Does word frequency affect phonology? Reasons to be cautious...

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The contents of this session
1. How do things stand in terms of the full balance of predictions...?
2. Do the examples hold up to scrutiny?
3. Are frequency effects omnipresent?

How do things stand in terms of the full balance of predictions...?

Let’s see... the interesting ones are where they disagree...

1. Low frequency (‘frequency conserving’) effects should exist
   • UBP – yes ✓
   • FP – yes ✓

2. High frequency effects should exist
   • UBP – yes
   • FP – no

3. High frequency effects should always exist in ‘natural’ changes/rules
   • UBP – yes
   • FP – no

4. Frequency effects, like all phonological generalisations, should always be gradient
   • UBP – yes
   • FP – no
How do things stand in terms of the full balance of predictions...?

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The type of frequency effects that are really problematic for formal phonology are **synchronic** frequency effects
- the kind of variation that is observed in connection with ‘change in progress’ is implemented by speakers synchronically
- in a formal approach, this would involve a rule-based statement
  - such rules should not show lexical effects on the FP assumptions that we have considered

Do the examples hold up to scrutiny? Do synchronic high frequency effects exist?

Reasons to be cautious...

**Coronal Stop Deletion**

Let’s return to the graphs summarising the Dutch case of \( t \rightarrow \emptyset / s,x \_\# \)
- they seem to make a strong case for the existence of a high frequency effect

Frequency of use and likelihood of deletion increase in a highly similar way
- given the interpretation of numbers in such situations, we cannot expect a perfect fit
- however, others interpreting results like this have complained that, to be persuasive, the numbers need to be shown to be statistically significant
And, actually, I slightly overplayed the Dutch case in the graphs by including only the first 20 words
• if we add in all the words that Phillips gives numbers for, the situation may be different

It’s not completely sure what the likelihood of deletion graph is showing
• there is a peak for *mocht*, and lots of variation between 10% and 30%
  o is it clear that the variation is significantly related to frequency?

What about the case of English: $t,d \rightarrow \emptyset / C\#$?

<table>
<thead>
<tr>
<th>Phonetic environment</th>
<th>Verb</th>
<th>% Deletion</th>
<th>CELEX – raw word form frequency</th>
</tr>
</thead>
</table>
| -ld                  | told | 68         | 1763                            | 765
|                     | held | 0          |                                  |
| -it                  | felt | 55         | 1449                            | 456
|                     | built| 0          |                                  |
| -nt                  | sent | 25         | 551                             | 515
|                     | meant| 0          |                                  |
|                     | lent | 0          |                                  |
| -pt                  | kept | 66         | 750                             | 120
|                     | slept| 50         |                                  |
| -ft/st               | left | 25         | 1503                            | 759
|                     | lost | 0          |                                  |

The claim is that:
• once phonological environment is considered – there is a frequency effect

But, is it right to place so much weight on the differences in phonological environment?
• would we expect deletion to pattern differently in [_lt] and [_nt] or [_pt] and [_ft]?
  o a case needs to be made *why* that should be the case
If we simply rank the high-frequency words in terms of their frequency, the correlation with deletion is not impressive...

It may also be relevant to consider that, as Abramowicz (2007) points out:

- t,d-deletion in varieties of English is generally regarded as stable variation
  - while there may be a frequency effect to be seen here, it is not a diachronic fact
  - it could be that that implementation of frequency effects in variation is due to a different mechanism to that which drives phonological change

“time and thyme are not homophones”

The work reported here is statistically sophisticated

- however, the case is less clear that Gahl argues – the full citation is as follows:

  Crucially for the current study, the log frequency of a word was a significant predictor of word duration when all other factors were controlled for: as frequency increases, word duration decreases, when other factors are held constant. This effect, while small, is similar in size to other theoretically important effects on word duration reported in the literature, such as effects of repetition, associative priming, and contextual predictability (e.g. Bell et al. 2003, Shields & Balota 1991), and to the effects of the other factors in the model.

The effect is small – is it robust?

- small statistic effects need some caution in interpretation
  - an effect of this kind has been replicated in some other studies, so it is likely robust
However...

The experimental record on this question is mixed. A majority of studies of homophone durations have so far failed to find differences in the durations of homophone pairs as a function of frequency. Other studies, however, found duration differences in some experiments, apparently varying with presentation order and context. Whalen (1991, 1996) found duration differences when homophones were presented in word lists with the words grouped by frequency, but not when the same words were presented in mixed-frequency lists. Similarly, Guion (1995) found that pairs of homophones differed in duration when the words were embedded in constructed sentence pairs (such as *We’ll need the watch for a few hours, We’ll knead the dough for five minutes*). When the same words were read in generic carrier phrases (*Say . . . to me again*), however, there was no significant difference in duration. Another study that did not report any significant durational differences between homophone pairs is Cohn et al. 2005a,b, which tested words in lists, as well as in constructed sentences, some of which were the same as in Guion’s study.

And

A small number of studies have examined homophone durations in corpora of speech in naturalistic settings. Lavoie 2002 examined the pronunciation of the words *four* and *for* in read speech and in spontaneous speech. Although Lavoie reported shorter durations for the more frequent *for* than for the less frequent *four*, those differences may be related to the prosodic environments of the two items in question, which affects the contextual speaking rate. Indeed, Lavoie’s interpretation of the durational differences is that they reflected effects of articulation in context, rather than differences in the representation associated with each word.

And

Jurafsky et al. 2002 examined the durations of four ambiguous function words (*to, that, of, and you*) in a subset of the Switchboard corpus of American English telephone conversations. A subsequent study (Bell et al. 2003) examined the ten most frequent English function words. Using multiple regression, Bell and colleagues controlled for factors known to affect duration, such as speaking rate, segmental context, pitch accent, and contextual predictability. Once these factors were controlled for, the frequency of the preposition *to* vs. the infinitival marker *to*, for example, was no longer a significant predictor of word durations.
The balance on imperceptible phonetic effects in reduction is:

- the issue is unproven, but certainly possible
  - however, it is not completely clear that all other potentially confounding factors could be accounted for in a corpus study
  - and, however, a reasonable question is: is this a phonological effect?
- it may well be that phonology does not need to account for such effects
  - but that doesn’t let FP completely off the hook...
  - something in a model of language-and-speech will need to be able to account for it

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**Syncope in English**

This does look phonological – the summary is:

There is a process of syncope in English, which is spreading through the lexicon, and is “more advanced in words of higher frequency (such as those just named) than in words of lower frequency” (Bybee 2001, 11)

- High frequency word: *every* [∅]
- Mid frequency word: *memory* [∅ ∼ ə]
- Low frequency word: *mammary* [ə]

There are several reasons to be cautious about this...

- certainly, syncope in English is more complicated that this makes out
  - there is a major debate as to whether syncope is actually a synchronic phonological process in English
It certainly does not just involve the post-tonic cases that are typically discussed in connection with frequency

High frequency word: *every* [ə]
Mid frequency word: *memory* [əʊ]
Low frequency word: *mammary* [ɔ]

Harris (to appear: 5)

“…” = descriptive terms whose status as analytic tools is debatable

Syncope in English: Fact or Fiction?

Syncope: the deletion of a zero

traditional secondary cluster is part of the inventory of well

American Studies, PPCU

Katalin

the targeted vowel is a sonorant and more sonorous than the consonant preceding […] The effect of the second pattern is to contract a trisyllabic sequence into a bisyllabic trochaic foot.

Balogné Bérces, Huber & Turcsán (2011)

<table>
<thead>
<tr>
<th>post-stress syncope</th>
<th>pre-stress syncope</th>
</tr>
</thead>
<tbody>
<tr>
<td>strict sonority constraint(^1)</td>
<td>phonotactically unconstrained (Zwicky),</td>
</tr>
<tr>
<td>Hooper: not before obstruents,</td>
<td>or: less constrained, on a relative scale</td>
</tr>
<tr>
<td>not even in sC clusters(^2)</td>
<td>(Hooper(^3))</td>
</tr>
<tr>
<td>e.g., <em>camera, family, different,</em></td>
<td>e.g., <em>terrain, police;</em></td>
</tr>
<tr>
<td><em>separate</em> (adj), etc.</td>
<td>also in <em>suppose, suffice, potato,</em> etc.</td>
</tr>
<tr>
<td>lexicalized cases</td>
<td>only attested in very fast and casual speech</td>
</tr>
</tbody>
</table>

Is English syncope phonological?
- if so, it creates all different kinds of opacity

Balogné Bérces, Huber & Turcsán (2011)

<table>
<thead>
<tr>
<th>Aspiration(^7)</th>
<th>Tapping(^8)</th>
<th>Voicing</th>
<th>Gemination</th>
</tr>
</thead>
<tbody>
<tr>
<td>sU[p(^b)]osed</td>
<td>li[r]Erature</td>
<td>po[z]ltive</td>
<td>pro[bb]ly (‘probably’)</td>
</tr>
<tr>
<td>[k(^b)]Onnections</td>
<td>ca[r]Alog</td>
<td></td>
<td>lib[rr]y (‘library’)</td>
</tr>
<tr>
<td>[k(^b)]Ollected</td>
<td>ca[r]Ering</td>
<td></td>
<td></td>
</tr>
<tr>
<td>- no aspiration after [s]</td>
<td>no tapping before C</td>
<td>no voiced fricatives before fortis obstruents morpheme-int-ly</td>
<td>no lexical geminates</td>
</tr>
</tbody>
</table>

Any consideration of syncope needs to consider these facts!
Kiparsky (2016) is scathing...

It has been claimed that the syncope of unstressed medial vowels between a consonant and a sonorant is a sound change in English that spreads through the lexicon, frequent words first. According to Bybee (2007), the high-frequency word *every* has undergone it, the low frequency word *mammary* has not, and the medium frequency word *memory* is in the process of changing. Phillips (2006: 97–98) likewise argues that syncopation depends on word frequency, so that *opera, salary, camera, cabinet, memory, history* tend to syncopate more often than the relatively less frequent *broccoli, gasoline, grocery, buffalo, surgery, chocolate*. Her figures show at best a tenuous correlation to frequency (she does not test for statistical significance). But the more important point is that these data are completely irrelevant, because syncope took place in Old English, and one cannot document the conditions of an Old English sound change with modern English vocabulary. None of the words cited by Bybee and Phillips actually underwent the sound change. *Every* is from OE *ǽfre ylc*, not *ǽfere ylc*, and the others were not yet in the language: *memory* is a 13th century borrowing from Anglo-Norman, *mammary* is a 17th century learned borrowing from Latin, and there was no *broccoli, gasoline or chocolate* in Old English. The Old English sound change was phonologically conditioned by stress and syllable weight, and conformed perfectly to the regularity hypothesis (Sievers-Brunner 1965: §158–159; Campbell 1983). It left the language with a productive variable synchronic syncope process, which has existed in the grammar, in a modified form, for a millennium down to the present.

Synchronic syncope is a variable rule whose frequency of application depends on a number of factors besides word frequency. The principal phonological inhibitor is the avoidance of stress clash, e.g. *gén’rative* vs. *génération*. Phonotactics also appears to play a role: sequences like -nm- that involve gestural overlap (Blevins and Garrett 1998, 2004) are avoided, as in *enemy, economy* vs. *enery, refectory* (trumping frequency). There is less syncope before word-level suffixes than before stem-level suffixes, e.g. *hinder* vs. *hindrance*. Opaque forms such as *parchment, poultry, butler, chaplain, apron, dropsy, chimney, remnant, damsel, partner, marshal, captain, laundry* have been entirely reanalyzed in their syncopated form, as have *fancy* and *curtsy* from *fantasy* and *courtesy*, whereas transparently derived words like *cursory, operative, summary, temporal, cidery, buttery, cobblerly, clownery, cookery* can retain the trisyllabic underlying form and remain subject to variable syncope indefinitely as long as their morphology stays transparent, because their trisyllabic pronunciation can be acquired (‘analogically restored’) even by speakers who have only heard them syncopated.

Are any high frequency effects robust...?

• it seems likely so, but the case is far less robust than UBP theorist contend
How do things stand in terms of the full balance of predictions...?

1. Low frequency (‘frequency conserving’) effects should exist
   - UBP – yes ✓
   - FP – yes ✓

2. High frequency effects should exist
   - UBP – yes ✓ (?)
   - FP – no × (?)

3. High frequency effects should always exist in ‘natural’ changes/rules
   - UBP – yes
   - FP – no
As a reminder...

- Tamminga (2014) explains how Pierrehumbert (2002) sets this out:

  Pierrehumbert explicitly extends the claim that frequent words lead sound change to any kind of gradient phonetic change, stating that “any systematic bias on the allophonic outcome would incrementally impact high frequency words at a greater rate than low frequency words” (2002:118). Just as frequent words that undergo reduction in speech should end up being more reduced in the phonetics inherent to their representation, frequent words that are undergoing non-reductive sound change (for example, the raising of /ey/ along the front diagonal in Philadelphia (Labov et al., 2013)) should accumulate advanced tokens more quickly than their less-frequent counterparts.

  In order to understand this, the study of contemporary variation (‘change in progress’ or ‘stable variation’) is crucial, and – luckily – this has been investigated in detail

  Labov (2006) explains that the large majority of changes described as being in progress across the United States in Labov, Ash, & Boberg (2006) show no frequency effect at all


  Labov (ms) writes that

  - “the study of a century of sound change in Philadelphia has found no evidence of lexical irregularity in the fronting of /aw/, /ow/ and /uw/, the raising of /ahr/ and /ohr/, the raising of /oh/or the backing of /e/, as well as the raising of /eyC/*

Pharao (2010) investigated the lenition of /p, b, k, g/ (and other phenomena) in Copenhagen Danish in real detail

- there is massive, variable lenition in Danish – exactly the kind of things that ‘should’ show a high frequency effect on UBP predictions

  Distribution of [pʰ] by position in the syllable

  Pharao found that

  - “[f]or the variables (ow), (p), (b), (k) and (g), word form (log) frequency does not emerge as significant”

- tokens of [p] which are expected to be realized as [b] in distinct, casual speech:

<table>
<thead>
<tr>
<th>Variant</th>
<th>b</th>
<th>β</th>
<th>p^b</th>
<th>deleted</th>
<th>ø</th>
<th>f</th>
</tr>
</thead>
<tbody>
<tr>
<td>Number</td>
<td>408</td>
<td></td>
<td></td>
<td></td>
<td>7</td>
<td>1</td>
</tr>
<tr>
<td>Percentage</td>
<td>76 %</td>
<td>13 %</td>
<td>7 %</td>
<td>3 %</td>
<td>.9 %</td>
<td>.1 %</td>
</tr>
</tbody>
</table>
Tamminga (2014) shows that the several types of the word *like* with considerably different frequencies behave in a way which goes against the predictions of UBP in terms of the introduction of *at-raising* in Philadelphia English.

*Like* has several different lexical entries, all with the same phonological form /lai̯k/

- these have massively different frequencies of occurrence

<table>
<thead>
<tr>
<th>Function</th>
<th>Example</th>
</tr>
</thead>
<tbody>
<tr>
<td>Lexical verb</td>
<td><em>I don’t LIKE the taste of beer anyways</em></td>
</tr>
<tr>
<td>Preposition</td>
<td><em>The tripe itself is almost bland as such, LIKE eating Jello.</em></td>
</tr>
<tr>
<td>Conjunction</td>
<td><em>But uh they didn’t go up together LIKE they used to.</em></td>
</tr>
<tr>
<td>Adjective</td>
<td><em>I don’t know if he’s exactly LIKE his father.</em></td>
</tr>
<tr>
<td>Quotative</td>
<td><em>And she’s LIKE, ”Let’s go to this fortune teller.”</em></td>
</tr>
<tr>
<td>Discourse marker</td>
<td><em>Um LIKE we used to play a lot of running games you know.</em></td>
</tr>
</tbody>
</table>

Tamminga concludes: “The adjective, conjunction, discourse marker, and preposition forms of LIKE are in lockstep throughout the entire course of the change, despite order-of-magnitude differences in their within-dataset frequencies.”

Dinkin (2008) conducted a detailed consideration of a change which is in progress in US English: the **Northern Cities Shift**, which can affect /i, ɛ, æ, ʌ/, and other vowels, at its most extreme, analysing measurements of a large number of tokens of vowels involved.

- one representation of aspects of the NCS is as follows:

\[ 
\begin{align*}
  & \uparrow \quad \uparrow \\
  & \uparrow \\
  & ɛ \quad \Lambda \quad \epsilon \\
  & æ \quad \Lambda \quad \epsilon \\
  & ʌ \quad ð \\
\end{align*} 
\]

A view of the Northern Cities Shift (based on Labov 2010:15)

Dinkin finds that **some small high frequency effect** seems to exist for /i, ɛ/  
- words which contain these vowels move a little more in the direction of the shift than infrequent words  
- however: the **opposite effect** exists for /æ, ʌ/ (and also /ð/ which is not involved in the NCS) as words which contain /æ, ʌ/ move a little less in the direction of the shift than infrequent words
This seems paradoxical, but Dinkin shows that it is understandable along the following lines:

- the NCS changes that affect /ɪ, ɛ/ involve centralisation
- the NCS changes that affect /æ, ʌ/ involve movements away from the centre

This looks like the same kind of thing as the ‘time-thyme’ imperceptible phonetic effect of ‘word reduction’

Dinkin (2008, 9) “it is certainly not sound change in progress in general that is led by more frequent words” – again, conflicting with the predictions of UBP.

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**How do things stand in terms of the full balance of predictions...?**

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2. High frequency effects should exist
   - UBP – yes ✓ (?)
   - FP – no × (?)

3. High frequency effects should *always* exist in ‘natural’ changes/rules
   - UBP – yes ×
   - FP – no ✓
How do things stand in terms of the full balance of predictions...?

1. Low frequency ('frequency conserving') effects should exist
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2. High frequency effects should exist
   - UBP – yes ✓ (?)
   - FP – no ✗ (?)

3. High frequency effects should always exist in ‘natural’ changes/rules
   - UBP – yes ✗
   - FP – no ✓

4. Frequency effects, like all phonological generalisations, should always be gradient
   - UBP – yes
   - FP – no
   - will it make all the difference...?