Transmissibility and the role of the phonological component

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Abstract

Determining the structure of the phonological component is crucial in understanding the predictive power of theories of transmissibility like Evolutionary Phonology. This article argues for a highly restricted, innatist phonological component. It discusses two testable predictions: the 'straitjacket' effect limits functionally well-motivated phenomena in arbitrary ways, the 'functional ignorance' effect means that some functionally well-motivated phenomena never occur. A restricted phonological component means that the explanatory domain of Evolutionary Phonology (and theories of transmissibility more generally) is i-language external phenomena, such as typological frequency.

1. The cognitive contribution[1]

Which theories of the phonological component is Evolutionary Phonology (EP) compatible with? Without an explicit answer, it isn't possible to determine the predictive power of EP: the weight of EP's explanatory burden differs depending on how limited the phonological component's generative capacities are.

At one reasonable extreme, the phonological component (PC) could be capable of 'anything': i.e. it could generate grammars in which an input is

[1] My thanks to Marc van Oostendorp and Emmon Bach for their help with Frisian and Haisla respectively, and to John Kingston, John McCarthy, Alan Prince, and Bruce Tesar for comments on related work and some of the proposals here. Scanned portions of the more obscure citations are at http://ling.rutgers.edu/~delacy/theoreticallinguistics.

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mapped to any definable output (e.g. an SPE without ch. 9).[2] With such a model, the burden for explaining sound patterns falls squarely on theories of language transmissibility, like EP. EP's principles effectively order languages in a hierarchy of the likelihood that they will be learned without alteration.

An alternative is a PC that places non-trivial restrictions on possible grammars. In this view, a gap in sound patterns has *two* potential sources of explanation: (1) the inability of the PC to generate certain grammars, and (2) transmissibility. With a restricted PC, EP takes on the role of a theory of Performance: EP is not in conflict with the idea that the phonological component is non-trivial; it complements it. EP's role is still to order languages in terms of their transmissibility, but it can only do so within the bounds set by the PC.

This article will argue that the phonological component is restrictive. Consequently, the phonological component itself is a source of explanation for sound pattern generalizations. Recent work along these lines includes Kiparsky (2004), de Lacy (2006a), and de Lacy & Kingston (2006); I will delve into different empirical issues here, though I will make broadly similar theoretical points.

A restrictive, innatist PC has two effects. Section 2 discusses the 'straitjacket effect': functionally well-motivated patterns are forced to abide by arbitrary restrictions. Section 3 discusses the 'functional ignorance' effect: some functionally well motivated patterns never occur. Both derive from the same fact: restrictions on the PC are not directly tied to transmissibility concerns. The implications for EP are explored in section 4.

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2. The straitjacket effect

Suppose a learner 'wants' to actuate final obstruent devoicing. The learner can employ a ranking that meets his/her aims, but only within the confines of the rankings available (I'll employ Optimality Theory here (Prince and Smolensky 2004) though the same points can be recast

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[2] I am of course assuming that there must be a phonological component: i.e. a cognitive component responsible for manipulating symbols that are ultimately interpreted (in some other module) as motor commands for speech. See Chomsky (1966, esp. p. 77) for relevant discussion.

in any generative theory). The rankings might not be able to generate the *ideal* grammar in terms of transmissibility – this is the 'straitjacket effect'. Final devoicing is discussed here to illustrate the straitjacket effect and to make an explicit connection with Blevins' article. Many other phenomena should show straitjacket effects, too; the point made here has been discussed in other terms previously by many others, including Kiparsky (1995), Hayes (1999§6.2), and Gordon (2006).

Blevins (this volume: (9iii)) observes that aerodynamic considerations may affect transmissibility so that 'selective' final obstruent devoicing could result: e.g. /g/ → [k] but /b d/ remain voiced. However, a restrictive, innatist PC offers no guarantee that it can generate a grammar that meets the functionally ideal pattern.

In fact, I am not aware of any languages that have selective devoicing (Blevins' counter-examples are discussed below). This can be attributed to a 'straitjacket effect': the PC is incapable of generating grammars with selective devoicing.

It is not a trivial matter to construct a theory of the PC that bans selective devoicing. To illustrate one approach, /g b d/ could be required to have the same feature for voicing – e.g. [voice]; crucially, /g/ cannot have a feature phonetically interpreted as voicing that differs from /b/ and /d/'s voicing feature. However, this representational restriction is far from enough. There can also be no markedness constraints that favour voiced stops of a particular place of articulation over other voiced stops – e.g. no *DORSAL/+VOICE, otherwise the ranking ||*DORSAL/+VOICE ≫ IDENT[voice] ≫ *+VOICE|| would create an unattested grammar. There can likewise be no faithfulness constraint that preserves voiced stops of a particular PoA over others: i.e. no IDENT[voice] *if dorsal*, and so on. Other constraints that could cause undesirable emergent or blocking interactions would also have to be banned. However, it is unnecessary to go into further detail; OT – and any other PC theory with sufficient structure – is capable of being restrictive. The point here is that selective devoicing requires a PC that is far from trivial.

On the empirical side, Blevins (this volume) identifies Tonkawa, Frisian c. 1900, and Haisla as cases of selective devoicing. However, there are no voiced stops in Tonkawa: Hoijer's (1933) presentation made unfortunate use of orthographic *b*, *d*, *g*, *gw* to represent voiceless stops (cf. Hoijer 1946, Mithun 1999).

Eijkman (1907: 19) reports that in the Grouw dialect of Frisian "le *b* et le *d* sont donc généralement à la fin d'un mot ou entièrement vocaliques ou soufflés seulement dans leur dernière partie." ["So, in general world-final [b] and [d] are entirely voiced, or only voiceless in their last half."]. However, there is no direct evidence that /g/ devoices to [k]: Eijkman (1907) provides no alternations, but only notes (p. 35 fn. 2) that no word ends in [g]. So, the generalization is that word-final codas never have [g]; they only ever have [b d]. Could this phonotactic generalization result from something other than selective devoicing of /g/ → [k]? (This is a significant question in Optimality Theory as there are no restrictions on inputs). There is: Tiersma (1985: 36–7) reports that Modern Frisian /g/ spirantizes to a fricative in codas, and due to a general process of obstruent devoicing ends up as [x]. The result is that word-final codas can contain [b d], but not [g]. The process of coda /g/ → [x] provides a grammar that accounts for a surface ban on coda [g]s without appealing to selective devocing.

For Kitlope Haisla, Lincoln & Rath (1986: 11) report that the stops /d͡z d͡l g^{j} ɢ/ "usually" devoice finally (there are no alternations involving /b g^{w} ɢ^{w}/), and there are "not many" examples. /d/ exhibits "free variation between variants with and without voicing", and "occurs very frequently". The fact that final /d/ devoices *more often* than other voiced stops does not mean that there must be a ranking in which /d/ devoices finally while underlying voiced dorsals do not; Competence theories do not necessarily seek to account for relative frequency of this type (Coetzee 2004). In any case, it would have to be shown that the impressionistic report of the frequency of devoicing is actually different, specially given the small quently". The fact that final /d/ devoices *more often* than other voiced stops does not mean that there must be a ranking in which /d/ devoices finally while underlying voiced dorsals do not; Competence theories do not necessarily seek to account for relative frequency of this type (Coetzee 2004). In any case, it would have to be shown that the impressionistic report of the frequency of devoicing is actually different, specially given the small number of lexical items with a non-/d/ voiced stop vs. the many with /d/.

In short, none of the cases cited offer unambiguous support for selective devoicing; further investigation of Haisla is clearly warranted.[3]

A subtle point emerges here: despite the multiplicity of transmissibility motivations for devoicing cited by Blevins, most PC theories provide only *one* way to devoice obstruents in codas. For example, Lombardi (1999) proposes ‖onset-IDENT[voice] ≫ *+voice ≫ IDENT[voice]‖. This is a type of straitjacket effect: *different* functional motivations are forced to be actuated in the PC by *the same* ranking.

3 At the time of writing, Emmon Bach is currently working on a Haisla grammar that may help (though on another dialect).

This point has an interesting implication for stop voicing. Stop voicing is predicted to occur by modern theories of the syllable: it is the result of a pressure for moraic consonants to have high sonority segments (Zec 1988, Morén 1997). The sonority scale for consonants is given in (1).

(1) *Consonant sonority hierarchy*
|−vd stops < +vd stops < −vd frics < +vd frics < nasals < liquids < glides|

As Blevins (this volume) reports, Saeed (1990) provides evidence that voiceless stops become voiced in Somali; voiceless fricatives remain voiceless (though cf. Kiparsky this vol.). The core ranking for Somali is given in (2), from de Lacy (2006a: 122–24). The constraint *μ/−VD_STOP militates against the worst (i.e. lowest sonority) coda consonant; it does so at the expense of faithfulness to [voice]. The coda does not become even more sonorous to keep its voicing (i.e. [s]) because doing so would fail to preserve its [continuant] value.

(2) *Somai stop voicing*

/adak/	IDENT[±cont]	*μ/−VD_STOP	IDENT[±voice]
a.dag$^{\mu}$ ~ a.dak$^{\mu}$		W	L
a.dag$^{\mu}$ ~ a.das$^{\mu}$	W		L

There is a crucial difference between the EP account of stop voicing and the sonority account: the sonority account predicts a language in which voiceless stops become even more sonorous than voiced stops. A striking case is found in Dakota (Shaw 1980; analyzed in de Lacy 2002a§6.5.1.1, 2006a: 171–3). Coda /k/ becomes voiced (3a), but this is clearly part of a more general process of coda sonorization as /p t tʃ/ all become nasals (3b).

(3) *Dakota coda stop sonorization*
(a) /wãjak/ → [wãjag] 'to see' cf. [wãjak-a]
/ʃok/ → [ʃog] 'thick, solid' cf. [ʃo.k-a]
/t͡ʃek/ → [t͡ʃeg] 'to stagger' cf. [t͡ʃe.k-a]
(b) /xap/ → [xam] 'to be stripped' cf. [xa.p-a]
/sdot-ja/ → [sdon.ja] 'to know'
/wa-nit͡ʃ/ → [wa.nin] 'be without, lack' cf. [wa-ni.t͡ʃ-a]

/k/ becomes [g] in codas because it's the best that can be done: it cannot become [ŋ] because velar nasals are banned in the language. The Dakota ranking is much like Somali's, except that there is greater pressure from markedness constraints to avoid all segments that are less sonorous than nasals in codas. The full influence of the markedness constraints are blocked by *DORS/NASAL, only allowing /k/ to get as far up the hierarchy as [g] (de Lacy 2002a, 2006a).

Stop voicing exhibits a straitjacket effect: whatever the functional motivations for actuating stop voicing, the restrictions on the phonological component mean that it must be formally modelled as an increase in sonority. Consequently, the ranking predicts that stop voicing could be intermingled with other types of sonority increase, as in Dakota. In summary, straitjacket effects show that the phonological component is restricted in non-trivial ways.

3. Functional ignorance

Innatist theories of the phonological component are functionally ignorant: they do not require that constraints/rules bear a direct relation to functional pressures such as transmissibility.[4] Consequently, the PC may be incapable of generating grammars that have highly desirable properties in terms of transmissibility.

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point of view, yet is unattested. Here, I make a similar argument for unattested types of stress systems.

Lower vowels have longer inherent duration than higher vowels (and often peripheral vowels are longer than central vowels) (Lehiste 1970). For example, Chung et al. 1999 report the hierarchy of inherent durations

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4 There can be an *indirect* relation to articulatory, acoustic or parsing pressures, as discussed by Chomsky & Lasnik (1977§1.2), Jenkins (2000), and others; the indirect relation comes through natural selection. Of course, constraints can come about through other means: exaptations, sexual selection, or as a necessary consequence of the structure of the language faculty (i.e. 'spandrels'). In the context of this discussion it is terminologically ironic that innatist theories are profoundly 'evolutionary' – they rely on evolution to have shaped the cognitive component.

for Korean: |a, ɛ > o > e, i u > ʌ ɯ| ([ʌ ɯ] are centralized), with a 15 to 20 ms difference between each group.

As stressed vowels usually (perhaps always) have longer duration than other vowels (Fry 1955 and many others), the inherent duration of vowels could be misinterpreted as stress. The result would be a system where lower vowels attract stress over higher vowels. It turns out that there are many languages (roughly) like this: stress seeks out lower vowels and ignores higher ones and/or ignores central vowels and seeks out peripheral ones ('sonority-driven stress' – Kenstowicz 1997, de Lacy 2002a, 2004, 2006a,b). (From the innatist point of view, the fact that there are sonority-driven stress systems means that the PC must be able to generate grammars in which stress seeks out more sonorous vowels over less sonorous ones.)

There is also a relation between stress and fundamental frequency. Stressed vowels often have higher F_0 or are the locus of a pitch excursion (not necessarily because they *induce* higher F_0 (which they may do), but because pitch accents dock on them) (Fry 1958, Nakatani and Schafer 1978; overviews in Ladd 1996§2.2.1 and Gussenhoven 2004§2.2.3). Transmissibility considerations might lead one to predict a language in which high-toned vowels are misinterpreted as stressed, so leading to a stress system in which stress seeks out higher-toned vowels over lower-toned vowels. Again, a number of languages have just such a system (de Lacy 2002b).

It now remains to examine the implications of the transmissibility approach for the correlations between pitch and vowel height, and pitch and duration.

In a number of languages there is a correlation between pitch and duration. Gandour (1977) identifies Panjabi, Zapotec, Mazatec, Chatino, Tengango Otomi, Kutchin, Standard Thai, and Mandarin as all having low-toned vowels with longer duration than high-toned vowels. It was suggested above that longer inherent duration (related to vowel height) could be misinterpreted as stress. Therefore, one might expect a learner to misinterpret longer duration on low-toned vowels as stress; the resulting system would have stress seek out low-toned vowels and avoids high-toned ones. No such stress system has been reported.

There is also a correlation between F_0 and vowel height: higher vowels have inherently higher F_0 than lower ones (e.g. Lehiste & Peterson 1961 report a 20 Hz difference between low and high vowels for American

English). It might be expected that a learner could misinterpret the higher F_0 as part of a realization of stress. The resulting stress system would therefore have stress seek out higher vowels over lower ones. No such stress system exists.

In other words, misinterpreting duration as stress cuts both ways: it predicts that systems could develop in which stress favours lower, more peripheral vowels (and there are such cases), but it also predicts that systems could develop where stress seeks out low-toned vowels (and there are no such cases). Misinterpreting F_0 as marking stressed vowels also cuts both ways: it correctly predicts languages where stress seeks out high-toned vowels, but incorrectly predicts languages where stress seeks out high vowels because they have higher inherent pitch.

So, there are imaginable grammars that make sense in terms of transmissibility, but never occur. A restricted PC provides the solution: the PC is simply incapable of generating a system in which stress (i.e. the location of foot/PrWd heads) ignores high-toned vowels and falls on low-toned ones; similarly, there is no ranking which allows stress to fall on high vowels and avoid high-toned ones. These arbitrary restrictions (in terms of transmissibility) are expected with a restricted, innatist PC.

Of course, to make the preceding argument robust a few things must be demonstrated but are well beyond the confines of this article to do. One is that the durational differences between low- and high-toned vowels are enough to potentially cause a misinterpretation as stress, and the same goes for the pitch differences in vowels of different heights. Another is to set the arguments within a well-defined theoretical framework. The obser-

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4. What does it all mean?

Some sound patterns that make good sense from a transmissibility point of view do not exist (i.e. low-tone- and low-sonority-driven stress), and sound patterns that are terms functionally well motivated have arbitrary restrictions imposed upon them (devoicing, voicing). A restricted PC can account for these gaps. So where does this leave EP?

There is no inherent conflict between EP and generative theories: they are about fundamentally different things. Generative theories like SPE and OT are about cognitive components that manipulate symbols which are eventually converted into motor commands. In contrast, EP is a theory of transmissibility: it is about a cluster of objects, including aspects of the perceptual system, the cognitive systems responsible for learning, and the articulatory system. In broad strokes, SPE and OT are theories of Competence while EP is a theory of Performance.

The methodological implications are that the PC may be solely responsible for some gaps in sound patterns, but transmissibility restrictions may be solely responsible for others. For example, no language has just one consonant; transmissibility must be responsible because no such language would survive for long (or even come about) *and* all generative theories are easily able to generate a grammar for such a system (and there is no need to prevent them from doing so).

A further implication is that there is no restriction that *prevents* the PC and transmissibility from providing overlapping accounts. In other words, the PC may be unable to generate a ranking for a system that is also undesirable in terms of transmissibility. This follows because the PC's internal structure is not necessarily sensitive to what is good for transmissibility, nor could it be: PCs act on *grammars*, transmissibility is relevant to *languages*, of which the grammar is only a small part.

This prediction of the approach cases some light on Blevins' Premise (this volume, p. 124f.) "principled *extra-phonological* explanations for sound patterns have priority over competing *phonological* explanations unless independent evidence demonstrates that a purely phonological account is warranted." The Premise has an exhortatory value: it requires rigorous consideration of extra-cognitive (Performance) explanations for gaps in sound patterns. However, it goes too far by insisting that if a sound pattern has a transmissibility account there *must not* be any PC account (at least, this is how I interpret "have priority"). This extra clause does not follow from any theoretical principle, either in EP, or in generative theories: there is no PC principle that prevents it from having constraints that have roughly the same empirical effect as transmissibility concerns.

A further prediction follows: "What OT fails to account for is why certain sound patterns, like final devoicing, are very common, while others, like final voicing, are rare ..." (Blevins this volume). OT, and every other generative theory, 'fails' by choice: there is no requirement that Competence theories should account for frequency (typological, lexical, etc.) as frequency falls squarely in the domain of Performance factors (recent discussion: Rice 2006§4.7, de Lacy 2006a§1.3, 8.3, to appear cf. Moreton to appear). This underscores the point that EP and SPE/OT are about fundamentally different things: they don't account for exactly the same range of empirical phenomena.

To summarize, EP is a theory of transmissibility. Transmissibility cannot account for every phonotactic pattern and alternation, so a restricted, highly structured phonological component is necessary. There is no inherent conflict here: EP is a theory of Performance, not of Competence. For some phenomena it may be unclear where to seek an explanation – in the phonological component, or in transmissibility; this is a good thing: the theories predict that the analyst *should* face such vexations.

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