological component can generate. P contains all grammars that are learnable. P contains subsets – the darker portions contain grammars that are more easily learnable (in an appropriate sense), and so are more likely to survive transmission intact than those grammars in lighter areas.

(7)



In this article we have argued that P ∩ C' has content: i.e. there are grammars that are favored in diachronic change and transmission but cannot be generated by the phonological component. These include grammars with epenthetic [k] and neutralization to [k]. In contrast, an extreme diachronic explanation approach would hold that P ∩ C' is empty: the phonological component can generate any output, and the grammars that are diachronically favored are a proper subset of C.

It is uncontroversial that C ∩ P' is non-empty: there are grammars that can be generated by the phonological component but are unlearnable or cannot survive inter-generational transmission. For example, all extant phonological theories are capable of producing a grammar with just two segments and a CV syllable structure, but no such language exists. Such a language would be a resounding failure for functional reasons; given the limited distinct combinations, words would have to be very long – human memory limitations in parsing would result in an extremely limited vocabulary. Functional pressures rule such a language out, so there is no need for a theory of phonological competence to do so.

A more striking example involves onsetless syllables. Blevins (2003) observes that Optimality Theory with a constraint ONSET ("Incur a violation for every onsetless syllable") predicts a language in which every syllable begins with a consonant. Such a grammar would have the ranking ONSET » MAX or DEP, and no constraint that favored onsetless syllables (e.g. ALIGN-L(Root,σ) – McCarthy & Prince 1993) would block ONSET. However, Blevins argues that there is no such language. Is this a problem for OT with an ONSET constraint, or more generally any formalist theory that can generate a grammar that bans onsetless syllables everywhere? No: while the phonological component defines a set of possible grammars, it does not guarantee that every definable grammar exists. In fact it has nothing to say about the popularity of individual grammars; the frequency of a grammar depends on performance factors, so the lack of a language without onsetless syllables perhaps indicates the functional utility of using onsetless syllables as boundary markers for words (essentially adopting proposals by Trubetzkoy 1939:273ff).

We also believe that it is uncontroversial that some grammars are easier to learn than others – i.e. they are more likely to survive the learning process intact. For example, the vowel inventory [i e a o u] is extremely stable (e.g. it has survived unchanged in Polynesia for over a thousand years). No doubt the stability is due to the inventory's perceptual and articulatory desirability (Liljencrants & Lindblom 1972; Lindblom 1986; Schwartz et al. 1997a,b). Grammars with a vowel system [i a o] are not robust in terms of these criteria, and so they are significantly fewer in number (see Kingston 2006 for references and discussion).

In terms of the frequency of vowel systems, what then are the roles of synchronic and diachronic explanation? Even if the learner comes to the task of learning a language with substantial innate knowledge of what a possible language can be, the variety of human languages at all levels of description, including the phonetic, is such that experience with speaking and listening will profoundly shape what is learned. Learning is, moreover, an occasion for mistransmission or misinterpretation of the current state of the language, which can lead to language change if it persists and spreads through the speech community. Vowel systems such as [i a o] lend themselves to misperception and misinterpretation, and so are liable to be altered in language change. In contrast, [i e a o u] vowel systems have communicative stability. Consequently, it is undeniable that learnability factors offer an account of relative typological frequency.

In contrast, it is not necessarily the case that the phonological component has any affect on relative frequency. The sole necessary requirement on the phonological component is that it is capable of generating grammars with both [i e a o u] and [i a o] systems; the popularity of individual systems can be ascribed to performance factors.

As another example, it is not necessarily a Competence theory's job to account for the fact that [g] is often absent while [b] and [d] are not. After all, every imaginable voiced stop inventory exists in terms of major place of articulation, as shown in table (8).

(8) *Voiced stop inventories*

g	b	d	Nhanda (Blevins 2001), Catalan (Wheeler 2005)
g	b		Tigak (Beaumont 1979)
g		d	Wapishana (Tracy 1972), Ayutla Mixtec (Pankratz & Pike 1967)
	b	d	Sioux Valley (Santee) (Shaw 1980:17), Xavanté Macro-Je (Rodrigues 1999a)
g			Makurap (Rodrigues 1999b:112ff)
	b		Koasati (Kimball 1991)
		d	Diyari (Austin 1981), Nambiquara (Kroeker 1972)

It is the job of a theory of phonological competence to produce grammars that generate all of the inventories in table (8). There is nothing more it needs to explain. It is true that languages with a [g] and missing some other stop (like Tigak, Wapishana, Ayutla Mixtec, and Makurap) are rare, but there is no reason to ascribe their rarity to Competence mechanisms. In contrast, factors such as difficulty of producing voicing on velars (i.e. [g]) (e.g. Ferguson 1975; Ohala 1983) mean that learners would be more likely to eliminate it from their inventories or reinterpret it as voiceless [k]. Therefore, an explanation of why [g] is rare relative to other voiced stops is not an explanation about markedness as a Competence concept – it is an account of Performance.

In short, diachronic approaches and synchronic explanation theories are not incompatible in some areas. Formalist theories of phonology have no interest in accounting for true universal tendencies. In the formalist conception, facts about typological frequency are likely to be accounted for by performance mechanisms. In short, if both α and β are attested properties of language, the phonological component must be able to generate grammars with both α and β. If β is more common than α, the reason for their relative frequency can be sought in diachronic change and performance. Of course, 'diachronic change' here means more than ease of transmissibility; war, disease, and migration could also influence the typological frequency of a particular sound pattern.

Of course, a methodological challenge arises with unattested grammars: is a grammar unattested because it cannot be generated, or because it has a very low chance of being transmitted intact? For example, is lack of epenthetic [⊙] indicative of a competence restriction, or is it just due to the fact that [⊙] is extremely rare in languages anyway?

We have argued above that one way to tell the two options apart is if there is a situation in which the language property is expected but fails to emerge. This was the case with epenthetic [k]. It's absence is particularly striking given the comparative ease with which it is produced by sound change from earlier *t. That is, a synchronic grammar could have [k] as an epenthetic segment or as the result of neutralization if sound change was not kept in check by a constraint ranking that harmonically bounds such an outcome. We have also argued that evidence can be sought by other means such as the experiments described in section 3. In short, if it can be shown that a particular grammar is expected for performance reasons yet does not arise, it is highly likely there is an active phonological mechanism that prevents it.

If one's performance theory predicts that an unattested sound pattern should be rare or non-existent, what can be determined about the role of synchronic phonological mechanisms? Blevins (2004:237) proposes a methodological principle: "Principled diachronic explanations for sound patterns have priority over competing synchronic ones unless independent evidence demonstrates beyond reasonable doubt, that a synchronic account is warranted." We take this proposal to mean that if a grammar is unattested and there is a diachronic explanation for its lack of attestation, then there are no phonological mechanisms that prevent the grammar from being generated.

We question the premise of this methodological principle. If a sound pattern has a diachronic explanation, it does not necessarily follow that there must be no synchronic account. Since diachronic change is influenced by performance pressures, the principle amounts to saying that the existence of a phonological constraint cannot be influenced by performance pressures (or at least those that influence diachronic change).

At least some of the time it should be expected that synchronic and diachronic explanations agree: some synchronic constraints will ban the same grammars that some diachronic changes avoid. Why should some synchronic constraints have the same prejudices as diachronic change? As performance pressures influenced the evolutionary development of innate phonological constraints, phonetically 'natural' constraints would be favored in survival, while 'unnatural' constraints may have been eliminated (see Chomsky & Lasnik 1977:487-9 for an example). Therefore, a number of innate constraints could militate against structures that have little perceptual, articulatory, or processing value.

We are certainly not denying the autonomy of synchronic constraints and diachronic change; after all, this paper has been at pains to identify and delineate the distinct roles of diachronic and synchronic explanation. We are merely observing that *some* convergence in their effect (and therefore some overlap in roles) should be expected as the performance pressures that have selected for particular constraints or constraint systems may also be those that influence synchronic perception, articulation, and processing. Therefore, the fact that a diachronic account predicts the presence or lack of a sound pattern does not rule out the possibility that there are also synchronic restrictions that have the same effect. Showing that there is a diachronic account for the lack of a sound pattern does not imply that there is no synchronic account.

However, the extent of the convergence is difficult to discern as it is difficult to tell whether a particular grammar cannot be generated because a performance theory predicts that it will have low probability of survival. The only immediate options are to examine such issues using experiments of the kind described in section 3.

In summary, we see diachronic explanation as crucial in accounting for some sound patterns. Diachronic approaches explain why some grammars that the phonological component can generate are unattested. They also account for the relative typological frequency of grammars (and no doubt partly for the popularity of particular sound patterns within languages). However, if the performance pressures that influence language learning also influenced the survival of innate constraints, some convergence should be expected; there should be innate constraints that ban some of the grammars that are also avoided in diachronic change.

5 Conclusions

The aim of this paper has been to identify sources of evidence for synchronic cognitive phonological restrictions. We argued that there is a role for diachronic explanation, but it is confined to accounting for the non-attestation of unlearnable grammars and partially for the relative frequency of attested grammars. Performance factors certainly influence why [g] is typologically rarer than [d] and [b]; the phonological component may have no influence on typological frequency. Clear evidence for synchronic constraints is found when sound patterns that well attested in diachronic change never result in synchronic alternations, such as [k] epenthesis and neutralization to [k]. We argued that such systems are avoided, so indicating that the phonological component is incapable of generating grammars with [k] epenthesis and neutralization to [k], and there must therefore be synchronic constraints that ban them.

We further argued that there is a necessary link between sound patterns and their phonetic motivations in Athabaskan tonogenesis, that sound change is synchronically optimizing, and that there is a variety of experimental evidence that there are psychologically active restrictions on sound patterns. We also identified evidence that infants have an innate initial ranking of markedness over faithfulness constraints.

In short, a strong version of diachronic explanation cannot hold – that there are no substantial cognitive restrictions on the phonological component. Active synchronic constraints are necessary.

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