Phonological Evidence

Paul de Lacy

This chapter examines a well-known generative innatist theory of the phonological component and related modules. It asks what this theory identifies as empirical evidence for it, and for which modules. It also identifies predicted ambiguities, where two or more modules influence the same phenomenon. Specific phenomena discussed include alternations, phonotactics, phonetic neutralization, loanword adaptation, and typological frequency.

1 Introduction*

Prince (2006) has emphasized that a theory itself is necessarily an object of study. Derived from this point is the theme of this chapter: a theory itself must be examined to determine what is evidence for it.

The past few decades have seen the development of a generative, innatist, modular framework (GIMF) for theories of the cognitive resources used for the production and perception of human speech. Theories of the phonological component (PhC) such as Chomsky and Halle (1968) and Prince and Smolensky (1993/2004) are set within GIMF, as are theories of the phonetic component like Keating (1988, 1990). GIMF theories share many properties (elaborated in section 2) – so many that it is possible to ask of them as a class: what is the GIMF phonological component responsible for? This question expands to: what does the GIMF PhC generate and influence, and what can interfere with a straightforward relation between PhC representations and their physical realization or perception?

Many empirical phenomena have been argued to provide evidence within GIMF phonological theories; they include phonotactic generalizations, synchronic alternations, free variation, diachronic change, loanword adaptation, language games, first and second language acquisition data, frequency (typological, lexical, text, allophonic), cross-dialect comparisons, and many more. Understanding of the various GIMF modules and mechanisms (phonological, phonetic, perceptual, learning) as well as external performance phenomena has developed to such a point that we can now profitably ask: Is the GIMF phonological component entirely, partially, or not at all responsible for these phenomena? How can we tell?

I emphasize that I am not asking whether a particular phenomenon should be the responsibility of *every* theory of the phonological component. I am asking whether a *GIMF* phonological component predicts that it should be responsible; non-GIMF theories need to be examined separately (e.g. non-innatist/functionalist theories – see Gordon 2007 for an overview), and nothing said here necessarily applies to them. Also, there is a wide range of GIMF phonological theories, and many differ on the details of what they are responsible for generating. However, there are properties common to all of them that make generalizations possible.

With a narrower focus, the issue of a GIMF phonological module's responsibility is crucial to theories of markedness. There has been a great deal of disagreement recently about which phenomena are relevant for the concept of markedness (e.g. Blevins 2004, Hume 2004, de Lacy 2006a, Rice 2007). A great deal of this disagreement can be traced back to fundamentally different conceptions of the properties of the phonological component, and its relation to other modules (or even if there are other modules). This chapter aims to clarify the phonological component's role for the particular framework I work in – GIMF. This framework is discussed in more detail in section 2.

The primary difficulty faced here is that a GIMF phonological component's output can be obscured by other modules and influences. For example, a non-trivial phonetic component (e.g. Keating 1988, 1990, Kingston and Diehl 1994) can obscure differences between phonological symbols (section 4). The perceptual system can also act so that the phonological representation a hearer deduces is different from the speaker's (section 5). External factors can also affect speech sound patterns, especially typological frequency and lexicon content (section 6). Section 7 discusses GIMF theory building and evaluation techniques.

2 The GIMF framework

The framework outlined here is generative, innatist, and modular, with 'non-trivial' modules, explained below (for recent overviews see Harris 2007, Kingston 2007). I reserve the term 'theory' here to refer to theories of particular modules. 'Framework' refers to the theory of modules (i.e. which ones exist, how they relate to each other), and of the core properties shared among modules (e.g. generativist, innatist). The overview is brief because the framework and theories are commonly used; apart from the discussion below, I will assume the reader's familiarity with them.

A symbol manipulation module – the 'phonological component' (PhC) – receives an input from a lexicon (via or at least influenced by a syntactic and/or morphological component). The PhC effects non-trivial mappings from input to output (Chomsky and Halle 1968, Prince and Smolensky 2004). The output of the PhC is the input for another cognitive component – the 'phonetic component' – which converts it into a different type of representation ('gradient') (Keating 1988, 1990, 1996, Kingston 2007§17.4.3 for an overview). The output of the phonetics is (eventually) realized as movements of articulators and the lungs; the movements cause speech sound. There is also a perceptual mechanism that processes auditory stimuli, converts it into a phonetic representation, then a phonological one, and matches or converts the phonological representation into lexical items (e.g. see Moreton 2000).

The theory is innatist in that each module comes 'genetically endowed' with primitives – i.e. representations, constraints, input→output derivational mechanisms, and so on. The primitives are not learned. (It is of course worth examining functionalist, non-innatist theories to see what they predict to be evidence – e.g. Gordon 2007; the aim here is to focus solely on innatist theories.) For an overview of the innatist approach, see Newmeyer (1998, 2003).

The cognitive modules are 'non-trivial' in the sense that they do not provide a straightforward conversion of input to output. PhCs may differ from language to

language so that the same input in different grammars is mapped to different outputs. The same is true for the phonetic component. For example, in some languages the phonological feature [+voice] is phonetically interpreted as voiceless unaspirated but in others as voicing (i.e. as different degrees of Voice Onset Time) (Kingston and Diehl 1994). The physical properties of the articulatory tract may alter the phonetic output; for example, while the phonetic output may call for a drop in F_0, physical restrictions may prevent this from happening if the speaker's pitch production has 'bottomed out'. Of course, each module is limited: not all imaginable unfaithful input→output mappings in the PhC are permitted, and the range of realization of phonological symbols by the phonetic component is restricted (though exactly how limited is debated – SPE cf. Declarative Phonology – Scobbie et al. 1996), and the articulatory tract is sufficiently similar in humans to often make individual physical differences irrelevant. Consequently, production and perception involve the complex interaction of modules in GIMF; the PhC is just one factor of many (Chomsky and Halle 1968:3).

There are some major differences between GIMF and other theories. A competing framework considers the PhC and phonetic component as 'the same' in some sense – for example, they may employ representations with similarly fine distinctions (e.g. Kirchner 1998). Another is to say that the PhC is very unrestricted in what it can generate, and that all observed restrictions follow from transmissibility in learning (perhaps advocated by Evolutionary Phonology (EP) – Blevins 2004, 2006, cf. de Lacy and Kingston 2006, de Lacy 2006a). Obviously, each theory needs to be separately examined for what it predicts to be evidence for it. The focus here is on the generative, innatist, modular framework outlined above and developed in detail by the cited works.

The model above is incomplete as it does not mention other relevant cognitive modules. These may include a 'paralinguistic' module (e.g. Ladd 1996§1.4), and an 'orthographic' module for conversion of written/typed text to phonological representation. These modules are of course important in a comprehensive examination of the capabilities of the PhC, but will not be discussed here due to lack of space.

Below I will refer to 'evidence' a great deal. 'Evidence' for the PhC refers to an individual's speech sound phenomenon which must be generated by the PhC (i.e. the PhC is 'responsible' for the phenomenon). Such evidence provides a way to determine individual states of the PhC, leading to a theory of all possible states (i.e. a 'phonological theory/theory of the PhC').

3 Phonological responsibility

The aim of the following two sections is to ask what the GIMF PhC is 'responsible' for, what the extent of its responsibility is, and to provide the beginnings of an answer. The point in doing so is not to exhaustively determine the answer, but to illustrate how the theory can be usefully examined in its own right.

While it would be a legitimate strategy to focus on one particular GIMF PhC theory like Optimality Theory, there is no need to – the many GIMF phonological theories proposed so far share many properties that mean they predict similar responsibilities.

GIMF theories are about the cognitive resources of an individual. A particular set of speech-related cognitive modules exists in a particular speaker, and is devoted to a particular grammar. A number of implications about potential evidence for the theory follow from these basic notions. An individual may have several different grammars (i.e. registers, dialects, languages). The theory is about individual grammars, so there is nothing inherent in GIMF theories that requires a ranking in one grammar to imply that some or every other grammar has the same ranking.[1] Similarly, the theory's principles mean that a ranking for one individual cannot be used to determine the ranking of another individual, even if they speak the same dialect. Therefore the only legitimate source of evidence identified by the theory is the output of an individual grammar in an individual speaker.

This restriction on evidence for a PhC theory may seem extreme, and is far from current practice. For example, phonological descriptions often draw on data from several different speakers of a language or dialect, amalgamate their data, and provide a description of the amalgamation. For those interested in the PhC, what's the problem with doing so?[2]

A significant problem is the creation of a 'pseudo-grammar'. Suppose different phenomena from different speakers are combined: S1's assimilation, S2's neutralization, and S3's stress pattern. Does the combination of different aspects of S1, S2, and S3 necessarily create a possible language? The theory does not guarantee this: since the only legitimate object of study is an individual grammar in an individual speaker, the combination of the output of different grammars could create a set of data that no grammar could generate.

Similarly problematic is the 'democratic' method of phonological description. In this approach, data from several speakers is examined, and the majority attestation is chosen as the actual process. For example, suppose of 6 speakers 4 have assimilation /nk/ → [ŋk], 4 have foot-initial aspiration of stops, and 3 have lenition of /v/→[w] in onsets. The democratic method would provide a ranking that could produce assimilation, stop aspiration, and lenition. However, there is no guarantee that this collection of data is ever possible for an individual speaker: it could be that none of the speakers have all three phenomena together. Consequently, this sort of democratic theorizing is a bad idea; the theory accounts for the grammars of *each* speaker so the only legitimate approach is to provide grammatical descriptions for each speaker.

But is it legitimate to generalize from grammar to grammar if they generate the *same* (or at least very similar) phonological outputs? Suppose there are two speakers S_1 and S_2 and they have the same phonological outputs *and* they have exactly the same linguistic experience: could we conclude they have the same ranking? This issue depends on a theory of learnability (not included in the theoretical package outlined in section 2; see Tesar 2007 for an overview). Because the components allow non-trivial input→output mappings, the theory can allow several different ways to produce the same output. For example, suppose both speakers lack [k] in codas. In OT, the [k] could be potentially eliminated in coda environment through deletion, coalescence /V_1k_2/→[$V_{1,2}$], lenition /Vk/→[Vx], change in place of articulation /Vk/→[V$\widehat{\text{tʃ}}$], epenthesis /Vk/→[V.k̲i], and so on. If one's theory of learnability forced the speaker to choose the same one of these particular options, then it would be legitimate to say that S_1 and S_2's ranking are the same. However, if the learning process allowed a random choice of rankings that

generated the same output, there is simply no guarantee that one speaker will have the same ranking as another, even though they have exactly the same output. This is one aspect of non-triviality in the PhC: the same phonotactic pattern can often derive from many different inputs via many different derivational routes.

This point is even more extreme in different registers. For example, for an individual speaker of Samoan, his/her 'colloquial' grammar has a [k] where his/her 'formal' grammar has a [t] (Mosel and Hovdhaugen 1991). Can one conclude that /k/→[t] in the formal grammar, or that /t/→[k] in the colloquial grammar? The theory does not commit itself to this assumption. The fact is that there are two different grammars (or at least two different rankings), and potentially two different lexicons. The fact that [tai] 'tide' in formal Samoan is used to express the same meaning as [kai] in colloquial Samoan therefore does not imply that the underlying form is /tai/ in both registers (or /kai/), or that there is a process of /t/→[k] (or /k/→[t]) in one or the other. What is relevant here is a theory of inter-grammar interference: if additional theoretical mechanisms required that all registers have the same lexicon, for example, then it would be possible to use one grammar to provide information about the other. However, the framework outlined above says nothing about such cases except that they are two different grammars, and therefore it is not necessarily legitimate to generalize from one to the other. Of course, it would be ideal to *have* such a theory of between-grammar influence, but the point is that no such theory necessarily *follows* from the basic GIMF principles. Any claim of between-grammar interference therefore needs to be buttressed by a theory of how that interference works.

In short, core properties of GIMF restrict reliable potential evidence for its modules to the speech that is generated by an individual grammar of an individual speaker. Other theories may make other evidence relevant. A theory of between-grammar influence may mean that a ranking or lexical entry for one grammar may be the same for another grammar in the same speaker. A theory of learnability may necessitate that given a particular output a learner will always posit the same ranking (choosing from several potential alternatives). However, GIMF does not guarantee such assumptions, so whenever evidence is adduced that does not derive from a single grammar in a single speaker, further theoretical devices that make the evidence relevant need to be made explicit.

The 'one grammar of one speaker' is an ideal – a way to avoid problems that can be introduced by pooling data of different speakers. However, from a practical point of view many descriptions with such amalgamated data are probably still useful, even more so for descriptions in which variation among speakers is noted. However, unless the issue of data amalgamation is overtly discussed, it introduces an element of uncertainty for the PhC theorist: were patterns found in the minority (or just one) of speakers ignored? The issue comes up frequently with 'variation': in several cases I have seen recently descriptions are unclear whether variation is truly free (i.e. the variants occur freely in the grammar of an individual speaker) or dialectal (i.e. speakers are internally consistent about the variant they use, but different speakers use different variants).

3.1 Phonotactics

Since the source of evidence for the theory is limited to a single grammar of an individual speaker, one can ask which aspects of the grammar are potentially observable. Given current technology, only the output is visible; even then, the PhC's output isn't directly observable – only sound and articulator movement is detectable, but for the moment it will be naïvely assumed that the phonological output can be unambiguously recovered from any speech output (see section 4 for a rejection of this view). So, the GIMF PhC's output is effectively observable. PhC theories predict that the phonological component is wholly responsible for the distribution of phonological symbols in different environments – i.e. 'phonotactics'.

For example, Hawai'ian permits [p k ʔ] in outputs and no other oral stops, so the PhC must be able to generate this inventory (Pukui and Elbert 1979). The lack of a [t] is very rare (though for similar languages see de Lacy 2006a§1.3.1.1). Nevertheless, its rarity is irrelevant: the PhC must be able to generate a grammar without it because no other cognitive module is capable of doing so. The theory predicts that the PhC must account for all phonotactic patterns; a single valid example is therefore enough to motivate a revision of the theory (a point emphasized by Everett 2003 for metrical theory). Exactly what is meant by 'generate this inventory' differs in specific GIMF PhC theories; in SPE it could be due to restrictions on lexical form (Morpheme Structure Constraints), in OT Richness of the Base requires consideration of the mapping of input /t/ and a constraint-based solution to its lack of attestation (if an 'accidental lexical gap' approach is eliminated).

There are limits on what phonotactics can tell the analyst, but they differ depending on what theory one adopts. In some GIMF PhC theories, phonotactics provide insight into what outputs the PhC must generate, but not necessarily how it generates them. As observed above, a language with no [k] in codas (and no relevant alternations) can achieve this pattern by many different means: deletion, coalescence, change in place or manner, and so on. The rankings and input→output mappings that end up in a lack of output [k] are not directly observable for this phonotactic generalization: only the fact that output [k] is not present is directly detectable; rankings and inputs must be inferred from outputs.

The fact that OT – and SPE, and every other generative theory – can often provide several ways to account for a particular phonotactic pattern is occasionally underappreciated. For example, Lhasa Tibetan allows [m] and no other nasal stop word-finally, though [n] and [ŋ] are allowed elsewhere (Denwood 1999). As there are no alternations to show what happens to word-final /n/, is it reasonable to assume it neutralizes to [m]? No – the theory does not necessarily require that /n/ *must* neutralize: it could delete, coalesce with a preceding vowel /Vn/→[$\tilde{V}$], and so on (see de Lacy 2006a§8.2 for other examples). (This is particularly true for Optimality Theory; for SPE, there may be a morpheme structure constraint that bans /n/ from the lexicon, and/or the choice of possible mappings may be limited by the simplicity principle – Chomsky and Halle 1968:295).

Phonotactics in OT may give some idea as to the input→output mapping and constraint ranking, given certain assumptions (Tesar 2006). If a segment such as [p] is

allowed in onsets on the surface, some relevant faithfulness constraint must outrank all markedness constraints against [p] (in onsets) (for a more precise characterization of neutralization rankings, see de Lacy 2002:ch.6). It is less clear what the appearance of [p] tells one about input→output mappings. It is possible that /p/→[p], or it could be that /p/→∅ and /f/→[p] in a chain-shifting fortition process; in principle, either input→output mapping is possible (e.g. McCarthy 2004). If one adopts a learnability principle whereby a learner always assumes a faithful map in the absence of alternations (an eminently reasonable principle), then non-alternating phonotactics provide evidence for faithful input→output mappings. Certainly, if a language *bans* [p], there is no direct way without an explicit learnability theory of knowing what happens to /p/ apart from the fact that it does not surface as [p].

In short, GIMF PhC theories predict that all phonotactic regularities in every output of every grammar of every individual are valid evidence for the PhC's structure. Phonotactics provides evidence that the PhC must be capable of generating the relevant outputs, but does not necessarily imply any particular input or ranking. Additional theories may change the picture substantially. For example, a particular theory of learning may predict that a speaker will choose a particular input and ranking if they observe a phonotactic pattern like lack of a coda [k] (i.e. perhaps lack of coda [k] always comes about through neutralization). However, unless such a learning theory is identified, phonotactic generalizations do not provide clear evidence in GIMF theories about I→O mappings.

3.2 Alternations

GIMF PhC theories provide a mechanism for relating the outputs of morphologically-related forms – i.e. 'alternations'.[3] The theory imposes one unique input form for any given morpheme (ignoring suppletion for the moment, discussed below). Therefore, differences in the realization of a morpheme in different environments provide evidence for inputs and rankings. It is an absolutely crucial point that no other cognitive module in GIMF is responsible for alternations – the PhC bears all responsibility for generating them (though see section 4 regarding the phonetic module).

As with phonotactics, individual theories differ significantly as to *how much* insight alternations provide. For example, in the Nepalese language Yamphu the morpheme 'daughter-in-law' surfaces as [næmːiʔ] on its own and as [næmːid-æʔ] with the instrumental/ergative (Rutgers 1998). The common part is the morph for 'daughter-in-law' – [næmːiʔ]~[næmːid]. There are two important aspects here: that [ʔ] and its corresponding segment [d] are not identical, and that [næmːi] is identical in both forms.

In both OT and SPE, the input must contain a segment that corresponds to the output [ʔ]/[d]. Therefore, the alternation shows that Yamphu has an input→output mapping that involves feature change: i.e. some input segment /α/ becomes [ʔ] in codas and [d] elsewhere. From this point on, individual theories differ widely as to how much more can be determined.

In classical OT with Richness of the Base, on the basis of this alternation alone the [d]~[ʔ] alternation could indicate that /d/→[ʔ] in codas, or that /ʔ/→[d] in onsets, or

both, or that /d/→[ʔ] in codas and /ʔ/→∅ in a chain shift, or that there is a different input segment (/t/→[ʔ] in codas, /t/→[d] intervocalically) (see McCarthy and Wolf 2005 for recent discussion). The theory allows a large range of options from phonotactics and alternations. These options can often be narrowed down by considering more alternations, which may eliminate some possible input→output mappings (not in this case, though). An explicit GIMF-compatible theory of learnability is often necessary to narrow down the options further.

It's worth making a further point about the significance of an explicit learning theory here. The theory does not necessitate that the two forms of 'daughter-in-law' are related by means of an input; it can account for the morphological relatedness by having two phonological forms for the morpheme 'daughter-in-law' – /næmːid/ and /næmːiʔ/; they are mapped faithfully to the output, and the appropriate form is selected by phonological principles (e.g. Mascaró 1996).

So why should the alternation analysis be favoured over the suppletion analysis? The fact that the [d]~[ʔ] correspondence occurs in dozens of morphemes would heavily bias any reasonable analyst towards adopting an alternation analysis, and so does the fact that [d] never appears in codas, as does the productivity of this alternation. However, nothing in the GIMF PhC theory requires this to be so in the sense that either a suppletion or alternation analysis will equally account for this data. It is perhaps here where a theory of learning is crucial: such a theory needs to impose a bias against learners adopting a suppletive analysis – the learner needs to have an almost pathological desire to analyze different morphs of a morpheme as deriving from the same input, with suppletion only as a last resort. Of course, this is only a description of what a theory of learning needs to do – it requires exact formalization. To summarize, GIMF PhC modules offer two ways of dealing with a morpheme's morph variation: through alternations and phonologically-conditioned suppletion. Putting aside suppletion, in all extant GIMF PhC theories alternations provide insight into underlying forms and constraint rankings/rules.

So far, this chapter has merely discussed what is already commonly known: that phonotactics and alternations are evidence for theories of the PhC. However, what's really of importance is to show *why* phonotactics and alternations are evidence. They're evidence because within GIMF there's no other module that can generate phonotactic patterns and morpheme alternations. A practical benefit of asking this question is that it leads to an argumentation generalization for those who work within a theory that subscribes to a GIMF framework (e.g. classical/innatist OT, SPE, LPM, etc.). The principle assumes a 'straightforward' phonetic interpretation where the phonetic translation preserves all phonological distinctions; it will be revised after considering the phonetic component in this section.

(1) *GIMF PhC I→O argumentation ('straightforward phonetics' version, revised below)*

Non-suppletive alternations generated by an individual grammar of an individual speaker provide evidence for input→output mappings (and from these mappings, for the form of constraints and ranking).

- An 'alternation' of a morpheme refers to different realizations ('morphs') of that morpheme in different phonological environments.

Depending on the theory, there may be other ways to determine input→output mappings apart from (1). Individual theories may allow non-alternating phonotactics to provide evidence for /α/→[β] mappings. However, (1) is what's common to extant theories committed to GIMF. It is worth noting that arguments for input→output mappings made without the support of alternations are not uncommon, usually without explicit explanation of how the non-alternating forms provide evidence for those mappings.

4 Phonetic Influence

The GIMF phonetic module is significantly non-trivial: there is no simple 1:1 relation between phonological outputs and their phonetic realization. Multiple articulatory strategies may be used to realize the same phonological structure, even within the same language (e.g. Kingston 2007 for an overview). Consequently, synchronic alternations and phonotactics are not always perfect evidence for phonological structure because the phonetic component(s) may obscure the observable form of phonological outputs. The idea that the phonetic module can obscure phonological form is expressed in GIMF in that (a) phonological representation is distinct from phonetic representation and (b) the phonological output is not straightforwardly recoverable from the phonetic output (e.g. Keating 1988; Kingston 2007).

There has always been recognition of the difficulties that a non-trivial phonetic module poses for recovering phonological structures. For example, Chomsky and Halle (1968:110-111) note that there are both phonological and phonetic 'vowel reduction' processes; the phonological process results in a phonological output with unstressed vowels as [ə], while the phonetic process takes fully specified unstressed vowels and realizes them as more centralized than their stressed counterparts. Similarly, Cho and Keating (2001) identify phonetic fortition processes, while Bye and de Lacy (2007) discuss phonological fortition. Some ways in which phonetic interpretation can obscure the form of phonological outputs are sketched in (2).

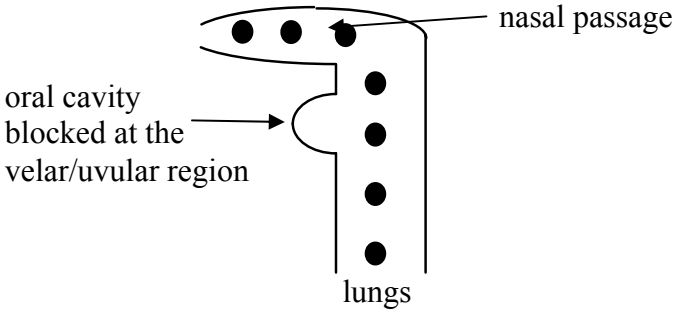
(2) *How can phonological structure be obscured?*
- (a) *Phonetic neutralization:* Two different phonological symbols are phonetically realized in the same way (e.g. voiced and voiceless epiglottal plosives both realized as voiceless – Ladefoged and Maddieson 1996).
- (b) *Phonetic non-realization:* A phonological symbol/feature has no phonetic realization (see below).
- (c) *Phonetic epenthesis:* Part of the phonetic output does not stand in a direct relation to any part of the phonological output (e.g. interpolation in intonation, 'intrusive' segments) (e.g. Ali et al. 1979 and many others).
- (d) *Phonetic deletion:* A phonological symbol's phonetic realization is 'overwritten' by other segments' (e.g. overlap – Browman and Goldstein 1995).
- (e) *Phonetic allophony:* A phonological symbol has different phonetic realizations, either in different languages, or in different environments in the same language (e.g. English [voice] – Kingston and Diehl 1994; domain-final lengthening causes duration allophony in vowels).
- (f) *Phonetic transference*: A symbol is not realized where it is specified in the phonological string (e.g. /ʔ/ in Pendau is realized as creaky voice on a preceding vowel – Quick 2004; [voice] in English coda stops is realized as lengthening of the preceding vowel; tone is often realized on segments after its phonological sponsors ('late realization')).
- (g) *Phonetic assimilation:* i.e. anticipatory/perseverative coarticulation.

Foot heads and stress provide a good example of phonetic allophony and non-realization. Foot heads can be realized as any, none, or all of raised F_0 (perhaps in some cases lowered F_0 – Gussenhoven 2004), increased duration, and increased loudness (Hayes 1995§2.1). Cairene Arabic provides a rather remarkable case where the word's head syllable has a phonetic stress realization while other foot heads do not (Allen 1975, McCarthy 1979, Hayes 1995§4.1.3). The location of main stress can only be identified by building quantity-sensitive trochaic feet from left to right: e.g. [(ˌʔad)(ˌwi.ja)(ˈtu.hu)] 'his drugs (nominative)', [(ˌka.ta)(ˈbi.tu)] 'she wrote it', [(ˈka.ta)ba] 'he wrote', [(ˌʔin)(ˈka.sa)ra] 'it got broken'. Without non-head feet, it would be impossible to predict whether main stress would fall on the penult or antepenult (cf. Crowhurst 1996, de Lacy 1998). However, the phonetic stressing is ||ʔinkˈasara||, not *||ʔˈinkˈasara|| – syllables that head non-head feet have no phonetic stress realization (||*x*|| indicates the phonetic realization of [x]). Consequently, the phonetic output does not provide direct evidence for foot structure.

Chomsky and Halle (1968:311) identify a case of phonetic neutralization: two different sets of features (i.e. [+anterior, –coronal, +back, +high] and [–anterior, –coronal, +back, +high, +round]) are phonetically realized as labiovelar. As another example, I have argued elsewhere for phonetic neutralization with glottal and velar nasals. There are nasal stops with a phonologically *glottal* Place of Articulation – symbolized as [N] (de Lacy 2002, 2006a,b). However, the [glottal] feature is interpreted as requiring an absence of consonantal constriction downstream from the sound source (de Lacy 2002, 2006b§2.2.1.1; adapting Ohala and Lorentz 1977). It is implemented by making an obstruction at the soft palate, as shown in diagram (3). After Ohala and

Lorentz (1977), the diagram shows the airflow through the oral and nasal cavities. The oral cavity is blocked at the velar/uvular region, so air is forced to go through the nasal cavity. The black dots indicate the air's path from the lungs out through the nasal passages.

(3)



Velar stops are made in much the same way – with a consonantal constriction at the hard palate – but for entirely different reasons: they have an articulatorily-defined target to achieve. Consequently, distinct phonological symbols velar [ŋ] and glottal [N] are phonetically realized in a similar way: they are phonetically neutralized to ||ŋ||.

Consequently, the only evidence that there are phonological glottal nasals is from their effect on other phonological elements. For example, assimilation shows that there are glottal nasal stops in Yamphu. Oral stops become [ʔ] before another glottal: e.g. /modok-ha/ → [modoʔha] 'like those' (p. 48), *[modokha]; /læːt-he-ma/ → [læːʔhema] 'to be able to do' (Rutgers 1998:48). Nasal stops also assimilate to glottals, and the result is glottal [N]: /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 as one would expect stop assimilation to produce a [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].[4]

[N] behaves like other glottals in triggering processes. 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. [faʔal]/[ja-fʕal] 'do', *[faʔil] (McCarthy 1994). The generalization holds for uvulars [ʁ χ], pharyngeals [ʕ ħ], 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. [feᵘN] 'fine'), but becomes moraic when a consonant follows (e.g. [fɛᵘNᵘd͡z]) 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] 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 coronals (e.g. Korean – de Lacy 2006a§3.3.2).

[N] has the same distribution as other glottals. It is common for glottals to be banned from onset position. For example, Chamicuro does not allow [h] in onsets (Parker 1994); 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. 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 (§3.2.3, Goldsmith 1990, Beckman 1998). 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 behavior mentioned above (Ghini 2001§5.1). Other evidence for the glottal nasal [N] is provided in de Lacy (2006a:39-42).

The implication of phonetic non-realization and neutralization is that the effect of a phonological symbol on its environment plays an essential part in establishing the validity of many phonological arguments. To take an extremely conservative stance, reliance on phonetic realization as evidence for a phonological symbol/mechanism is in many cases inadequate. Of course, specific theories of the phonetic component place strong limits on phonetic realization; [t] cannot be interpreted as ||m|| (though cf. Declarative Phonology – Scobbie et al. 1996). Phonetic realization alone can therefore provide a range of possible phonological structures, but to narrow the options down further it is necessary to provide evidence from environmental interaction.

The method of providing evidence for a phonological element from its interaction with other elements is currently sporadic in phonological descriptions and analyses. For example, a large number of descriptions of foot structure rely entirely on phonetic evidence for locating foot heads and not on the foot's effect on other phonological elements (e.g. on vowel allophony, etc.). Relying on speech output alone to provide evidence for a phonological symbol is of course predicted by the theory to be acceptable if it can be shown that no other cognitive component (e.g. none of the processes in (2)) has interfered. Given that it is unknown (at least to me!) whether (2) is exhaustive and what the range of possible realizations of a given segment is, a methodologically conservative approach is to insist that evidence from environmental interaction be provided along with phonetic evidence for any assertion about phonological structure. Again, this is an ideal; in some cases there may be no environmental interactions to bolster the evidence for a particular phonological structure. In such a case, strictly speaking there is no choice but to acknowledge the ambiguity of the evidence.

(4) *GIMF PhC I→O argumentation (revised)*
(a) To show that /α/→[β], evidence must consist of non-suppletive alternations generated by an individual grammar of an individual speaker.
(bi) It should be demonstrated that the phonetic module has no (or irrelevant) influence.
(bii) Evidence from environmental influences should be supplied.
(c) The implications drawn from phonotactic evidence should be appropriately limited, as determined by the theory (e.g. many different rankings can account for the lack of coda [k] in OT).

A broader implication for the analyst is that the aims of phonological descriptions need to be carefully scrutinized before using them in service of a PhC theory. Many descriptive works do not aim to provide an account of the outputs of an individual grammar of an individual speaker; consequently, for the PhC-theorist translating the grammar's claims into evidence for the PhC is often a non-trivial task.[6] A related implication is that following the principles in (4) rigidly means that the amount of reliable information that the analyst can use is currently *extremely* small: very often descriptions pool information from different speakers, rely entirely on phonetic evidence alone for phonological structures, and make assertions about underlying forms and processes that rely on a particular theory not shared by the analyst.[7]

5 Perceptual Influence

The GIMF perceptual process is significantly non-trivial. The speaker's phonological output is translated into phonetic form, converted to articulation, and the resulting sound then passes through the physical medium; the hearer must segment the acoustic signal and match it to phonetic categories, then convert the phonetic categories into a phonological representation, which is then matched with lexical form. So, between the speaker and the hearer lie a number of opportunities for the speaker's phonological output to be obscured to the hearer – i.e. the speaker's phonological output may be inaccurately reconstructed by the hearer. The challenges posed by the perceptual system are clear in loanword adaptation – a commonly used source of evidence for the phonological module.

There has been a recent upsurge in interest in the role of perception on loanword adaptation, including Silverman (1992), Yip (2002), Kenstowicz (2003), Peperkamp and Dupoux (2003), Broselow (2004, 2006), Paradis and LaCharité (2005), C. Ito et al. (2006), Hsieh et al. (2006), Smith (2006, this volume) and others. This section can't hope to do justice to the proposals and analyses in these articles. Instead, it examines what GIMF with a non-trivial phonetic component says the PhC must and may not be responsible for in loanword adaptation.

Decoding the acoustic signal into a phonetic representation is a non-trivial process that may result in loss (or gain) of information in relation to the speaker's intentions. The hearer must determine which part of the acoustic signal is actually speech, and which aspects of the acoustic signal are significant in phonetic segmentation. Languages may differ in this – some features of the signal may be ignored because they never figure

contrastively (with the result that non-native contrasts may be hard or impossible to perceive (e.g. for me the difference between [ɪ] and [ə])). As a practical example, how does a Māori (Polynesian; New Zealand) hearer process the New Zealand English (NZE) phonetic output ||saɪn|| 'sign'? One challenge for the Māori is that ||s|| is not an output of his/her language. The Māori needs to discover the speaker's phonetic intention in producing ||s||, but faced with the fact that ||s|| is not a phonetic realization associated with the Māori's phonetic and perceptual system, it would be consistent with GIMF to expect that only part of ||s|| is treated as salient for speech: i.e. part of the acoustic signal is ignored. The part that is not ignored is sufficiently close to ||h|| that it gets perceived as ||h||. In other words, the hearer's phonetic representation may differ from the speaker's phonetic output as a side effect of focusing on the language-specific significant parts of the speech signal.

Even if the hearer can correctly interpret every aspect of the speaker's phonetic speech output as speech, there is another major hurdle to overcome: the conversion of phonetic into phonological representation. As mentioned above, the same phonological symbol can have a range of phonetic realizations in a particular language, and the realizations of the same phonological symbol may differ. For example, Māori has a range of phonetic realizations of phonological [h], ranging between ||h||, ||x||, and ||ʃ|| (i.e. there's free variation in dorsal-palatal constriction) (Bauer 1993). In contrast, NZE [h] is consistently ||h||. Consequently, if a Māori says ||ʃ||, another Māori will perceive it as ||ʃ|| and then map it to phonological [h]; if a NZE speaker says ||ʃ||, another NZE speaker will map it to [ʃ] because ||ʃ|| is not in the range of phonetic realizations of [h]. However, if a *Māori* hearer perceives NZE ||ʃ||, the Māori perceptual system will map it to [h]. Consequently, NZE [ʃ] is uniformly borrowed as Māori [h]: e.g. NZE [ʃip] 'sheep' ~ Māori [hipi]; NZE [ʃu] 'shoe' ~ Māori [huː].

Another challenge is that the same phonological symbol can be phonetically interpreted in different ways in different languages (Kingston and Diehl 1994). For example, English word-initial [b] (like other word-initial [+voice] stops) is interpreted as voiceless and unaspirated (i.e. ||p||). In contrast, Māori [p] is realized as voiceless unaspirated (||p||) (the same is true for [t] and [k] as ||t|| and ||k||). So, Māori [pɪɾipɪɾi] *Acaena anserinifolia* is phonetically realized as ||pɪɾipɪɾi||; when English speakers heard ||pɪɾipɪɾi||, they converted it to phonological [bɪɾibɪɾi] (thus New Zealand English *biddybiddy*).[8] This loanword adaptation of Māori [p] to NZ English [b] is therefore 'pre-phonological' – it is a result of the different mappings between the phonological and phonetic representations in Māori and English.[9]

So, there are many opportunities for pre-phonological adaptation of a speech signal by a hearer. Since the perceptual mechanism can have something to do with loanword adaptation, the remaining issue is *how* responsible the perceptual system can be (something that's not going to be resolved here). After all, the PhC certainly could be responsible for some adaptations – the theory does not prevent the PhC from influencing loanword form: if a loanword is correctly perceived and assigned an input phonological representation that cannot be faithfully produced in the speaker's grammar, the grammar would be responsible for that aspect of the adaptation. The challenge is that until there is a full understanding of the power of the perceptual component to adapt foreign sounds to

fit the language's expected sounds, there are very few sure ways to figure out the extent of influence of the PhC on loanword adaptation. However, the theory does provide a couple of ways to tell.

One way is to see what sort of PhC capabilities a particular loanword adaptation would require. In the Māori [p] to NZE [b] adaptation, a phonological account would require a mapping of /p/→[b]. Such across-the-board stop voicing is unknown in synchronic alternations (cf. intervocalic voicing).

Another way is through alternations. For example, NZE [saɪn] 'sign (verb)' was borrowed into Māori as [haina]. Is the appearance of final [a] due to a pre-phonological misanalysis (e.g. Peperkamp and Dupoux 2003), or a phonological process?

Happily, Māori has a number of underlyingly consonant-final roots. For example, /hopuk/ 'catch' surfaces as [hopu] on its own, but as [hopuk-ia] in the passive and [hopuk-aŋa] in the gerund (see de Lacy 2003 and references cited therein). A problem with putative /haɪn/ is immediately apparent: it should undergo *deletion* like other consonant-final roots: i.e. /haɪn/ doesn't map to *[hai].

One way out of this problem is to propose a loanword-specific faithfulness constraint (e.g. loanword-MAX); this constraint would block deletion in loanwords, and so allow epenthesis as a back-up strategy. However, this solution also fails. If the passive suffix is added to /haɪn/, the result would be /haɪn-ia/ → *[hainia]; however, the attested form is [haina-tia] (the [tia] allomorph of the passive is used for long roots with *underlying final vowels* (also the gerund: [haina-taŋa] 'signature', *[hain-aŋa] – Ngata 1993)). It is also not possible to argue that the first adopter of the word had underlying /hain/, and other speakers mislearned it as /haina/: Māori speakers have no difficulty learning underlying consonant-final words, and the very high frequency of passive forms gives them ample opportunity to do so.

'Sign' is merely one example of a general pattern: *all* English consonant-final words borrowed into Māori take the allomorph of the passive and gerund that attaches to underlying vowel-final verbs (Hale 1968, Blevins 1994:41). It is clear from alternations that no English consonant-final word is underlying consonant-final in the Māori lexicon; therefore, there is no evidence that word-final loanword 'epenthesis' is phonological.

To hammer the last nail into the coffin, English words that end in a C_1C_2 are adapted in Maori as C_1V: e.g. NZE [ˈʔiːdʒəpt] 'Egypt' is adapted as MAO [ˈiːhipa], NZE [ˈsɜːvənt] ~ Māori [ˈhaːwini], NZE [ˈtæks] ~ Māori [ˈtaːke]. The perceptual account would say that the Maori perceptually ignore the final consonant: their perceptual mechanism discards the acoustic cues to the final consonant as non-speech or irrelevant speech sound. The phonological account requires a deletion mechanism, with a UR for 'tax' as /tæːks/ (or /taːkh/), while the perceptual account says that it is [taːke]. Again, alternations provide a way to determine the difference. The UR of 'tax' with the passive /taːkh-ia/ should surface as *[taːkehia]; it does not: it takes the form of the passive used with underlying V-final words: [taːke-tia]; [h] does not appear (e.g. [ko toː utu teːnaː i aːhei kia taːketia i muri iho i ŋa taŋohaŋa katoa] "That is your taxable income after all deductions have been made (Ngata 1993).

The only way to maintain a phonological analysis would be to propose that output-output faithfulness specific to loanwords accounts for their invariant shape under affixation (i.e. loan-OO-FAITH » IO-FAITH » OO-FAITH). However, this analysis predicts

that there should be at least *some* language in which alternations show that the underlying form of loanwords has the same shape as the source language form (i.e. IO-FAITH » loan-OO-FAITH, OO-FAITH); this would show that the phonological component is crucial in loanword adaptation. I have been unable to find clear cases so far, suggesting that the majority of loanword adaptation is due to perceptual mechanisms.

One final alternative to consider is that the Māori speaker does not store the perceived form [haɪn] as /haɪn/, but instead passes it through the phonological system and lexically stores the output. The problem with this approach is as noted above: the phonological component would generate *[hai] from input /haɪn/, not the attested [haina].

To summarize, GIMF with a non-trivial perceptual system and phonetic module predicts that the phonological component is not responsible for all loanword adaptation. GIMF predicts that it *is* responsible for alternations, so the way to show that the phonological component has determined the form of loanword adaptations is through alternations. Although GIMF predicts that the PhC *could* be responsible for some loanword adaptations, the current limited understanding of the extent to which the perceptual mechanisms may influence loanword form means that it would be reasonable to err on the side of caution in the sense that every claim that loanword adaptation is due to phonological mechanisms should ideally be accompanied by an alternation of a loanword to verify its underlying form. A little less stringently, if no phonetic/perceptual motivation for a particular loanword adaptation can be found, then there's the beginning of an argument that the PhC must be responsible; however, the clincher would be providing an alternation.

The influence of the perceptual system is also relevant in diachronic change and language acquisition. Proto-Eastern Polynesian *t became Hawai'ian [k] (Clark 1976), but does this mean that there was a speaker who had /t/→[k]? By no means: the sound change could have been actuated through a misperception (e.g. Blevins 2004, Ohala 1983). At the time of the *t→k change, pre-Hawai'ian had the stops [p t ʔ]. If there was variation in phonetic realization of the coronal stop between ||t|| and ||k||, the sound change could have been a matter of the learner analyzing the phonetic symbol behind the variation as [k] rather than [t]. The fact that a speaker has α and a learner β does not necessarily mean – in the theory – that the learner has /α/→[β]. In fact, it is telling that there is *no* case of synchronic neutralization in which /t/→[k], eliminating the possibility of an account that relies on the PhC (de Lacy 2006a).

There are probably cognitive modules other than the perceptual system that could influence speech sound. For example, a 'paralinguistic' module could alter the phonetic output to signal emotion by altering pitch range, vowel length, degree of VOT, and so on; see Ladd (1996§1.4) for discussion. There may also be a symbol manipulation module that is used in language games to alter phonological structure in fairly unconstrained ways (as in games which reverse the order of segments – a process not attested in other morphological/phonological processes). No doubt an orthographic module relates lexical items to the hand movements that produce text (through writing/typing). The issue, of course, is one of responsibility: a theory of the PhC will identify which phenomena it is responsible for, but other cognitive components may also influence those phenomena. Consequently, only after examining other cognitive components is it clear what the 'extent' of the PhC's responsibility for particular phenomena is.

The immediate methodological implication is that using loanword adaptation to argue for GIMF PhC mechanisms is a non-trivial and challenging task. To be sure that a particular adaptation is due to the phonological component, it must be shown that it is not a pre-phonological adaptation.

6 External influences

Typological frequency is often used to motivate proposals about GIMF PhCs. Two types of frequency must be distinguished here (as in traditional work on universals): absolute (implicational) universals and universal tendencies. An absolute universal is all or nothing: α is either present or absent in every language; absolute implicational universals work the same way: if α is present then so is β in every language (e.g. if a language has a default epenthesis of stops, it will be [ʔ] or [t] – de Lacy 2006a:79-109). Universal tendencies are just that: if a language has α it may also often have β, but sometimes may not (e.g. if a language has a [g] it will also probably have a [b], but a few [g d] and [g] inventories exist – see below). The discussion below focuses first on universal tendencies because they illustrate most clearly the role of external influences.

Many GIMF theories of markedness have interpreted the universal tendency that [t] is very common in segmental inventories to mean that the PhC favors [t] over other stops. In representational approaches, [t]'s high frequency has been taken to mean that it is representationally less complex than other segments (Paradis and Prunet 1991); in

constraint-based theories of markedness, [t]'s violations are always less significant (through fixed ranking of *k, *p above *t), or a proper subset of other consonant's violations. However, what is the extent of the GIMF PhC's responsibility for typological frequency? Are other modules and factors involved?

Certainly, external factors *can* influence language typology. War, pestilence, and plague can affect populations, and even wipe out entire languages (e.g. the genocide of various Native American populations in the 19th and early 20th centuries – Brown 1970); a result is a loss of linguistic diversity. Similarly, invasion increases language contact, and borrowing can reduce typological diversity. For example, the Pacific nation of Tonga invaded a number of Pacific islands in the 18th century. Through language contact it left behind its stress system so that the number of languages with strictly right-aligned trochees (e.g. [ma(áma)], [pa(káta)]) increased relative to those with non-strictly right-aligned trochees (e.g. [(máa)ma], [pa(káta)]) (e.g. Pukapuka – Salisbury 1993). One would hope that such external effects could be minimized by taking a genetically diverse sample with a large number of languages. However, there is no guarantee this is the case. The world's languages are very closely related (most are Austronesian, Niger-Congo, and Indo-European), and there is no reason to think that their precursors represented a well distributed sampling of possible grammars.

Another important influence is diachronic change. Some languages are not possible to actuate or transmit for functional reasons. For example, no language has just one consonant and one vowel – such a language would be communicatively impossible for many reasons (e.g. memory limitations would prevent hearers from distinguishing a word with 12 syllables from one with 13, etc.). No language uses every symbol in the IPA chart – such a language would make grotesquely inefficient use of its segmental resources.

As Blevins (2004) and others have observed recently, pressures in diachronic change can also favor some types of language over others. Blevins observes that [g] is more perceptually confusable than [b] and [d]; there are also production and aerodynamic difficulties with [g] (Ferguson 1975, Ohala 1983; velars induce a strong build-up of intraoral pressure which inhibits voicing). Consequently, if any voiced stop was to be altered through misperception (or to avoid excessive articulatory effort) by a learner, it's likely to be [g]. The typological result is that of the voiced stops, [g] is the most likely one to be missing in a language. So, there is no need to appeal to properties of the GIMF PhC to account for the lack of desirability of [g] in inventories.

It's clear that typological frequency can be influenced by factors other than the PhC. So what aspect of typological frequency is the PhC responsible for? Continuing with the focus on voiced stops, what is definitely relevant for the PhC is the fact that *all* imaginable voiced stop inventories exist (limiting to just [b d g]):

(5) *Voiced stop inventories (limited to [b d g])*

g	b	d	*Languages*
✓	✓	✓	Nhanda (Blevins 2001), Catalan (Wheeler 2005)
✓	✓		Tigak (Beaumont 1979)
✓		✓	Wapishana (Tracy 1972), Ayutla Mixtec (Pankratz and Pike 1967)
	✓	✓	Sioux Valley (Santee) (Shaw 1980:17), Xavanté Macro-Jê (Rodrigues 1999a)
✓			Makurap (Rodrigues 1999b:112ff)
	✓		Koasati (Kimball 1991)
		✓	Diyari (Austin 1981), Nambiquara (Kroeker 1972)
			(No voiced stops) Māori (Bauer 1993)

GIMF PhCs are responsible for phonotactic outputs of this kind (see section 3.1), so it must be the case that the PhC can *generate* each type of voiced stop inventory. The ability to generate attested languages is the minimum required of the PhC. Is it also the maximum required? Does the PhC have *anything* to do with inventory tendencies?

It is not obvious that it must. The GIMF PhC generates the phonological part of grammars, but does not inherently make any claim about the frequency of those grammars in languages. It is quite possible that the PhC has nothing at all to say about universal tendencies – pattern frequency is entirely determined by external factors and pressures in diachronic change, as outlined above.

Consequently, a challenge for those who wish to use universal tendencies to determine properties of the PhC is that all external factors and diachronic influences must be eliminated from the tendency under examination. This is an immensely difficult issue because it is hard to measure the potential influence of some of the external factors above – it requires knowing a great deal about a language's social history. There is also no guarantee that the languages attested in the world represent an even distribution of all possibly generable languages. The ancestors of current languages may have represented only a small and skewed range of possible languages, but their influence is still felt. Consequently, getting a typologically diverse sample of languages is no guarantee that the PhC's capabilities are represented in any reasonably distributed way.

However, Elliott Moreton's recent work (2004, 2006, this volume) provides a potential way forward. Moreton constructed artificial languages with different phonological processes which have phonetic motivations of equal magnitude. He then determined how easy the two languages were to learn. People were able to learn (i.e. have judgments about) one language much more easily than the other. Since the languages had no 'ancestry', and their phonetic precursors were identical, the learning bias must be due to cognitive (i.e. PhC) effects. The general strategy employed by Moreton is what is needed to determine the cognitive contribution to universal tendencies – i.e. eliminate the effects of language change, performance, and non-phonological modules; whatever's left must be due to the PhC.

Unfortunately, there are uncertainties with the approach used by Moreton. Moreton (p.c.) observes that it is as yet unclear how lab-based learning of artificial languages relates to learning of natural languages. The learners are adults, not pre-critical period children, and unlike natural languages the artificial languages have no semantic content (like glossolalia – de Lacy to appear). The relation between the natural-

language-learning process and the artificial-language-learning process may be a complex one. In any case, Moreton's approach is significant: it offers the promise of being able to eliminate external factors that cannot be easily (or ever) eliminated when dealing with actual language typology.

Moreton's work makes it even more imperative to eliminate all PhC-external influences on typological frequency before attempting to evaluate the PhC's role. A statement like "[t] is present in 98% of currently attested languages" is worthless in judging the PhC's capabilities unless the effects of PhC-external influences are taken into account. In this case, there are clear functional biases towards [t] involving ease of articulation and perceptibility. Once these PhC-external biases are considered, there may be no role for the PhC to play; it might even be possible that [t] is *under*represented!

By no means am I suggesting that the PhC could not be adapted to influence typological frequency. The PhC *could* be adapted to have influence on universal tendencies. Purely formal biases could be introduced into the learning process to favor the actuation or transmission of some grammars over others. Moreton (2007) proposes a method of doing so (also see Coetzee 2002). What I am merely pointing out is that at the moment typological frequency is not straightforward evidence for the PhC's structure. How do we tell what the cognitive contribution is to typological frequency since external factors interfere so significantly? If Moreton's approach can be perfected, it will provide a way to tell. At the moment, however, it's impossible to be sure. For example, while functional factors make one expect fewer languages with [g] than with [b], once those factors are eliminated is [g] actually *more* frequent than one would expect, relative to [b]? If so, then there's room for a cognitive explanation; at the moment, we don't know whether there's anything for the PhC to account for.

Absolute universals are slightly easier to deal with. If a particular pattern is never present (or always present – e.g. CV syllables), it is possible in some cases to determine whether the PhC is responsible. If α never occurs but there are good 'diachronic' motivations for its occurrence (e.g. good reasons to actuate it), then the PhC must be responsible for its absence. This point has been argued for the unattested epenthetic [k] (de Lacy and Kingston 2006), lack of 'selective' coda stop devoicing (de Lacy 2006b), lack of stress systems with attraction to schwa (Kiparsky 2004), no word-final obstruent voicing (Kiparsky 2006), absence of stress systems attracted to low tone or higher vowels (de Lacy 2006b), and lack of systems in which vowel height is affected by consonant voicing (Moreton 2006). In all of these cases, there are good functional reasons for actuating such a process in diachronic change, yet they do not occur. In at least some of the cases, it has been shown that there was a series of diachronic changes that made actuation of the property almost inevitable, yet the property did not develop.

Of course, if there *is* a good diachronic reason why a phenomenon may never occur, it is then hard to argue that the PhC prevents it from happening. For example, Myers (2002) argues that clusters of a nasal stop followed by a voiceless consonant are never altered by epenthesis because there is no robust phonetic motivation that would cause the cluster to be misperceived with an intervening vowel, and thereby actuate a sound change. In this case, it is not easy to determine whether the PhC also actively prevents epenthesis as a response to N$\underset{\circ}{\text{C}}$ clusters. Of course, it is not necessarily the case that the PhC *allows* epenthesis in this situation (cf. Blevins 2004:237; de Lacy and

Kingston 2006§4), but it is not obvious that the *lack* of NC̥ epenthesis is something the PhC's must account for.

To summarize, it is by no means straightforward to argue from typological frequency facts to a claim about the GIMF PhC. External factors and the learning process significantly obscure the cognitive contribution. In some cases with absolute universals, the external factors can be mitigated. With universal tendencies the challenge is much greater. Consequently, many arguments from typological frequency for the PhC must be treated with keen scepticism as they frequently fail to take external factors into account. The way forward is for every typological frequency fact to try to eliminate external influences, and see what is left for the PhC to account for. Moreton's work may open up a new way forward in this regard.

The discussion above has focused on typological frequency, but there are many other types of tendency. For example, 'corpus frequency' is about how frequently a particular sound pattern shows up in a selection of spontaneous speech; 'lexical frequency' is about sound patterns in a person's lexicon, and so on. Many of the same issues with typological frequency arise with these other types of frequency. Altshuler (2006) provides an example from Osage. Osage has lexical stress: some words have primary stress on the initial syllable while others have it on the pen-initial: e.g. [bágа] 'burr' cf. [nãːxó] 'break by foot'. Altshuler (p.c.) reports that there are more initial-stressed words in the Osage lexicon than ones with peninitial stress. From this fact, a common conclusion would be that the words with peninitial stress (i.e. the less frequent ones) have underlying (lexical) stress. However, there is nothing inherent in GIMF theories of the PhC that leads to this conclusion. The PhC theories must account for both stress patterns: i.e. that words with initial stress and peninitial stress are generated. There are two ways to deal with this pattern (apart from lexically-specific constraint rankings): either default stress is initial and all peninitial stresses are lexical, or default stress is peninitial and all initial stresses are lexical (in OT the fact that default stress is initial does not prevent there from being lexical items with underlying stress on the initial vowel). As it turns out, Altshuler argues that other phonological processes show that default stress is peninitial, and so initial stress is marked lexically.

7 Evidence in GIMF theories of the PhC

In some analyses, a 'data-oriented' approach is taken: a speech sound phenomenon is identified, the PhC is assumed to be responsible, so a PhC theory is proposed. In many cases, the assumption is benign in that the PhC theory being used demonstrably (or at least reasonably) *is* responsible. However, a danger is that the analyst could be trying to use a GIMF PhC theory to account for data that some other module (or external effects) is entirely or partially responsible for. Another subtler problem is that the data-oriented approach guarantees that some PhC capabilities will be extremely hard – perhaps impossible – to discover because no analysis has either encountered the relevant phenomenon or on encountering it thought to ascribe it to the PhC.

Some examples of the dangers of the data-oriented approach to theory building are found in work on markedness. A frequent strategy since the Prague School (e.g.

Trubetzkoy 1939) and Greenberg's typological work has been to compile cases that have similar tendencies in terms of sound patterns then provide a theory to account for them. For those whose aim is data collection – i.e. taxonomy (e.g. Greenberg 1966) – this approach makes perfect sense. However, for those interested in identifying data that can provide insight into the adequacy of a GIMF theory of the PhC, the data-oriented approach is fraught with danger: it employs an assumption that apparent similarities in phenomena necessarily have a single common source – i.e. the PhC.

7.1 Labial unmarkedness

For a recent example, I focus on labial unmarkedness: the idea that labials can be the least marked of all major Place of Articulation features, at least in some languages. This discussion here focuses on Hume (2003) (hereafter 'H'); the proposal is also advocated in Hume and Tserdanelis (2002). An important caveat: H's theoretical assumptions may not necessarily be set within GIMF. Hume's later and developing work (2004, 2006) assumes a non-GIMF approach to phonology and phonetics. Consequently, the following should be read as the evaluation of a GIMF version of H, not necessarily of H itself.

What does it mean for a feature to be 'unmarked' in terms of the PhC? Work within Optimality Theory over the past several years has equated markedness with constraint violation profiles. So the claim that [labial] is the least marked PoA in a particular language is formalized by having a constraint C (or constraints) that favor [labial] over other PoA features, and no constraint that favors another PoA feature over [labial] outranks C. In more concrete terms, labial unmarkedness could be expressed as a ranking || *dorsal, *coronal, *glottal » *labial ||, ignoring other labial-favoring and -disfavoring constraints.

H presents an archetypal markedness argument for labial unmarkedness. A number of diagnostics that are traditionally accepted as giving insight into markedness are applied to the question of whether labial can ever be the least marked PoA in a language. Some of the diagnostics H discusses are summarized in (6). The references in square brackets are my own: they agree with the claim the particular phenomenon gives insight into markedness. H also discusses deletion and syllabification; for relevant evaluation see de Lacy (2006§8.2.3, 8.7.2).

(6) *Hume's (2003) diagnostics for labial unmarkedness*
- (a) Labials are acoustically less salient than other PoAs in English (Miller and Nicely 1955) and Japanese (Sekiyama and Tohkura 1991) [Battistella 1990, Jun 1995]
- (b) Labials are almost as typologically frequent as coronals (Maddieson 1992) [Greenberg 1966]
- (c) The labial [m] is more frequent in Sri Lankan Portuguese Creole words than [n] [Greenberg 1966]
- (d) The labial [m] can appear in more environments in Sri Lankan Portuguese Creole than [n]
- (e) Labial stops are acquired before other segments in language acquisition [Jakobson 1941, 1949b]
- (f) Labials are the sole undergoers of assimilation in Sri Lankan Portuguese Creole [Kiparsky 1982, Mohanan 1993, Jun 1995, Rice 1999]
- (g) Labials can be the sole segment in a language's coda [Rice 1999]

For present purposes, the central question is whether any GIMF PhC theory is necessarily responsible for the phenomena in (6). There are a group of diagnostics which are simply outside the purview of a theory of Competence: (6a) refers to 'acoustic salience', a concept which is definable within the perceptual modules but not within the PhC; (6b,c,d) are statements about frequency whose relation to the PhC is currently unclear (see section 6); and (6e) is about order of language acquisition, which can be affected by Performance concerns (e.g. labial articulation is more visible than other articulations, so may be easier for the learner to identify).

There are some phenomena that the PhC *does* predict that it's responsible for – i.e. alternations and phonotactics. These include (6f) and (g). (6f) is about alternations: it refers to assimilation; if it is shown that the assimilation is not due to phonetic coarticulation then the phonological issue is valid. (6g) is about phonotactic distribution; if it is demonstrated that there is a language with a coda segment that is phonologically labial, the generalization is valid.

In short, worrying about what the theory predicts is relevant has cut down the potential evidence for labial unmarkedness from 7 to 2 items. One can now ask if (6f) and (g) are really evidence for labial unmarkedness (i.e. for a ranking like *dors, *cor, *glottal » *labial). This is the point at which specific theories of the PhC must be consulted (e.g. de Lacy 2006a§8.2.3). (6g) is unlikely to be a relevant indicator in any PhC theory because it is a phonotactic generalization and does not involve alternations – there is therefore no way of knowing what happens to other nasals, so /m/'s relation to /n/ and /ŋ/ is indeterminable. As an interesting aside, the PhC theory in de Lacy (2006a) predicts that the synchronic alternations of epenthesis and positional neutralization do provide evidence for markedness. However, no language has context-insensitive epenthesis of a labial (except perhaps for [w]), and none has neutralization (with alternations) to labials (de Lacy 2002, 2006a). de Lacy (2006a) also argues that (6f) is not relevant because assimilation is adversely affected by faithfulness.

The surprizing result is that in the light of GIMF, the items in (6) are at best superficially related: the PhC is not responsible for all – or even most – of them. However, here is where the methodological danger is at its most extreme: the phenomena in (6) all superficially *look* like they have something in common: i.e. labial sounds and phonological symbols that are phonetically realized as labial are 'preferred' over other PoAs. The vague commonality could lead analysts to attempt to provide a theory to account for them all. However, this is no more legitimate than randomly choosing *any* subset of (6) and providing a theory for them: the selection of data is not driven by a theory, but rather by intuition.

This point may seem to be an overstated – even unfair – criticism. After all, doesn't the clustering of similar phenomena make it at least *likely* that there's a unified explanation behind them all? There is no doubt that intuitions and hunches like this play an important role in any scientific investigation. However, once intuited, they deserve careful scrutiny because they're not derived from the principles of one's theory. For (6), the phenomena look similar because the different modules that are responsible for them happen to agree on some things: labials happen to be easy to perceive, easy to produce, and stable in diachronic transmission. In contrast, I've argued elsewhere that they are *never* the least marked major place of articulation in any phonological component (de Lacy 2006a). So, the 'labial markedness facts' above illustrate well how non-PhC modules and external effects may favor a sound pattern while the PhC does not.

Tradition is also an acute problem for theories of markedness. Greenberg's oft-cited work on markedness had a taxonomic aim; there was no cognitive theory underlying the taxonomy, so language-related phenomena were not organized on a cognitive basis. However, Greenberg's work has often been adopted wholesale into generative theories with the unfortunate assumption that all his observations and taxonomies are relevant to the grammatical competence. However, from the point of view of GIMF, the phenomena used in Greenberg's work have a variety of sources: the PhC, the phonetic component, the perceptual system, and external influences. The same point has been made for many other traditional terms. Archangeli and Pulleyblank (2006) observe that the traditional term 'harmony' is unlikely to refer to any single formal phenomenon; also see Bye and de Lacy (2007) for 'fortition' and 'lenition', and Gussenhoven (2006) for 'lexical tone' and 'intonation'.

Of course, it's usual – perhaps inevitable – practice to collect data from a variety of phenomena and try and give a unified explanation for it. However, the examples above illustrate the sort of dangers that arise. Without an analysis of the resulting PhC theory and its related modules, there is an acute danger that too much is being asked of it.

A similar issue has arisen recently in terms of theories of free variation (see Anttila 2006 for an overview). Theories differ as to *how much* they should account for. Many recent free variation theories agree that the PhC should be able to generate the free variants of a wordform (Anttila 2006). Theories differ as to whether they should also account for the *relative* frequency of the wordforms (Coetzee 2004, 2006), or the *absolute* frequency of a wordform (as in Stochastic OT – Boersma 1998). Is a theory that provides a way for the PhC to accurately generate grammars in which one of two free variants occurs 92% of the time better than one that only accounts for the relative frequency of wordforms, which are in turn surely better than those theories that merely generate the free variants? By no means. To show that a theory that only generates free

variants and says nothing about their relative or absolute frequency is clearly wrong, it is necessary to show that there is no other cognitive module or external influence that could also provide an account (e.g. could variability in phonetic coarticulation be responsible for some variation?, etc.). Coetzee (2006) puts this point succinctly: "If grammar [i.e. the PhC] accounts perfectly for the observed frequencies, it actually accounts for more than its fair share of the variation. The close fit that is sometimes observed between observed and predicted frequencies in these models can then be a liability rather than an asset… The grammar is but one of the things that determine the frequency of variants." In short, ignoring the limitations of the PhC and the responsibility of other modules can be a great liability – it can lead to proposing unnecessary PhC mechanisms.

7.2 Evaluation

A problem similar to the one encountered above arises in theory evaluation. It's a common technique to identify a phenomenon and see whether a particular PhC theory can account for it. Usually this technique is used to compare PhC theories with the point being that one PhC theory can account for the data while another cannot. Is this a valid evaluation method?

Yes, but only under two conditions. One is that it must be demonstrated that the GIMF PhC theory that is being evaluated predicts that it is responsible for the phenomenon, rather than some other module. If so, then the theory must be examined as to the 'extent' of its responsibility: i.e. are there any other cognitive modules or even external influences that could also account for/influence the data?

These points will be illustrated by examining a critique in Blevins (2006), a synopsis of Evolutionary Phonology (EP – Blevins 2004). EP is a theory of language transmissibility: i.e. why some sound patterns are more likely to survive inter-generational transmission intact, while others rarely survive. EP provides a way to think about typological frequency, such as why so few languages lack a [t] (i.e. because [t] is very stable in transmission as it is unlikely to be altered/misperceived), why final devoicing is common, and so on. Blevins (2006) contrasts EP with Optimality Theory, but the evaluation could equally be applied to any GIMF PhC theory.[10]

(7) *Evaluation of Optimality Theory*
"However, what Optimality theory fails to account for is why certain sound patterns, like final devoicing, are very common, while others, like final voicing, are rare. Factorial typologies, like generative feature/rule schemas, provide a vocabulary for describing sound patterns and alternations, but they offer little of predictive value when we ask why a particular sound pattern occurs where and when it does." (Blevins 2006§3.1)

The critique in (7) is missing a link its argumentation. It does not show that OT predicts that the PhC *should* be responsible for typological frequency generalizations like the ones cited. It also does not show why OT should account for diachronic actuation (i.e. why a "sound pattern occurs where and when it does"). In short, it is not enough to criticize a

theory by showing that it fails to account for a phenomenon P; it must be shown that the theory predicts that it is responsible for P.

On a related point, it is not an easy task to argue that if the PhC *could* account for a phenomenon P it must do so. It is necessary to ask how other modules or external effects influence P. If there is a residue of P that must be accounted for by the PhC, then an argument can be made for altering the PhC to account for that 'residue' of P. As a quick example, many languages have neutralization of nasal place of articulation to (what emerges as phonetically) velar or uvular. GIMF PhCs are capable of dealing with alternations, but *must* it be responsible in this particular instance? Certainly, no other module can effect unfaithful mappings from inputs to outputs, but is the PhC responsible for the output being velar or uvular? As discussed in section 4, the phonetic component can interpret [glottal] in nasals as velar/uvular, so in this case while the PhC module is responsible for an unfaithful input→output mapping (i.e. neutralization), it is not responsible for the interpretation of phonological [glottal] and velar/uvular. If one wishes to argue that the PhC is responsible for the velar/uvular articulation, it must be shown that the output sound is phonologically [dorsal], and is not a phonological [glottal] that has been interpreted as velar/uvular by the phonetic component (cf., e.g., Howe 2004). In an ideal world: it is not enough to show that the PhC *could* be responsible, it must be demonstrated that no other module or external influence could be responsible *and* that the PhC predicts that it is responsible. Of course, demonstrating responsibility is a big challenge, and may seem to simply lead to a lot of qualifications before anything is asserted about the PhC (or any other module). This is probably true (see below for an example), but it's not obvious that careful qualification of the validity of one's reasoning is a bad idea.

I mentioned at the beginning of this chapter that the usual data-oriented approach means that the analyst may miss phenomena that the theory is responsible for simply because he/she has not yet encountered them. Alan Prince (p.c.) observes that this problem arises clearly with 'data-driven typologies': when a slew of (intuitively) related data is collected, and then a theory is provided to account for it. The theory-builder might receive hints and inspiration from looking at the data, but without then examining the theory in its own right for what it predicts should exist, there is great danger. This danger is evident in some analyses that propose constraints. For example, Lombardi (2003) argues for a set of constraints to account for the set of epenthetic segments she identifies; however, when other constraints (i.e. the theory *in toto*) are considered, the number of epenthetic segments predicted increase significantly (de Lacy 2006a§7.2). As argued above, it is not enough to collect data, hope that it is all due to a single mechanism or module, and propose mechanisms to deal with it. It is necessary to examine a theory for what it predicts it is responsible for, and seek out the data it predicts should and should not exist.

I am concerned that the preceding discussion might be read as an exhortation to do nothing: if one is faced with a speech sound phenomenon that no theory accounts for, or that more than one module could account for, what can one do?

Luckily, this issue is becoming less and less of an issue as theories of the various GIMF modules and understanding of learnability, perception, and external effects has increased significantly. However, until a comprehensive Theory of Every(speech-sound)thing is developed, it will always be a potential problem for theories of the PhC.

A first step is to be sure that the theories of speech-related cognitive modules (i.e. phonetics, perceptual mechanisms, etc.) and related external effects have been adequately examined. Understanding of speech-related modules and mechanisms has advanced enough now that they can be seriously examined in relation to many speech sound phenomena. In some cases, inadequate examination has persisted until quite recently. Typological frequency is a relevant case: it has been common to ascribe the frequent appearance of [t] in languages as evidence for a PhC mechanism that favored coronals over other places of articulation. The contribution of its ease of actuation and transmission in diachronic change to understanding typological frequency has only recently received serious attention, even though the pressures involved have been understood for some time (e.g. Ohala 1983).

The step above would identify modules that could be responsible for the phenomenon and those that could not. It has been good practice to acknowledge such discoveries, as Chomsky and Halle (1968:11) do:

> "Since other aspects of performance have not been systematically studied, our attempt to delimit the boundary of underlying competence by providing specific rules for vowel reduction must be taken as quite tentative. When a theory of performance ultimately emerges, we may find that some of the facts we are attempting to explain do not really belong to grammar but instead fall under the theory of performance."

This acknowledgement is different from acknowledging the possibility that *anything* could be responsible. Chomsky and Halle identify *particular* sources of potential explanation here (i.e. in slightly updated vocabulary – the phonological and phonetic modules). The GIMF theory will more often than not exclude some modules as sources of potential explanation. The value of giving disclaimers – i.e. of identifying potentially responsible modules as in the quote above – is that it provides an easy way to re-evaluate old proposals as theories progress, just as Chomsky and Halle's acknowledgement underscores the point that their theory does not *exclude* the possibility for a phonetic module to produce the effect observed as 'vowel reduction'. To put this point another way, proposing a PhC mechanism to deal with a particular phenomenon without acknowledging the role of other modules makes an implicit claim about responsibility: i.e. that no other module could have any effect (e.g. see Coetzee's critique of free variation theories above). For example, if a PhC theory accounts for typological frequency within a high degree of accuracy, it makes the implicit claim that external effects, learning, and diachronic change can have *no* (discernible) effect. If this is indeed the claim, then acknowledging it is crucial.

The final step is to modify the GIMF theory of the PhC. At this point, the GIMF theories need to be re-examined to see what they predict their modules are responsible for.

A final issue: are all GIMF theories of the PhC immune from criticism?[11] Are theories of the PhC free to pick and choose what they can handle easily and simply exclude any potential counterevidence as falling outside their domain of responsibility? By no means. The discussion in this chapter has simply enlarged the domain of consideration from the PhC alone to the GIMF modules that interact in the speech sound

cognitive system. Every speech sound phenomenon must be accounted for somewhere within this system (or through external effects as discussed in section 6). If it can be demonstrated that the PhC is the only possible module that could be responsible for a particular phenomenon, then it must be responsible. If other modules necessarily influence a certain phenomenon, then the PhC is necessarily not the sole source of explanation for that phenomenon.

If one's current PhC theory cannot account for a particular phenomenon, then either the PhC theory must be changed or it must be shown that some other module is responsible. Given advances in the understanding of GIMF modules other than the PhC, this enlarged focus is a necessity.

So, suppose someone points out that a PhC theory cannot account for a phenomenon P. Either the theory is wrong, or another module is responsible for P. If it can be shown that no other module could be responsible, then the PhC theory is wrong. Otherwise the theory is only potentially wrong; there is then a burden to show that some non-PhC module is responsible for P.

8 Conclusion

This chapter has examined a well developed generative framework and asked what it predicts its phonological module is responsible for. It is clear that the phonotactics and alternations of a single grammar of a single speaker are crucial evidence for the structure of the PhC. However, even this 'best evidence' can be obscured by the effects of other cognitive modules such as the phonetic component and the paralinguistic module. 'External' factors (e.g. influences on learning, the physical structure of the articulatory tract) can further obscure the effect of the phonological output on speech sound.

Recent consideration of the role of non-phonological modules has cast doubt on the relevance of a variety of phenomena for phonology. This is clear for loanword analysis, where a great deal of adaptation may be due to perceptual mechanisms, and perhaps also for some aspects of language games, which often show symbol-manipulation that is not observed in natural language. The influence of external factors means that it is extremely difficult to show that typological frequency necessarily must be explained by the PhC. Many sound-related phenomena were not discussed in detail here, but require the same sort of evaluation, including diachronic sound changes, speech sound regularities in first- and second-language acquisition and aphasia, lexical and corpus frequency, and so on.

In terms of theory-building and -evaluation, the implications identified here are that the technique of identifying a set of data and then finding a theory to account for it, or evaluating a theory with respect to it, poses dangers. In terms of evaluation, it is not adequate to identify data and evaluate whether a theory can or cannot account for it; it is essential to show that a theory predicts that it should account for a set of data before evaluating it with respect to that data. In GIMF, it is also necessary to show that no other module could be responsible for (aspects of) the data.

Methodologically, the implications place a huge demand on the GIMF PhC analyst. Every description must focus on an individual grammar of an individual speaker unless additional theories of inter-grammar interaction are made explicit; data must

therefore be controlled for dialect and register. It is not enough to rely on phonetic realization to argue for a particular phonological structure/segment; the effect of the structure/segment on its environment should also be examined so as to eliminate the obscuring effects of phonetic interpretation. Nonetheless, I doubt that this chapter has added anything that is not already known to phonological analysts working within GIMF: many of the results are accepted as methodologically good principles of analysis anyway. However, the aim here has been to show *why* they are methodologically good principles – they are so because of what the theory claims it is responsible for.

Finally, the focus here has been on GIMF. The conclusions reached here may not have any relevance to non-GIMF theories; this is an issue that needs to be taken up with each separate framework.

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[1] I will assume Optimality Theory's formalism here; the same arguments in general form apply to SPE or any other generative theory that fits the framework in section 2.

[2] I am not criticizing the practices of descriptive linguistics here. Problems only arise when it is assumed that the concerns of descriptive linguistics (e.g. taxonomy) are the same as those of the PhC-linguist (i.e. cognition).

[3] I am using the term 'alternation' here to refer to any pair of morphologically related forms that give insight into the input, as determined by a particular theory (e.g. OT, SPE). Alternations have been contrasted with 'automatic phonological processes', also called 'allophony'. In Prague School phonology, SPE, underspecification theory, and their successors, non-alternating allophony also gives insight into underlying forms through requirements of simplicity and economy of representation. This is not the case in OT: e.g. there is no reason to conclude that English [k^hæt] is underlying /kæt/ rather than /k^hæt/ (see Prince and Smolensky 2004 on Lexicon Optimization). At this level of detail, individual GIMF PhC theories must be consulted for what they predict to be relevant in determining underlying forms. However, they all share enough properties so that useful generalizations can be made.

[4] Could this assimilation be analyzed as having [ŋ] (not [N]), with /n/→ŋ/_ʔ because [ŋ] is the closest thing to a glottal the language has? If so, one would expect a similar situation in some language involving stops: i.e. /at-ha/ → [akha] if [ʔ] was banned. I don't know of any case like this or of any analogous example involving major Place of Articulation. In all cases I have seen, place assimilation results in agreement of major place features.

[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. [ɪŋ.d͡ʒénw] 'naïve', *[ɪŋ. d͡ʒénw]).

[6] I'm not implying that it *should* be the aim of a descriptive work to provide evidence for the PhC. It should be the job of a PhC-theorist to show that all descriptive work appealed to really provides evidence, as defined by the particular theory being used.

[7] I have not discussed the effect of the physical structure of the articulatory apparatus and its obscuring influence on speech sound. For example, the articulators responsible for F_0 modulation cannot keep up with phonetic specifications in fast speech; consequently, F_0 may not reach the depths of a low target between two high targets (e.g. Myers 1999 and others). So, the physical apparatus may obscure phonological outputs.

[8] Other early borrowings reflect the /p/→[b] borrowing: e.g. [ˈpakaɾu] 'broken' > NZE [ˌbʌgəˈɹu], [ˈpuːhoi] {placename} > early NZE [ˈbu(w)ai]. The influence of missionaries, orthography and an upsurge in public interest in Māori pronunciation has overwritten some of the early loans: e.g. [puːhoi] is now said [ˈp^{h}uhoi] (note the medial [h] in an unstressed syllable – a phonotactic impossibility in native NZE words).

Standardization of loanwords by a panel of experts makes recent adaptations close to worthless as giving insight into cognitive processes.

[9] As expected, all NZE word-initial phonological [b] (phonetic ||p||) were adopted as Māori [p] (phonetic ||p||), as both are voiceless unaspirated (e.g. [ˈpia] < NZE [ˈbiə] 'beer', [ˈpaki] < NZE [ˈbʌgi] 'buggy'). NZE [p^h] (phonetic ||p^h||) was borrowed as Māori [p] (phonetic ||p||), too.

[10] I cite Blevins (2007) merely for convenience; I have seen and heard the view echoed many times that generative theories fail because they do not account for various types of frequency generalizations, in particular when comparing formalist/innatist with functionalist theories.

[11] My thanks to an anonymous reviewer for raising this issue.

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