Bermúdez-Otero, Ricardo. 2017. Against the direct lexicalization of phonetic patterns. Online comment on 'Voice-induced vowel lengthening' by Tobias Scheer (*Papers in Historical Phonology* 2). 24 November 2017.

AGAINST THE DIRECT LEXICALIZATION OF PHONETIC PATTERNS, FOR LARYNGEAL EQUIPOLLENCE

(27 November 2017)

In this paper you set out a bold and ambitious argument. I subscribe to some of its premises, but I cannot agree with its far-reaching conclusion. In this comment, therefore, I want to explore some alternative ways of looking at the evidence.

As regards the premises of your argument, I absolutely agree that distinguishing between active and passive voicing and devoicing (Jansen 2004: 36-38) is crucial to understanding laryngeal features and their behaviour. I also agree that presonorant voicing does not involving feature spreading from sonorants (Strycharczuk 2012), and hence does not warrant the expedient of redundantly specifying sonorant consonants as [+voice]. *Pace* Cyran (2014), however, I hold it to be an empirical fact that presonorant voicing can become stabilized as a phonological process, as shown by the criterion of categoricity (Strycharczuk 2012; Strycharczuk & Simon 2013; Strycharczuk, Van 't Veer, Bruil & Linke 2014).

Your conclusion is that a purely phonetic phenomenon can induce lexically specific changes in underlying representations without going through an intermediate stage of stabilization as a categorical phonological pattern. This assertion strikes me as deeply problematic. If direct lexicalization were a real mechanism of sound change, we would expect endogenous lexical splits to be common, whereas they are in fact very rare and typically attributable to contact between different languages or varieties. In fact, countenancing direct lexicalization causes the diachronic predictions of a modular architecture to converge with those of Exemplar Theory, which also predicts frequency-driven endogenous lexical splits (Bermúdez-Otero 2007: 513-4).

In this light, let me now look at the way in which your premises link to your conclusion. Consider, in particular, the Western Slavic sound change stated as (3) in your paper:

(3) o > oo / __C_{+voice}# where C_{+voice} = sonorants and voiced obstruents

On p. 130 you argue that (3) is 'not a possible phonological process' because it would require sonorant consonants to be phonologically specified as voiced. Yet the process can easily be stated in constraint-based terms in a way that does not refer to the voicing of sonorants at all:

(i)	MAX-μ	Don't delete moras (≈ Don't shorten a long vowel)
	*$V^{\mu\mu}C_{-voice}$#	Don't have a long vowel before a word-final voiceless consonant
	*V^{μ}C#	Don't have a short vowel before a word-final consonant
	DEP-μ	Don't insert moras (≈ Don't lengthen a short vowel)

Ranking: MAX-μ ≫ *$V^{\mu\mu}C_{-voice}$# ≫ *V^{μ}C# ≫ DEP-μ

I expect you will respond by saying that Western Slavic was a true-voicing language in which voiceless obstruents were in fact laryngeally unspecified and that, accordingly, the constraint *$V^{\mu\mu}C_{-voice}$# cannot be formulated in the grammar of such a language. Yet this claim would be different from the assertion that sonorant consonants are not redundantly specified for voice: the latter does not entail the former.

In fact, my own view is that laryngeal equipollence is empirically unavoidable, as argued on typological grounds by Wetzels & Mascaró (2001). Laryngeal equipollence is moreover grounded in the phonetics of voicing contrasts. It is well-known that, in a true-voicing language, the production of prevoiced obstruents will involve active articulatory strategies to overcome passive devoicing in vulnerable positions (e.g. finally in breath-groups): these active voicing strategies may include not just lowering vocal fold tension, but also lowering the larynx or advancing the tongue root (Jansen 2004: 39-40). In the same way, however, the production of voiceless obstruents often involves active articulatory strategies to overcome passive voicing in vulnerable positions (e.g. postvocalically and postnasally): these active devoicing strategies may include not just abducting the vocal folds, but also raising the larynx or tensing the vocal tract walls (Jansen 2004: 40). Of course, equipollent specification allows one to represent a ternary distinction between voiced, voiceless, and laryngeally unspecified (i.e. delaryngealized) obstruents: in this view, a delaryngealized obstruent is one that is neither actively voiced nor actively devoiced. This is precisely what is required for the right analysis of presonorant voicing: phonetic data, typological patterns, and the evidence of opacity effects all indicate that a voiceless obstruent always goes through an intermediate stage of delaryngealization, losing its active devoicing target, before becoming subject to presonorant voicing (Bermúdez-Otero 2011: §6; Strycharczuk 2012: §7.2; Strycharczuk, Van 't Veer, Bruil & Linke 2014: 441).

If that is indeed the situation, rejecting direct lexicalization, as I do, does not come at the price of accepting redundantly specified sonorants. Rather, it comes at the price of accepting laryngeal equipollence, which I believe is independently motivated. At this point, then, we have

two package deals: direct lexicalization and privativity, vs the normal life cycle of phonological processes and equipollence. Behind this choice lies, I suspect, a more fundamental dichotomy between two ways of demarcating phonology from phonetics: one focuses on crosslinguistically possible vs impossible contrasts; the other focuses on categoricity vs gradience. Our life would be easier if the two lined up, but they do not.

Adopting one or the other of these two package deals forces us to analyse particular phenomena, such as English /aɪ/-raising, in different ways. We can therefore choose between the two options by considering whether the analyses they lead to are empirically tenable. In this respect, I should like to point out a number of empirical problems for your approach to English /aɪ/-raising, which you develop in commendably explicit detail.

1. In your analysis, /aɪ/ undergoes raising only if the following voiceless obstruent is tautomorphemic. There is one piece of evidence that conflicts with this assertion. Idsardi (2006: 25) reports raising in morphologically complex neologisms like *i-th,* as in 'the i-th number'. This is presumably because the ordinal suffix *-th* is stem-level: it causes shortening in *five ~ fif-th*.

2. In your analysis, /aɪ/ undergoes raising only if stressed. This cannot mean 'only if bearing primary stress', as /aɪ/ undergoes raising in words like *mìcroscópic* (Chambers 1973: 125), where it bears secondary stress. I presume, therefore, that 'stressed' means 'accented' (i.e. bearing a pitch accent). That gives the right results for *micróscopist* (no raising) and *mìcroscópic* (raising), since /aɪ/ is unaccented in the former but accented in the latter (see Dabouis 2016: 38–40 for a helpful summary of the relevant literature). Unfortunately, /aɪ/ is raised, though unaccented, in words like *párasite, récondite, súlfite,* and *térmite*.

3. In your analysis, the diachronic precursor of /aɪ/-raising, namely offglide peripheralization, is phonetic (p. 136). Yet you hold prefortis clipping (or, in your view, stretching before sonorants and voiced obstruents) to be phonetic too. If both are phonetic, both should be blind to the underlying voicelessness of /t/-flaps, given that flapping is categorical (Herd, Jongman & Sereno 2010). Both predictions are false. First, Kwong & Stevens (1999), cited in Bermúdez-Otero (2014: §7), study the pronunciation of *writer* and *rider* in an American accent in which offglide peripheralization has begun but there is as yet no nucleus raising: they find that the offglide is peripheralized before the /t/-flap of *writer* but not before the /d/-flap of *rider*. Secondly, the fact that prefortis clipping registers the underlying difference between /t/-flaps and /d/-flaps is, of course, a classic observation, replicated in study after study (see Bermúdez-Otero 2014: §30 for a subset of references).

I conclude that there is, as yet, no empirically tenable analysis of English /aɪ/-raising built on the assumption that prefortis clipping is purely phonetic. I therefore further conclude that there is, as yet, no good evidence for direct lexicalization.

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