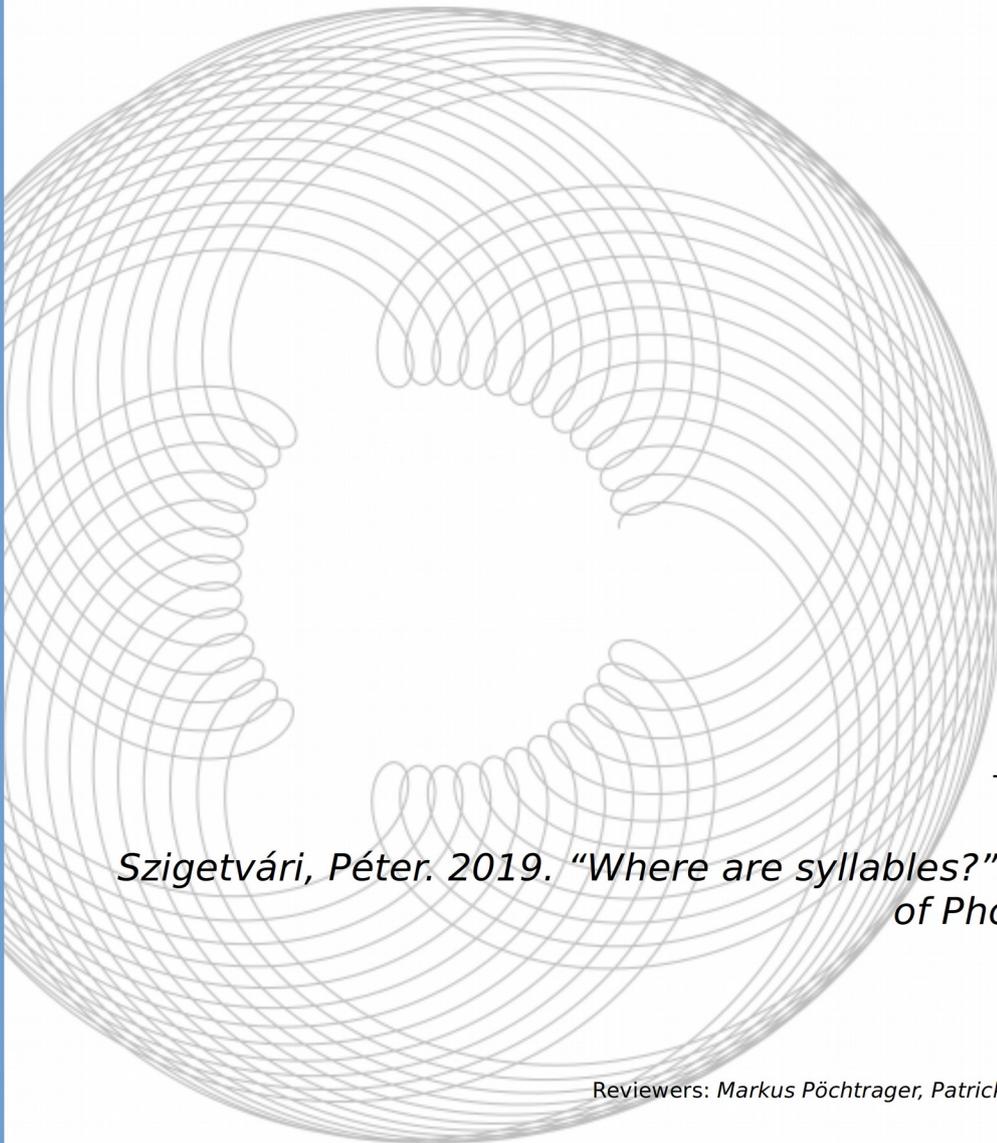


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WHERE ARE SYLLABLES?

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Wells argues that pronunciation dictionaries should indicate syllable boundaries to provide information about allophonic variation. I show that this is unnecessary, all the details needed to condition allophony are derivable from morpheme boundaries in English. I also show that, besides being unnecessary, the syllabification algorithm Wells offers is arbitrary, indeterminate, and inconsistent.

English consonant allophony, syllabification principles, pronunciation dictionaries

INTRODUCTION

I address an apparently practical lexicographical question: do syllable boundaries have to be indicated in the transcription of words in dictionaries, more specifically, pronunciation dictionaries? The question, however, is not only a practical one, it has an important theoretical repercussion. Unless we entertain some hard-core generative theory, the transcriptions given in a dictionary are (close to) the “lexical” form of the given entry. It follows that what we claim about the necessity of syllable boundaries in them will hold of the lexicalness of syllabification, too. In fact, if syllabification is not lexical, then it may even be considered if there is any need for it in the first place. If the position of syllable boundaries can always be determined on the fly, then anything that

depends on these boundaries can be determined by the same algorithms as those locating syllable boundaries without a need for the latter. In most cases (in all cases in many languages, though under standard analyses not in English), “being in onset position” is equivalent to being prevocalic, and “being in coda position” to not being prevocalic. That this may hold for English too will be argued for in §3.

So the question is whether syllabification is lexical (or, if you like, underlying), or it is derived? Or — to be radical — is there any need for syllabification at all? This question has been raised before by a number of phonologists (e.g. Hooper 1972, Kahn 1976, Lowenstamm 1981), or, from a practical point of view by compilers of pronunciation dictionaries: do we have to indicate syllable boundaries in dictionary transcriptions? Wells (1990a) tries to defend his decision to mark syllable boundaries in the *Longman Pronunciation Dictionary* (LPD, Wells 1990b), against Gimson’s decision of not doing so in the *Everyman’s Pronouncing Dictionary* (Gimson 1977). Their predecessor, Jones (1917), marks *some* syllable boundaries.

In this paper I argue that all cases of English that Wells shows to prove the necessity of lexical syllabification can be disambiguated by morpheme boundaries, just as Gimson (1980) argued. Therefore, by Occam’s razor, there is no reason for including syllabification in a pronouncing dictionary, as long as morpheme boundaries are indicated, at least where necessary. The reader suspecting that ultimately I argue against any kind of syllabification in any language is right, however this cannot be explicitly claimed on the basis of some examples of a single language.

In §1 I discuss principles that govern syllabification. One of these principles, sonority sequencing, is rather universally accepted,¹ albeit in its weaker interpretation, to be discussed presently. Onset maximization, on the other hand, is rejected by some analysts, or at least subjected to a third principle, which I will call the Uniform Boundary Principle. Wells assigns top priority to stressed syllables claiming that their

1 Although there are dissenting views too, e.g. Harris 2006.

coda is maximized. In §2 I use mostly his own examples to show that this is not only theoretically undesirable, but also empirically inconsistent and unnecessary. Finally, in §3 I cite a classic case where syllabification seems to provide an easy formulation of the environment of aspiration vs glottalization, but where alternative analyses are also available.

1 PRINCIPLES OF SYLLABIFICATION

Syllabification is governed by a number of principles. What can be — and is — debated is whether these principles — and which if any of them — are inviolable. I briefly review each principle below before looking at Wells's algorithm in more detail.

1.1 SONORITY SEQUENCING

The Sonority Sequencing Principle (Sievers 1876, Lowenstamm 1981, Selkirk 1984) requires that there be a single sonority peak in a syllable. This renders the falling-sonority cluster [rt] impossible as a syllable onset and the rising-sonority cluster [tr] impossible as a syllable coda, since (being more sonorous) [r] would create a second sonority peak in both cases. In a stronger version of this principle sonority must rise before the syllable peak and it must fall after it, ruling out adjacent segments of equal sonority within a syllable. However, to save [s]+plosive clusters, one must adopt a weaker version of the principle, furthermore, the sonority hierarchy has to be simplified, so that all obstruents rank equal in sonority (Clements 1990). According to this weaker version of the Sonority Sequencing Principle, sonority cannot fall before the syllable peak, and it cannot rise after the syllable peak. That is, sonority can remain level within the onset or the coda of a syllable, so that the now equally sonorous [s] and [t] may occur in an onset (and [ks] or [ps], as well as [kt] and [pt] may occur in a coda). But then again, with these modifications, we lose the explanation for the impossibility of a large set of other clusters, such as onset [ft] or coda [tf], in languages like English.

1.2 ONSET MAXIMIZATION

A second principle governing syllabification is the Onset Maximization Principle (Kahn 1976, Selkirk 1981, Harris 1994, Blevins 1995). In its Kahnian formulation it requires that onsets be constructed before codas. Therefore, if the Sonority Sequencing Principle allows a consonant to occur both in a coda and in the following onset, then it will go in the latter, making the onset as long as possible and, as a consequence, the coda as short as possible. Hence the preference for the syllabifications *cha.pel*, *sim.ple*, *pe.trol* over *chap.el*, *simp.le*, *pet.rol*, although all of these are allowed by the Sonority Sequencing Principle. However, the weak version of the Sonority Sequencing Principle and onset maximization together would also produce *go.spel*, *a.fter* or *cha.pter*, since [st], [ft] and [pt] are equal in sonority, at least in Clements's view,² and so are possible within a syllable onset (or coda).

1.3 UNIFORM BOUNDARIES

A third principle constraining syllabification is the assumption that possible syllable-initial and -final sound strings are identical to possible word-initial and -final sound strings, respectively. This principle seems so obvious to many syllabifiers that it does not even have an accepted name,³ so let us refer to it as the Uniform Boundary Principle. According to this principle, the difference between *gospel* and *after* is that [sp] is a possible word-initial cluster in English, therefore it is a possible syllable-initial cluster, too (*go.spel*), while [ft] is not possible word initially, and that is how we are supposed to know that it is not possible syllable initially either, at least in this language (*af.ter*, rather than *a.fter*). However, there is no particular reason for making the assumption that word and syllable boundaries coincide. In fact, as Lowenstamm (1981),

2 If we back out of accepting the idea that all obstruents are equal in sonority, then we will not be able to syllabify [st], [sk], and [sp] in the onset. Kaye (1992) provides several arguments to support that this is in fact desirable.

3 Claims like “in all languages, syllable edges correspond with word/utterance edges” (Blevins 1995:209) illustrate the point.

Kaye (1990), Harris (1994) among other authors demonstrate, this assumption leads to untenable conclusions, so it must be rejected. (Some further evidence against the equation of word and syllable boundaries is provided in §1.5).

1.4 “CODA MAXIMIZATION”

Wells’s syllabification algorithm contains a coda maximization principle, at least after stressed vowels. The idea is not unique to Wells, in fact, it stems from what I dubbed the Uniform Boundary Principle above, and may be detected in standard hyphenation conventions of English too: *chapel* is hyphenated *chap-el*, because the absence of word-final checked [a] is thought to entail the absence of syllable-final checked [a].⁴ Wells follows an extreme version of this principle in his syllabifications by stacking every possible, sometimes even impossible consonant in the coda of a stressed syllable, and even of an unstressed one if the following syllable is also unstressed. As we will see in §2.2, this is because he believes that fortis obstruents can only affect the preceding vowel(+sonorant sequence) if they are in the coda of that syllable.

Let us see how this algorithm works. In (1) I collect words containing a stressed vowel followed by a sonorant+obstruent cluster, with a liquid following where available, in a word that is probably monomorphemic, at least from a synchronic point of view. I indicate their syllabification as shown in LPD3 (Wells 2008). (N=nasal, T=plosive, F=fricative)

4 Cf *ca-per*, in which [ej] is available word finally.

(1)

	LABIAL		ALVEOLAR		PALATAL		VELAR	
NTV	temp.er	am.ber	Ant.on	And.es	anch.ovy	tang.ent	anch.or	an.ger
NTr	emp.ress	cum.brous	Antr.im	Andr.ew			Tanc.red	In.grid
NTI	Temp.lar	em.blem	ant.ler	hand.ler			ank.let	En.glish
ITV	scalp.el	alb.um	sult.an	Hild.a	cult.ure	Belg.ium	polk.a	alg.a
ITr	culp.rit	Hilb.ry	filtr.ate	caldr.on			fulc.rum	pil.grim
nFV	inf.ant	en.vy	canc.el	Kans.as	cens.ure			
mFV	amph.ora		Gims.on	crims.on	Gom.shall			
IFV	alph.a	Alv.a	bals.a	colz.a	Bolsh.evik	Sol.zhenitsyn		
IFr	Alf.red							

The data in (1) show that fortis obstruents are syllable-final with the sole exception of that in *Gomshall*. This is probably due to the lack of word-final [mʃ] in English, i.e. to the putative Uniform Boundary Principle. The position of lenis obstruents also appears to be governed by the availability of the given cluster word finally within a morph: [mb], [nv], and [ŋg] is separated, while [nd], [nz], or [lv] is not, and these clusters occur finally in e.g. *mend*, *lens*, *twelve*.

1.5 AGAINST THE UNIFORM BOUNDARY PRINCIPLE

A serious difficulty with the Uniform Boundary Principle is that to be able to properly apply it we first have to have a definitive list of possible word-initial and word-final clusters. The categorization of some clusters is clear: [nv] or [lʒ] do not occur word-finally (or word-initially), hence they must be split by the syllable boundary (*en.vy*, *Sol.zhenitsyn*); [nd] or [lk] do occur finally, hence they will be in the coda of a stressed syllable (*cind.er*, *polk.a*). There are other clusters, however, which occur rarely, making their status dubious. For example, [lj] is found finally only in *Walsh* and *Welsh*, making Wells quite uncertain: *Bolsh.evik*, *compuls.ion*, *convuls.ion*, *propuls.ion* vs *emul.sion*,

impul.sion,⁵ *Wil.sher*. Word-final [lg] is even rarer, occurring only in the little-known Scottish place name *Glenelg*, nevertheless LPD has *alg.a* and *vulg.ar* on the one hand, but *pil.grim* on the other. Such inconsistencies also crop up with clusters whose status ought to be obvious; for example, we have *Temp.lar* and *simpl.er* vs *am.pler* (but *amp.ly*). These examples show that the syllabification algorithm Wells entertains is somewhat loose and unreliable, which necessarily results in ad hoc syllabification.

Wells himself admits that the syllabification algorithm he proposes runs into difficulties. He mentions three cases: (i) syllables ending in a checked (short full) vowel, e.g. *no.stalgia*; (ii) syllables ending in [r], e.g. *starr.y*, or [ʒ], e.g. *vis.ion*; and (iii) syllables ending in [tr] (and, let us add, [dr]), e.g. *petr.ol* and *hundr.ed* (2008:xxvii). All these three anomalies violate the Uniform Boundary Principle. The first group has hundreds of examples, because it is not only checked vowels followed by [sp], [st], or [sk] that are involved, but any word containing a checked vowel followed by a stressed syllable with a single consonant onset is subject to this anomaly, e.g. *à.bérrant*, *à.déssive*, *À.léxis*, *À.ssám*, *Bè.nín*, *blò.ckáde*, *cà.shíer*, *è.mír*, *è.ssáy*, etc. Furthermore, words with a muta-cum-liquida onset in their stressed syllable also qualify, e.g. *à.bláte*, *hà.plólogy*, *lè.prótic*, *nè.crósis*, etc. Group (ii) contains even more “anomalously” syllabified items: there exist thousands of words in English containing a checked vowel followed by an [r], from *Ár.ab* to *zór.il*. In each of them either the checked vowel or the [r] must be syllable final, but neither configuration is available word finally. Finally as for group (iii), neither [tr], nor [dr] occur word finally in English. However, Wells analyses them as affricates, hence he must keep them tautosyllabic, even at the end of a syllable.

I conclude that the syllabification algorithm Wells proposes — and applies in LPD — is highly debatable, because it leads to indeterminacy and contradiction. But, as I will demonstrate in the next section, it is not only that there are serious problems with these

5 Even if some of these words were somehow different, it is hard to imagine how the syllabification of *compulsion* and *impulsion* could not be the same.

syllabifications, there is no need at all to have them in the first place.

2 CONSONANT ALLOPHONY

There are several types of allophonic phenomena that obstruents are involved in. The claim is that the allophony shown or induced by these obstruents is dependent on their syllabic affiliation, i.e. whether the obstruent is in onset or coda position. Interestingly, it is only fortis obstruents that are relevant, lenis obstruents pattern with sonorants, and it is rarely of any consequence where they are syllabified. In this section we will look at some cases that should allegedly be explained by the position consonants, mainly fortis obstruents occupy in the syllable.

2.1 ASPIRATION AND ITS ABSENCE

One feature of English that allegedly calls for lexical syllabification is aspiration observed in fortis plosives, namely, [p], [t], and [k]. Following the tradition, Wells claims that these are aspirated “when initial in a full-vowelled syllable. Elsewhere they have less aspiration or none” (1990a:77). This is why, he goes on, the second [p] is aspirated in *plum pie*, but not in *plump eye*. However, it is clear that these two [p]’s are not only different in that in the first phrase it is syllable initial, while in the second one it is syllable final, but in the first case it is also morpheme initial, and morpheme final in the second.

According to the hypothesis Wells entertains, a difference in the position of a morpheme-internal fortis fricative may be posited in examples like those in (2), where we indicate strong and weak/variable⁶ aspiration as ‘[h]’ vs the absence of aspiration as ‘[Ø]’.

6 G. Kiss (2017) displays measurements which show that the mean VOT before an unstressed vowel (e.g. in *writer* 79.75 ms, *hiker* 55.25 ms) may be as strong or even stronger than VOT before a stressed vowel (e.g. in *table* 75.00 ms, *cable* 55.75 ms). So the labels “strong” and “weak/variable” may as well simply be traditional, impressionistic categories.

for a brief discussion with audio illustrations, see Lindsey 2016). The aspiration of the [t] in *winter* or *filter* may not be as strong as in *intense* or *Voltaire*, where it is before a stressed vowel. Crucially, however, aspiration is simply absent in *after*, just like in *master*. This fact goes largely unnoticed because (i) the possibility of aspiration before an unstressed vowel is typically ignored and (ii) it is almost exclusively [s] that occurs before a plosive followed by a stressed vowel, words like *dràftée* or *káftàn* are few and far between. The problem is that it is not possible to produce a meaningful distinction between such words based on syllabification: neither Wells's *wint.er* and *aft.er*, nor the orthodox *win.ter* and *af.ter* explains the presence vs absence of aspiration of the [t] in them. Very few would go as far as to posit *win.ter* vs *a.fter* to get this difference, but Kahn (1976:49) is a notable exception.⁷ Yet, unless one accepts Kahn's syllabifications for these cases, one cannot account for the absence of aspiration in *after* by reference to the syllabic position of the plosive.

I cannot go into details of an alternative account for aspiration in this paper (but see Szigetvári, in press and references there). The crux of the matter is that phonetic voicing vs voicelessness is not the main clue in English to tell fortis and lenis obstruents apart. The plosives in the words *dog* or *god* pronounced in isolation may be partially voiced, but even when voiceless they qualify as lenis, because of their lack of aspiration (vis-à-vis the initial fortis plosives in *t[h]og* and *c[h]od*), and because of the relative length of the vowel of these words (vis-à-vis that of *döck* and *göt*). But then it follows that the plosives following the fricatives in *dispel*, *gospel*, *master*, and *after* must also be lenis: although they are voiceless, they are unaspirated. Thus I claim that the lenis plosives in these words, as well as all others of their ilk, are simply miscategorized in spelling, and as a consequence in transcription too. In fact, they are *dis[b]el*, *gos[b]el*, *mas[d]er* and *af[d]er*. In English any aspirated plosive following a fricative is either separated from it by a morpheme (not syllable) boundary, as in *mistime* [s#t], or is preceded by a lenis

⁷ As a reviewer points out, Kahn analyses the [f] in *after* as ambisyllabic. In any case he posits the same syllabification to *after* and *aspen*.

(not fortis) fricative, as in *Aztec* or *lieutenant* [lɛv'tɛnənt]. Or occasionally both, as in *Jamestown* [z#t].

The conclusion to draw is that the syllabic position a fortis plosive occupies has got nothing to do with whether that plosive is aspirated or unaspirated, this is a lexical property of the plosive: it is fortis if aspirated and lenis if not. That is, it is unnecessary to indicate syllable boundaries for predicting the presence vs the absence of aspiration in English.

2.2 PRE-FORTIS CLIPPING

It is not only the aspiration of fortis plosives that Wells believes to be governed by their syllabic affiliation, but also the length of the vowel or vowel+sonorant consonant sequence that precedes a fortis obstruent. These are indeed shorter before a fortis than before a lenis obstruent (or a sonorant) in otherwise comparable words.

However, contrary to what Wells claims, the fortis obstruent does not have to be in the same syllable as the vowel(+sonorant sequence) that it shortens. It has to be in the same morph. This is the case in every single one of the examples brought up by Wells (1990a:78): *self*, *selfish*, *dolphin*, *feet*, *feature*, *lap*, *lamp*, *happy*, *hamper*; while in each of those where there is no shortening, the obstruent is in initial in the following morph: *shell#fish*, *fun#fair*, *fee#paying*, *tea#kettle*. It is truly remarkable how Wells fails to notice this obvious difference. Even on purely structuralist grounds “juncture phonemes”, representing morphological/semantic structure, are always there to distinguish such cases.

Pre-fortis clipping affects stressed vowels only. So the first, stressed vowel or vowel+sonorant consonant string of the words in (3a–e), where it is followed by a fortis obstruent, is shorter than their counterparts in (3f–j), where a lenis obstruent follows.

- | | | |
|-----|-----------|-----------|
| (3) | a. back | f. bag |
| | b. jackal | g. Jagger |
| | c. martyr | h. pardon |
| | d. viper | i. tribal |
| | e. mantle | j. sandal |

It has already been noted above that Wells syllabifies words using a coda maximizing algorithm. This means that typically all consonants following a stressed vowel will be stacked into one syllable: *jack.al*, *mart.yr*, *vip.er*, *mant.le*, etc. As a result any fortis obstruent following a stressed vowel will be in the same syllable as the vowel before it, except if the obstruent is part of another morph. So the claim that pre-fortis clipping only occurs before a tautosyllabic fortis obstruent becomes trivial, it simply follows from the arbitrary syllabification algorithm Wells posits.

The length contrast before fortis and lenis obstruents in word-final stressed syllables, i.e. in (3a) vs (3f), is well-documented (Cruttenden 2014, Kaye 2014). There is less evidence that the same contrast also holds in the case of nonfinal syllables, i.e. in (3b) vs (3g), etc (cf Pöchtrager 2006). Let us nevertheless accept Wells's stance that there is such a contrast, even if it is not as pronounced as it is word finally. Consider the word pairs in (4).

- | | | |
|-----|------------------------|-------------------------|
| (4) | a. absolute | e. óbservación |
| | b. Ágfa | f. ádvert |
| | c. Húdson | g. Púdsey |
| | d. médecine %['medsən] | h. médecine %['medəsən] |

The stressed vowel of the words in the first column is followed by a lenis and then a fortis obstruent, while that in the second is not followed by a fortis obstruent at all (before the next vowel, that is). If the stressed vowel of the words in the first column is

indeed shorter than that of the words in the second (the first stressed vowel in *óbservátion*), then the LPD system should indicate this by stacking the fortis consonant, which is apparently responsible for the contrast, to the end of the stressed syllable: [ˈæbs.ə.lu:t], [ˈægʃ.ə], [ˈhʌds.ən], [ˈmeds.ən]. This would also subvert the Uniform Boundary Principle: lenis+fortis (or fortis+lenis) clusters are not supposed to occur at word edges — at least in the wide-spread analysis of obstruent clusters. It seems that all that is necessary for pre-fortis clipping to apply is that a stressed vowel be followed by a fortis consonant before the next vowel, within the same morph. That is, syllabic affiliation is again an irrelevant factor in determining if this process applies in a word or not.

2.3 TAPPING AND GLOTTALIZATION

The environment where tapping and glottalization occur are similar, but not identical. Wells claims both processes occur syllable finally, unless an obstruent precedes the [t]. The example given for tapping is again blatantly inappropriate: *might I* has tapping, *my tie* does not (1990a:78). Clearly, there is no need to talk about syllable boundaries here either, the two [t]’s are in different morphs: while tapping is possible at the end of a *morph* (provided the next morph begins with a vowel), it is not at the beginning of one.

In §2.1 we have seen that Wells ignores the fact that the [p] of *chapel* may be aspirated to some extent, while that of *gospel* (which therefore I claimed was actually a [b]) may not. This ignorance may be defended: aspiration is a gradual property, VOT values may vary on a wide scale, and one can only arbitrarily determine what to consider “aspirated” and what “unaspirated”. Tapping is different, it is categorical, and fails to apply to any [t] that is before a vowel stressed to any extent within a morpheme. This causes a problem for an analysis that wishes to tell if a [t] is tappable by its syllabic position and maximizes codas after a stressed vowel at the same time. There are scores of monomorphemic words in which a stressed vowel is followed by an other, “less” stressed one with an intervening [t]: e.g. *ádamántine*, *amórtize*, *átòll*, *áutàrchy*, *Bótòx*,

bútàne, cántòn, céntàur, etc. In these words the vowels before and the one after the [t] are both stressed, but the former one is a possible tonic (marked by an acute accent), the latter one is not (marked by a grave accent), accordingly it counts as a lesser degree of stress. Following Wells's syllabification algorithm, these [t]'s are all syllable final, hence they are predicted to be tappable. Yet they are not.

In fact, to my knowledge two words like *átom* and *átòll* are not syllabified differently by any syllabification algorithm. Therefore the fact that the [t] in the first word is tappable, but not in the second cannot be accounted for by syllabification.

2.3 CLEAR AND DARK [l]

Some varieties of English, Standard British English among them, exhibit two allophones of the lateral liquid, the so-called clear (i.e. palatalized) [l] and the dark (ie velarized) [ɫ]. Their distribution is rather easy to capture: clear [l] occurs before vowels (e.g. *Lear, clear, Alice*), dark [ɫ] otherwise (e.g. *Alps, alpha, all*). At a first glance, one may think that syllable-initial [l]'s are clear, syllable-final ones are dark. However, this is not the right distribution if we follow Wells's syllabification algorithm, since stress does not influence this allophony: the [l] is clear in both *allów* and *fóllow*. Thus we do not get the distribution if we assign consonants, in this case [l], to the coda of a stressed syllable: *fóll.ow*. In fact, clear [l]'s are always in the syllable onset only if we give top priority to the Onset Maximization Principle, making any prevocalic [l] (part of) an onset, rather than a coda. Such an analysis even explains the fact that there is one consonant, [j], before which [l] is clear: this is because [lj] is a possible syllable-initial cluster in English, and it is only [j] with which [l] may form an onset.

The fact that [l] is clear even morpheme finally provided that the next morph begins with a vowel or [j] (e.g. *kill it, kill you*) must make us less enthusiastic about the role of syllabification in this allophony. To explain the clear [l] in its word-final instances, we have to assume that word-final consonants are resyllabified across the word boundary. But while resyllabification is commonly assumed in VC#V sequences (>V\$CV), it is

not in VC#CV (>V\$CCV), i.e. it is quite unlikely in the case of *kill you*.⁸ The alternative analysis is that the selection of the two allophones again is independent of syllable boundaries, all that matters is the identity of the following segment. Unlike in the case of aspiration, here even morpheme boundaries are ignored. It is hardly an accident that [l] allophony is not even mentioned in Wells's (1990a) catalogue of allophony supporting the inclusion of syllabification in LPD.

3 SO ARE SYLLABLES OF ANY USE?

After all these cases that are not arguments for syllabification, the question rightly arises: is there any argument for it at all? In fact, there is one case where traditional syllabification seems to offer an easy formulation of a contrast: the first, pretonic [t] of *attractive* is aspirated, that of *Atlántic* is not, it possibly undergoes glottalization. The difference clearly mirrors the fact that [tr] is available word initially, while [tl] is not, accordingly — a proponent of syllabification may argue — the first cluster is made an onset by the Onset Maximization Principle, the second one is not. This is a classic example brought up in support of syllabification by, for example, Kenstowicz (1994:251).

An alternative that lends itself to dismiss this argument for the necessity of assuming syllabification is adopting Lowenstamm's (2003) proposal according to which [tr] (and [tw], as well as [tj] for varieties of English that have it) is a single segment, just like, for that matter, [tʃ]. Consequently, aspiration is a phenomenon that only occurs prevocally and it is the obstruents [t], [tʰ], [tʷ], [k], [kʰ], [kʷ], etc that undergo it. In this view, the first [t] of *Atlántic* remains unaspirated, just like the [t] of *Atkins* or *Vietnam*, because these fortis plosives are not before a vowel. Of course, an account must still be produced for why [tʰ] can be a segment, while [tʰ̥] cannot (in English?), although [kʰ] is also a segment. Theories of syllabification are facing exactly the same

8 The reason is quite obvious: while a singleton consonant in an onset is less marked than an empty onset (therefore V\$CV is preferable to VC\$ØV), a consonant cluster in an onset is more marked than a singleton onset.

task.

CONCLUSION

At first sight, the aim of this paper may seem to be dissing Wells's syllabification algorithm. I believe that the stakes are higher. There appears to be hardly any allophonic alternation in English that is dependent on syllabification, instead they are determined by whether the following segment is a vowel, a consonant, or a morpheme boundary. As opposed to syllable boundaries, morpheme boundaries can be discovered by comparing different members of paradigms, which in turn can be discovered on semantic grounds. None of these discovery methods are available for syllabification.

I have argued that the syllabification algorithm offered and applied in LPD by Wells is arbitrary, inconsistent, leads to wrong predictions, and — most importantly — is unnecessary. In this paper I have not had the room to touch upon other issues that might call for syllabification (like, for example, stress assignment).⁹ In any case, allophony does not seem to be provide support for the usefulness of syllabifying sound strings either in pronunciation dictionaries or probably in phonological theory in general.

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9 In Szigetvári 2017, I argue that word stress in English is lexical, i.e. there are no rules that assign stress, as a consequence syllabification is not needed for this purpose either.

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DISCUSSION WITH MARKUS PÖCHTRAGER

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Pöchtrager, Markus. 2019. discussion in: Szigetvári, Péter (auth.) “Where are syllables?”. *Radical: A Journal of Phonology*, 1, 111-119.

COMMENTS

In his article, Péter Szigetvári demonstrates quite convincingly that Wells’s syllabification algorithm is either inconsistent or incorrect in that (to the extent that that can be determined at all) it makes wrong predictions about various phonological phenomena of English. Those problems are so pronounced that I can only agree with Szigetvári’s discussion.

There is, however, a larger issue at stake, which the author only hints at, both in the introduction and in the final discussion: Whether syllabification is necessary at all. It is not entirely clear to me what interpretation of “syllabification” is intended. The construction of a syllable as used in mainstream phonology or, more generally, the construction of *any* kind of constituent?

If Szigetvári refers to former, then of course he can build on a substantial body of literature, especially (but not only) that of Government Phonology (GP) and its various kinds of offspring (mentioned by the author himself several times). In that literature, arguments against the “classic” syllable have been made several times. Usually, some concept of constituency remains: Either explicitly in terms of constituents such as onset, rhyme or nucleus (and nothing beyond those, in particular no syllable), or, more implicitly, in the emulated form of (infrasegmental) government relations and the like which mimic constituency.

If the second interpretation is intended, one where any kind of constituency is given

up on, then that is a much taller order. Szigetvári does not make any explicit claims in that respect and he is careful enough to point out that any such task would require more elaborate discussion. If such complete abolishment of constituency is the ultimate target, then there are of course a couple of points that need to be addressed, also with respect to the data discussed by Szigetvári. I will go through them in what follows. Let me stress that I do not intend this as a criticism of what Szigetvári says, but rather state these points in the hope of creating an interesting dialogue.

Constituents as a unifying force. In the introduction, Szigetvári says: “If the position of syllable boundaries can always be determined on the fly, then anything that depends on these boundaries can be determined by the same algorithms as those locating syllable boundaries without a need for the latter.”

This implies giving up on the unifying character of constituent structure: If constituent structure is taken as real, even if established by a certain algorithm, then it is not surprising if several processes or regularities should refer to that same particular constituent structure. In the alternative view, which is what the author seems to have in mind, “anything that depends on these boundaries can be determined by the same algorithms”. That is of course correct, but the crucial word is “can”: There is no deeper reason why one and the same syllabification should be relevant for more than one process/regularity. Branching onsets are not only relevant for explaining why before certain clusters long vowels occur quite freely (*apron, patron, sacred, secret*), but they also form a relevant set of clusters that is systematically barred in final position in English. Pretty much the same set comes back with similar characteristics in other languages, e.g. tonic lengthening in Italian. If constituency had the random character suggested by something “determined on the fly”, it seems to me that such recurring properties would be accidental.

Pre-fortis clipping. In section 2.2 we read that “contrary to what Wells claims, the

fortis obstruent does not have to be in the same syllable as the vowel(+sonorant sequence) that it shortens. It has to be in the same morph.” In the introduction, Szigetvári had made a similar comment on the superiority of morpheme boundaries, in so far as “all cases of English that Wells shows to prove the necessity of lexical syllabification can be disambiguated by morpheme boundaries”.

In both cases, the wording is somewhat surprising. If taken literally, it leads to an incorrect prediction: *rabbit* and *rabid* differ in their word-final (and morph-final) consonants, which (of course) has no effect on the duration of the stressed vowel *a*. What Szigetvári must mean, as becomes clear from his item (3), is that pre-fortis clipping should apply in *rap* as much as in *rapid*, no matter that by mainstream assumptions the *p* is part of the first syllable in *rap*, but part of the second in *rapid*. (Analogously in *cab* and *rabid*, but there is no fortis consonant to clip anything.) So what is intended is that there has to be adjacency for pre-fortis clipping, but not necessarily a shared constituent that subsumes both the “clipper” and the “clippee”.

It is not so clear that this statement is really correct, though. The durational difference is substantial in stressed final syllables, such as *rib/rip*, *dug/duck*, *cab/rap* etc. The vowels before a lenis consonant are about 90% longer than vowels before a fortis consonant. (There are additional complications with the low vowel in pairs like *cab/rap*, since that vowel before a lenis consonant has undergone changes in quality as well, cf. Kaye 2012 for a detailed discussion.) But in non-final syllables the difference is much more subtle and lies at about 10% maximum in pairs like *rabid/rapid*. Translated in absolute numbers, it is not always clear that that corresponds to a difference that is reliably perceptible. In Pöchtrager (2006) I assumed that it was not, and hence played no role phonologically. But even if that decision should have been the wrong one to take, it is still a fact that the *amount* of the effect of fortis consonants does not simply depend on adjacency. (It is not completely clear to me how Szigetvári sees this, his wording on pp. 12–13, “If the stressed vowel [...] is indeed shorter” suggests some indecision.)

Tapping and glottalisation. Szigetvári is again right in pointing out the shortcomings of Wells's account of tapping and glottalisation. But the generalisation he himself gives is not without problems, either: "While tapping is possible at the end of a morph (provided the next morph begins with a vowel), it is not at the beginning of one." This is both too general and too narrow.

It is clear that this characterisation falls in line with the suggestion made at the beginning of the text, viz. that morpheme boundaries help to disambiguate where syllable boundaries fall short. But tapping/glottalisation rely crucially on metrical properties, which Szigetvári also elaborates on. It is therefore all the more surprising how much the above statement stresses the importance of morphs. "[T]he fact that the [t] in [atom] is tappable, but not in [atoll] cannot be accounted for by syllabification" is correct, but neither can it be accounted for by morpheme boundaries. Furthermore, contrary to what Szigetvári claims, tapping is possible morpheme-initially, but only with certain morphemes like *to*, *today*, *tomorrow* etc. Goldsmith (2011: 92) gives *Go to sleep!* as an example of morpheme-initial *t* being tappable.

Branching onsets and contour segments. Towards the end, Szigetvári hints at treating branching onsets (BOs) like [tr] as in *train* and affricates like [tʃ] as in *chain* as the same kind of object. Finding 100% waterproof arguments for separating those two groups and claiming bisegmental status for BOs and monosegmental status for affricates (i.e. the mainstream view) is indeed tricky. That there are differences between BOs and affricates seems clear, but how to interpret them, less so. Unlike coda-onset clusters or branching nuclei, where bisegmental status and the metrical visibility resulting from it are generally not called into question, prosodic phenomena like stress do not usually come to our aid in the analysis of BOs. (Though see Topintzi 2010.)

Furthermore, arguments in favour of a bisegmental status of BOs can often be countered by a reinterpretation in terms of melodic content. For example, following

Charette (1990, 1991), French allows BOs finally, as in *table*. (More precisely, it allows BOs to be licensed by a final empty nucleus that remains uninterpreted.) English does not, hence when that word was borrowed the final BO could only be lexicalised with a final “syllabic” *l*, treating *b* and *l* not as a unit, but as two separate entities (as two separate onsets in GP). While English does not allow final BOs, it certainly allows final affricates: *rich*, *branch*, *wedge*. In an account that takes BOs as bisegmental (grouped into a branching constituent) and affricates as monosegmental, the distributional difference we find could be blamed on structural complexity: A constituent licensed by a final empty nucleus cannot branch in English. (Essentially Charette’s analysis.) But there is of course no guarantee that this is the real reason for the asymmetry. It could have nothing to do with structure and everything with melody: Bona fide affricates will contain certain melodic features and BOs will contain others, and it is those melodic properties that might be problematic in final position. Spanish allows final [n] but not final [m], and this is just one precedent of many where melodic properties decide about distribution in final position. But without a concrete and detailed proposal it is hard to assess which of the two, a restriction on structural grounds or one on melodic grounds, holds more promise. Szigetvári is aware of that and he is also right that both accounts would have to explain phonotactic restrictions (between segments or within the segment), such that the lack of (English?) [tl] would have to be explained by either camp.

So let me try and up the ante then: If we give up the idea that [tr] etc. consist of two segments, it also becomes meaningless to set a two-segment limit on BOs (as is done in GP). Under such a view, both *pay* and *pray* will have a single initial segment. *Spray* would presumably have two: *s* and *pr*, but there is evidence that *s* is not part of the onset anyway, as also pointed out by Szigetvári, so all is good. But then what about German *Zweig* [tsv-], *Pflaume* [pfl-], or *Pfropf* [pfr-], cognate with English *twig*, *plum*, and *prop*. (And identical meanings except for *Pfropf* “plug, stopper”.) Treating affricates as monosegmental and BOs as consisting of two positions still allows us to express those

cases. But if we give up on the difference and thus on a limit, why do those cases express the maximum that German allows in an onset? How are to be represented even? If [pf] is a contour segment and [pl] is, too, then what is [pfl]? A contour segment inside a contour segment? What guarantees the respective ordering? Getting rid of constituency is like stuffing balloons in a bag, and it is surprising to see where they come out again.

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REPLY

First of all, I would like to thank all my commentators for their contribution, they have significantly improved the paper in the first round. It is clear that they are not fully satisfied with the outcome, hence this second, public exchange. In his public comments Markus Pöchtrager asks for clarification on my stance on whether syllabic constituency exists, and raises some further issues. I follow his sectioning.

Constituents as a unifying force. Pöchtrager is right in pointing out that “syllables” can be rejected in two different senses: (i) one can argue that the node labelled “syllable” is missing from representations of prosodic structure or (ii) one can argue that syllabic constituents of any kind, onsets, nuclei, codas, etc are absent from these representations.

The first interpretation is not new, it has been extensively argued for by government

phonologists (Kaye & al 1990). My agenda — not necessarily in this paper — is to go for the second interpretation, which is the more obvious one, this is why I do not make this distinction between the two interpretations in the paper. I find it important that to support “constituents as a unifying force” Pöchtrager only mentions “branching onsets”. Indeed, this is the obvious reason why syllabic constituents are assumed to exist. Notions like “coda capture”, the ambiguous syllabification of intervocalic consonants (*de.mon* vs *lem.on*) is rejected by Pöchtrager and everybody educated in the government phonology tradition. That is, if the necessity of “branching onsets” could be disproved, syllabic constituency would lose much, perhaps all of its attractiveness. I will return to this below.

Pre-fortis clipping. I must admit I failed to notice the ambiguity of the wording “the fortis obstruent does not have to be in the same syllable as the vowel(+sonorant sequence) that it shortens. It has to be in the same morph.” In light of Wells’ examples (*selfish* vs *shellfish*, *dolphin* vs *funfair*, *happy* vs *fee paying*) the meaning should be obvious. But Pöchtrager has a more serious objection: the degree of shortening is significantly larger if the fortis obstruent is word final than if it is followed by a further vowel within the morph. So the length difference of the vowels of *cab* vs *rap* is much larger than that of the first, stressed vowels of *rabid* vs *rapid*. Crucially, Pöchtrager does not deny that there exists some difference. Wells argues that this has to do with syllabification. I argue that it does not.

The larger difference in length between the vowels of *cab* vs *rap*, as compared to *rabid* vs *rapid*, is not that surprising given that this length is the main cue for distinguishing the lenis [b] and the fortis [p] in the first pair, while in the second pair voicing is also available.

Tapping and glottalization. It was not my intention to provide a complete theory of tapping and glottalization, especially since I am not capable of doing that. It is clear that

both phenomena are dependent on stress: they are much rarer (but not outright impossible) before a stressed vowel than before an unstressed one. There also is large scale variability in the environments where these phenomena occur. The only point I wish to make, the relevant point for the argument of this paper, is that syllabification is not capable of distinguishing the environments where tapping and glottalization do and do not occur.

Branching onsets and contour segments. Returning to branching onsets, Pöchtrager lists a set of common arguments against taking these objects as monosegmental. In the context of this debate I find the fact that English does not have word-final [bl], but it does have word-final [tʃ] no more significant than that English does not have word-final [h], but it does have word-final [k], or that Spanish does not have word-final [m], but it does have word-final [n], as Pöchtrager notes. Even if both [bl] and [tʃ] were contour segments, some consonants are found in the unfavourable word-final position, others are not. It is true that French [tabl] is adapted to English as [tejbəl], i.e. an allegedly single segment of French, [bl], is interpreted as two segments in English ([b]+[l], repaired by the epenthesis of [ə] word finally). However, loan word adaptation is known to reinterpret a single segment of one language to a sequence of segments in another language. For example, French [y] was adopted as [iu:], later [ju:] in English (e.g. *pure* F [pyr] > E [pju:r]). Accordingly, I see no reason not to assume that the monosegmental [bl] of French is reinterpreted as bisegmental [bl] word finally in English, since the monosegmental [bl] is unavailable here. In a similar vein, the monosegmental German [ts] is reinterpreted as bisegmental [ts] in English words like *quartz* or *waltz*. A more revealing comparison would be to check whether a syncope-created [tr] cluster (e.g. in *cemet(e)ry*), which was certainly bisegmental, at least before syncope struck, is identical to a “lexical” [tr] (e.g. in *symmetry*). I do not know.

I do not take it for granted that affricates are “contour segments” (provided we have a working hypothesis of what a contour segment is in the first place). There is no

consensus on this issue in the literature, in fact, Lin (2011:387f) claims that the balance tilts towards the “stop approach”, namely, that affricates are stops, not complex (ie contour) segments. Therefore it is a plausible assumption that there is no significant difference between a monosegmental [pr] and a monosegmental [pfr]. It remains to be explained why German lacks [tsr], but I do not see the arguments of which party this asymmetry strengthens.

Kaye, Jonathan, Jean **Lowenstamm**, and Jean-Roger **Vergnaud**. 1990. Constituent structure and government in phonology. *Phonology* 7:193–232. **Lin**, Yen-Hwei. 2011. Affricates. In Marc van Oostendorp, Colin Ewen, Elizabeth Hume, and Keren Rice (eds.) *Blackwell Companion to Phonology*. Wiley-Blackwell. 367–390.

DISCUSSION WITH SŁAWOMIR ZDZIEBKO

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Zdziebko, Sławomir. 2019. discussion in: Szigetvári, Péter (auth.) “Where are syllables?”. *Radical: A Journal of Phonology*, 1, 120-125.

COMMENTS

Wells’s *Longman Pronunciation Dictionary (LPD)* has been argued to be a remarkable piece of lexicographic work by virtually all its reviewers (see Snook 1990, Hung 1992, Bald 1993, Sobkowiak 1994, Lewis 2009). At the same time, those reviews that mention the syllabification conventions assumed in LPD admit that they might be considered ‘somewhat controversial’ (Hung 1992:97) or even ‘legibility-impairing or counter-intuitive’ (Lewis 2009: 239).

In order to briefly justify the above opinions, let me point to the fact that LPD recommends the syllabification of the words *flimsy*, *windy* and *petrol* as /flɪmz.i/, /wɪnd.i/ and /petr.əl/. These and other such divisions are the consequence of the application in LPD of the principle which Szigetvári in his paper refers to as coda maximization: the coda of the stressed syllable should be as big as possible, typically at the cost of the following syllable onset and not infrequently in violation of well-motivated principles of the English phonotactics.

The choice to sacrifice phonotactic restrictions and to ignore speakers’ intuitions is made by Wells completely consciously. In the introduction to the dictionary we read that the aim of the syllabification proposed in LPD is to help the users predict the distribution of allophones of consonants (Wells 2008: xxvii). Thus, according to Wells, syllabifying *petrol* as /pet.rəl/, i.e. in accordance with the intuitions of speakers and respecting the phonotactics of English, would lead the user to expect that the /t/ is likely to be glottalized while /r/ is voiced (as in *ratrace*). Since the pronunciation /peʔrəl/ is

not the pronunciation attested in the dialects represented in the dictionary (or at least it seems not to be recommended) the author opts for the syllabification /petr.əl/: the one with impossible coda-cluster which is most likely unpronounceable for an untrained Englishperson but at the same time points to the /t/ being pronounced as a voiceless affricate.

In his paper Szigetvári argues that the principles underlying the syllabification conventions assumed by Wells are highly questionable on theoretical grounds and shows that even Wells's dictionary fails to apply the said conventions consistently. Last but not least, the syllabification proposed by Wells is argued to be unnecessary for predicting the allophonic variation in English. Szigetvári defends a more traditional view whereby all allophonic variation is predictable on the basis of morpheme boundaries and adjacency relations between segments (*kill you* is said to be pronounced with the 'clear' /l/ due to the adjacency of /j/).

Szigetvári's claim is, however, more general and it has been made clear in the introduction to the paper. It may be summarised by the following quotation '...by Occam's razor, there is no reason for including syllabification in a pronouncing dictionary, as long as morpheme boundaries are indicated, at least where necessary.' (Szigetvári 2019: 93). In sum, therefore, the main claim of the paper appeals clearly though indirectly to the notion of the economy of representation: syllable boundaries should not be represented in pronunciation dictionaries because they serve no tangible purpose. All the information that could be inferred on the basis of syllable boundaries may be inferred on the basis of an independently needed morpheme boundaries and adjacency. Similar appeals to parsimony can be found throughout the paper. On page 102 Szigetvári writes:

'The conclusion to draw is that the syllabic position a fortis plosive occupies has got nothing to do with whether that plosive is aspirated or

unaspirated, this is a lexical property of the plosive: it is fortis if aspirated and lenis if not. That is, it is unnecessary to indicate syllable boundaries for predicting the presence vs the absence of aspiration in English.’

The theoretical *modus operandi* taken up by Szigetvári in his paper might be said to be the typical methodology that any theoretical linguist and indeed most practicing scientists would take up. He isolates a certain variable factor (the syllable divisions in English as postulated by Wells (2008)) and examines the possible relationship between this factor and the observed phenomenon (consonant allophony). The conclusion that Szigetvári reaches is that the correlation between the variable and the phenomenon under consideration is negligible, i.e. it is not possible to predict whether a given consonant will show a particular realization on the basis of the syllable boundaries as indicated in LPD. Instead the correlation between the allophonic variation and morpheme junctures is argued to be the significant one. Furthermore, the insignificant correlation should be ignored as making it part of the representation is uneconomical (by Occam’s razor).

Szigetvári’s criticism of Wells’s syllabification algorithm is correct in its own respect as is his conclusion according to which the syllable boundary seems to be a negligible factor for consonant allophony. At the same time reading the paper I had a distinct impression that the methodological toolkit applied by Szigetvári and outlined in the previous paragraph is not entirely suitable for the evaluation of the notation employed in a dictionary of pronunciation.

To be precise, I think that the methodology based on isolating correlates and ignoring weak correlations in favour of strong correlations in line with economy considerations is not the appropriate strategy of evaluating teaching materials. If LPD was a monograph promoting a particular approach to the phonology of English or if Wells’s syllabification conventions were clearly a claim concerning the theory of syllabification or the shape of syllables in English, then it would be fully justified to carefully consider

the principles that underlie the said conventions in order to establish whether they form a coherent system and whether the correlations that the conventions are meant to account for are reasonably strong.

Wells's dictionary, however, is not theoretically focused and the syllabification conventions assumed in it are simply meant to aid the users in deciding which consonant allomorph is more likely to occur in a given position within the word. For this reason, I do not think the economy of representation or Occam's razor are necessarily the principles that should be employed in the evaluation of the syllabification conventions assumed in LPD.

It may well be the case that most if not all the phenomena related to allophonic variation in English are derivable from morpheme boundaries and adjacency. Nevertheless, if marking syllable boundaries does not contradict the information inferable from junctures and syntagmatic relations between segments, then it may well be included in a pronunciation dictionary provided that the authors of such a dictionary see this as a useful piece of information from the point of view of the users of the dictionary.

The logic of teaching materials is not the logic of redundancy avoidance and Occam's razor. It is rather the logic of teaching and the logic of teaching says that it is desirable to provide the learner with as many helpful cues as it is necessary to foster their self-development. It may often be the case that the said cues involve redundancy in that they partially or fully repeat the same information. If such a situation is helpful to the learner, e.g. by underlying the crucial information, then so be it.

Obviously, whether Wells's syllabification conventions really help the learner master English pronunciation more efficiently is a very different question.

First of all, the choice made by Wells to ignore speaker's intuitions and phonotactics in favour of providing the learner with cues to allophonic variation seems quite arbitrary. Why should the information that /t/ in *nostalgia* (syllabified in LPD as /nɒ.stældʒ.ə/) is not aspirated and the /t/ in *petrol* is unlikely to be glottalized be more

important than the information that English syllables and morphemes do not terminate in /b/ and /tr/ and that speakers intuitively syllabify two-consonant clusters into adjacent syllables? I see no compelling reason to treat the former details of English pronunciation as more important than the latter.

At the same time, one could argue that since transcription conventions do not allow us to represent all the relevant details of English pronunciation, any dictionary has to make an arbitrary choice as to which details are represented and which are omitted. LPD omits the details pertaining to phonotactics and speakers' intuitions, while, say, Cambridge English Pronouncing Dictionary (Jones et al. 2011) that syllabifies *nostalgia* as /nɒs.tæl.dʒə/) gives up on allophonic variation and aims at conforming with phonotactic restrictions.

The real question is, however, whether and to what extent the syllabification conventions assumed in LPD actually aid the learners in predicting allophonic variation as they are meant to do. In particular, to what extent does syllabification of *nostalgia* into /nɒ.stældʒ.ə/ and *petrol* into /petr.əl/ allow the speaker to predict the exact realization of /t/ in those words better than the syllabification postulated for example in the Cambridge English Pronouncing Dictionary?

Clearly the exact answers to the above questions call for a detailed research among the users of the said dictionaries and cannot be given in this short contribution. At the same time my intuitions as a learner of English, pronunciation trainer and phonologist tell me that in order to know that the /t/ in *petrol* is likely to be realized as an affricate and the /t/ in *nostalgia* will not be aspirated a user of LPD, as well as any other available dictionary, must have undergone at least a preliminary training in English phonology. To be more precise, such a learner must know that /t/ in Southern Standard British English is likely to be realized as an affricate before the rhotic and that /t/ may be aspirated only at the beginning of the syllable or that it is never aspirated after /s/.

The point, therefore, is not whether LDP and other pronunciation dictionaries ought to aid the users by providing some possibly redundant cues to allophonic variation. The

point is that such cues are hardly recoverable to a user unless they already have some rather specialized knowledge concerning allophonic variation in English that LPD and other such publications do not provide them with.

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REPLY

It is not easy to argue with Sławomir Zdziebko's comments, mostly because I agree with them. He argues that the justification of Wells's admittedly odd syllabifications in the *Longman Pronunciation Dictionary* comes from the fact that this work is "not theoretically focused", i.e. these syllable divisions serve pedagogical purposes only. "The logic of teaching materials is not the logic of redundancy avoidance", says Zdziebko, a position that I am ready to subscribe to. However, I still wonder what use are Wellsian syllable boundaries to anybody at all, if they (i) go against standard syllabification principles, (ii) do not properly represent some allophony (cf *at.om* vs *at.oll*, with tap in the first, not in the second), and (iii) misrepresent some allophony (e.g. both *Ju.lie* and *Ju.ly* contain clear [l], despite their different syllabic affiliations).