

Chapter 6

Against Gradual Phonologization

This chapter will serve as a synthesis of the results from Chapters 4 and 5. I will address the challenges these results pose for the most commonly accepted views of sound change, as well as their implications for theories of phonology, phonetics, and language acquisition.

6.1 Conventional Wisdom Regarding Sound Change

There is a conventional wisdom regarding conditioned sound changes like those that I've investigated here that appears to be roughly comparable across disparate research programs that conditioned phonetic changes are the product of gradual accumulation of errors. The results reported in the previous two chapters cast doubt on the gradualness of phonologization. That is, the categorical phonologization of phonetic change appears to occur at the onset these sound changes, and does so so rapidly that a transition period from pre-phonologization to post-phonologization is not observable. So as to avoid knocking down strawmen, I'll first outline a frequently referenced formulation of this conventional wisdom, then describe how it has appeared in a number of research programs.

I believe the formulation by Ohala (1981) is most representative of the conventional wisdom I'm addressing, even though other researchers depart from this approach either in detail or mechanism. As was mentioned, in Chapter 4, Ohala (1981) proposes a model for back vowel-coronal consonant coarticulation which is based on natural coarticulatory properties. Figure 6.1 presents

that they are grounded in properties of the world external to the system of linguistic competence and acquisition. They then percolate up into speakers' linguistic competence through systematic misattribution.

Grounding phonetic changes in the natural systems of production and perception has the benefit of deriving the fact that some kinds of sound change are relatively common, and that they are typically phonetically "natural." Additionally, once these phonetic changes become phonologized and added to the grammar, the explanation for their apparent naturalness can be tied to their origins in phonetic change which in turn have their origins in natural phenomena. As such, it is unnecessary to posit phonetic naturalness constraints on phonological processes, as their observed phonetic naturalness is an historical artifact (Blevins, 2004; Hale and Reiss, 2008).

I would posit, however, that given a hypothetical phonetic change for any speech sound along a single phonetic dimension which is conditioned by one additional factor, that a sufficiently clever analyst could construct a plausible explanation for its naturalness. It appears that for many researchers the naturalness of phonetic change is definitional, rather than a result of empirical investigation. Garrett and Johnson (2011) do point out that the inverse of many common sound changes are unattested. One example they give is that while the palatalization of [k] to [tʃ] before front vowels is common, the backing of [tʃ] to [k] before front vowels is unattested. However, if [tʃi] to [ki] were attested, the explanation for its naturalness is given by Ohala (1981) as hyper-correction, that is, listeners misattributing the phonological target of /tʃi/ as being a coarticulated form of /ki/. Additionally, Kiparsky (2006) provides an elegant counter argument that purely historical accounts of phonetic naturalness alone cannot account for typological gaps. He lays out five hypothetical scenarios where sequences of common and phonetically natural sound changes could produce languages with a productive voicing contrast, but with only voiced word final obstruents, and argues that despite proposals to the contrary, there is no such language attested. A more probabilistic way to phrase Kiparsky's argument is that the rate of attestation of languages with word final voicing (possibly 0) is disproportionately low given the frequency with which sound changes that *could* produce such a pattern happen. I am not arguing here that phonetic changes aren't grounded in natural phenomena, but merely that the sheer obviousness of this

assumption should not be taken for granted.

6.1.1 This conventional wisdom across research programs

I'll briefly outline how this conventional wisdom of error accumulation regarding phonetic change is formulated in a number of research programs here.

Evolutionary Phonology

As outlined by Blevins (2004), the mechanisms of phonetic change assumed by Evolutionary Phonology are very similar to those proposed by Ohala (1981). The three C's of Evolutionary Phonology are CHOICE, CHANCE, and CHANGE, and all three are cases of listeners failing to correctly reconstruct the intentions of speakers. For CHOICE, Blevins (2006) gives the example of a speaker intending to say /tuʔəlaŋ/, and producing the variants [tuʔəlaŋ], [tuʔəlaŋ], [tuʔlaŋ]. A listener then chooses one of these variants as the underlying form for the lexical entry, and if that happens to be [tuʔlaŋ], then syncope has occurred. This mechanism of CHOICE is not quite adequate in detail to account for the phonetic changes I'm investigating here. In Chapter 2, I argued that phonetic changes don't progress as shifting probabilities over discrete options, but rather as a continuous shift through phonetic space. Moreover, a more realistic formulation of CHOICE would have speakers acquiring probability distributions over the available variants. Nevertheless, CHOICE is the most compatible EP mechanism with the conditioned phonetic changes investigated here, where speakers produce a distribution of phonetic variants, and listeners reconstruct new expectations over those distributions.

It is also worth noting that Blevins' (2004, 2006) formulation of the CHOICE mechanism is also incompatible with my theoretical commitments. Specifically, if the phonetic implementation is qualitatively different from its phonological representation, then it is not possible for the phonetic production of a speaker to be wholesale adopted as an underlying form. Rather, it must be translated into a surface phonological representation by the language specific phonetics, then processed by the phonology. When no distinction is made between surface phonetic production and phonological representation, then it is, in some sense, trivially true that phonological innovation

occurs simultaneously with the onset of phonetic change.

At any rate, the primary driving force behind sound change in the Evolutionary Phonology model is the accumulation of listeners' errors in reconstructing the intentions of speakers.

Exemplar Theory

Exemplar theories run the gamut with regards to the degrees of abstractness they allow. For example, Bybee and McClelland (2005) appear to rule out any abstractness beyond the stored phonetic memory traces when they say that

The innovation in this approach is that language knowledge is not stored in the form of items or rules, but in the form of changes to the strengths of connections among simple processing units.

Pierrehumbert (2006), on the other hand, advocates a more hybridized theory, where phonetic memory traces are associated with phonological categories. This latter position appears to be closer to the mainstream of exemplar theoretic research, so it is this position that I will be referring to when I discuss "Exemplar Theory," although the dynamics of error accumulation are essentially the same under most formulations of ET (e.g. Bybee, 2002).

Simulations of sound change under Exemplar Theory, of which Pierrehumbert (2001) and Garrett and Johnson (2011) are good examples, all involve the same basic mechanism of sampling with replacement. When a speaker has the intention of producing a particular speech segment, they sample from their phonetic memory traces and average over them. Typically, either the sample, the averaging, or both, will be weighted by the individual exemplar's "activation strength," which may be a function many factors including the time since the exemplar was originally perceived, the time since the exemplar was last activated, the exemplar's typicality¹, and a number of other potential factors. This average becomes the speakers' new phonetic intention, which they then produce. Of course, production (and perception) is an imperfect process, so the value which gets stored back in the listener's exemplar cloud is perturbed by this systematic error. Figure 6.2 plots

¹Garrett and Johnson (2011) implement the down-weighting of atypical exemplars by excluding them from memory upon perception, but this is equivalent to storing them and giving them a 0 activation strength.

an example of simulated phonetic drift from an exemplar theoretic simulation, based on Pierrehumbert (2001).

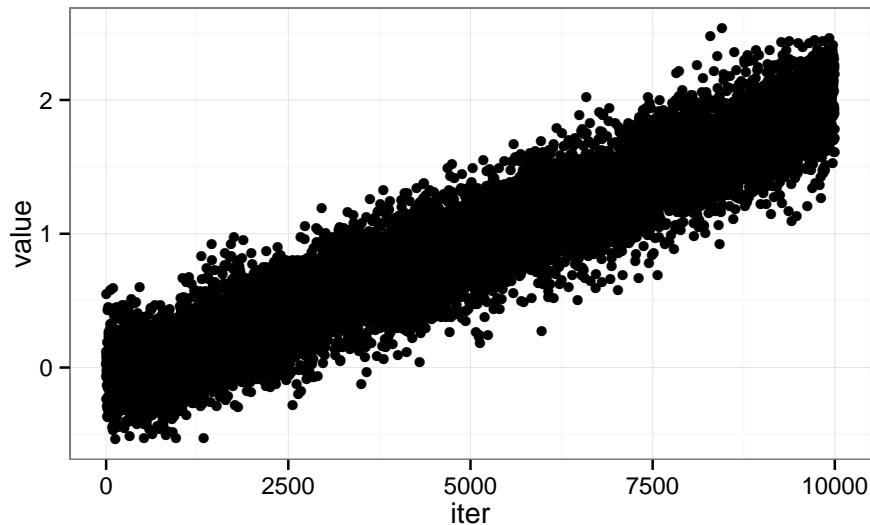


Figure 6.2: Phonetic drift based on exemplar simulation. Model based on the description in Pierrehumbert (2001).

The primary driving force in sound change under Exemplar Theoretic models is the noise introduced by the production-perception feedback loop. When there is a systematic bias to the noise, the exemplar cloud will drift in the direction of that bias. Pierrehumbert (2001) describes this bias in terms of lenition, but coarticulatory drift like that proposed by Ohala (1981) would produce a similar result.

Phonologization

The notion of “phonologization,” whereby a phonetic pattern becomes a phonological one, is central to a number of research programs which posit a qualitative difference between phonetics and phonology, including the Lifecycle of Phonological Change (Bermúdez-Otero, 2007) and much of Labovian Sociolinguistics. While there is, perhaps, less emphasis on the error mechanisms triggering phonetic change in these research programs, they still commonly assume that phonologization is a gradual process.

The first step of phonologization as described by Hyman (1976, 2008) is some “intrinsic” pho-

netic effect becoming “extrinsic.”² Intrinsic phonetic effects are those which are caused by natural properties of the vocal tract, just the same as those I discussed at the beginning of this chapter, and therefore subject to the same caveats. Extrinsic phonetic effects are the product of the speaker’s competence, and therefore part of either the language specific phonetics, or phonological system. Hyman (1976, 2008) does not actually argue that phonologization takes place by the gradual exaggeration of a phonetic effect, but crucially he ties the distinction between intrinsic and extrinsic phonetics to the *size* of the effect.

When the F_0 perturbations are exaggerated to a degree which cannot be attributed solely to universal phonetics, we speak of a phonologization process. (Hyman, 1976, p. 410)

The empirical fact of phonetic change, as established in Chapters 2, 4, and 5, is that the the phonetic quality of vowels differentiate between contexts gradually, meaning there must be a gradual transition from the point in time where we could consider a phonetic effect to be “intrinsic” till the point in time where the phonetic effect has gotten large enough for us to consider it “extrinsic.” This conceptualization is compatible with both approaches where the boundary between phonetics and phonology is fuzzy in reality, not just for researchers, and with approaches which make a stronger assumption about qualitatively different phonology and phonetics. Under the assumption that phonology and phonetics are qualitatively different, phonologization could be conceived of as the gradual approach towards a tipping point, whereby a secondary change reinterpreting the phonetic difference of a vowel between contexts is reinterpreted as a phonological one.

Labovian sociolinguistics as a research program has traditionally made a distinction between phonological and phonetic effects, and has typically operationalized this difference in terms of the overlap of two phonetic distributions. As Labov, Karen, and Miller (1991) say, “[t]hat linguistic categories are discretely separated into mutually exclusive nonoverlapping sets is perhaps the most fundamental concept of linguistics.” This is not quite the same as the phonetic effect size metric Hyman (1976, 2008) proposes. They would classify near-mergers, for example, differently, since the size of the phonetic difference between categories is small, but so is the degree of overlap.

²The second stage Hyman (1976, 2008) describes involves rule inversion, and the phonemicization of phonological patterns. On this transition, Bermúdez-Otero (2007) has developed a more articulated model.

The Labovian approach to distinguishing between phonetic and phonological effects is perhaps best illustrated by the discussion of /æ/ tensing in various North American dialects in the Atlas of North American English (Labov et al., 2006, p 173–184). In particular they contrast two patterns of /æ/-tensing: the Nasal System and the Continuous System. The Nasal System has two clearly distinct allophones of /æ/. One is low and front, close to canonical [æ]. The other is longer, higher, more peripheral, and can have an inglide: [eə]. This tense-/æ/ is restricted to appear just before nasals, and the phonetic distributions of the two allophones are non-overlapping. The Continuous System is very similar, covering about the same of phonetic variation, but there is not a clear separation of allophones into non-overlapping distributions. As Labov et al. (2006, p. 180) say, however, “[i]t is evident that a continuous system of this sort differs from the nasal system only in the degree of differentiation of the vowels before nasal consonants.”

The Nasal System could be considered a phonologized version of the Continuous System, distinguished by a larger phonetic difference, and smaller phonetic overlap with /æ/ in non-nasal contexts. If a dialect with the Continuous System were to transition to a Nasal System, it would necessarily have to do so gradually, per the results of Chapters 2, 4 and 5.

6.1.2 The challenge posed by my results.

My results pose a challenge to the common and intuitive idea that conditioned phonetic change occurs due to the accumulation of production and perception errors, and that phonologization is a gradual and gradient process. First, in Chapter 4, I found that in conditioned changes where some context did not undergo the change, that context was categorically excluded from the change at its outset. Specifically, in the case of /ow/ and /uw/ fronting, these vowels before /l/ have remained unchanged, and never showed any sign of fronting along with the other contextual variants of these vowels. That is, [owl] and [uwl] allophones appear to be categorically distinguished from other allophones at the very outset of the change. Moreover, for most of the contextual variants of the vowels investigated, they moved in parallel throughout the century, even if they had very large effects. For example, the effect of a following nasal on /aw/ fronting is fairly large. In fact, around the turn of the century, the phonetic difference between [aw] and [awN] was greater than

the phonetic difference between [ow] and [owl]. Yet, the size of this effect is not predictive of their parallelism. [aw] and [awN] move in lockstep together, even beginning to reverse their trajectories together, while [ow] and [owl] begin to diverge nearly immediately.

Out of all the phonetic variants investigated, only one fits the profile of gradual divergence, and potential phonologization: /ow/ followed by nasals. Before nasals, /ow/ begins to front at about the same rate, but stalls out earlier than /ow/ in other contexts. Figure 6.3 plots these predicted trajectories from the rate of change model, along with [owL] for comparison.

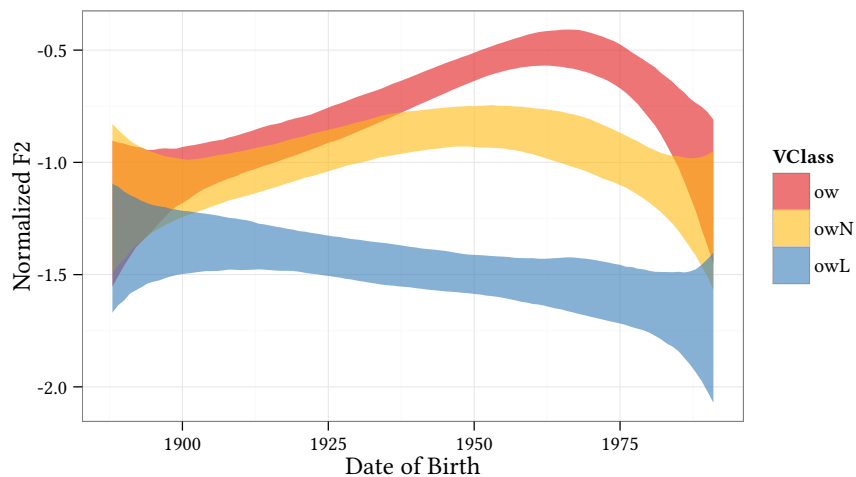


Figure 6.3: Predicted trajectories of change for /ow/ variants.

Perhaps this is the archetypal example of a phonetic process gradually becoming phonological which would be predicted under the accumulation of error model. However, it is not exactly an ideal case. Even though [owN]’s rate of change is reliably slower than [ow] (which admittedly was the diagnostic I proposed for distinguishing between phonetic and phonological effects), it still reaches its maximum around the same time as [ow], and even begins to retract with it. Figure 6.4 plots the predicted rates of change for [ow],[owN] and [owl], and while [owN]’s rate of change curve is in a much more compressed space than [ow], it is qualitatively very similar, especially when compared to [owl]. It seems clear that the link between [ow] and [owN] was not completely severed, as it was between [ow] and [owl], and that they are destined for similar outcomes.

If a difference between [ow] and [owN] was not phonologized, the question remains as to

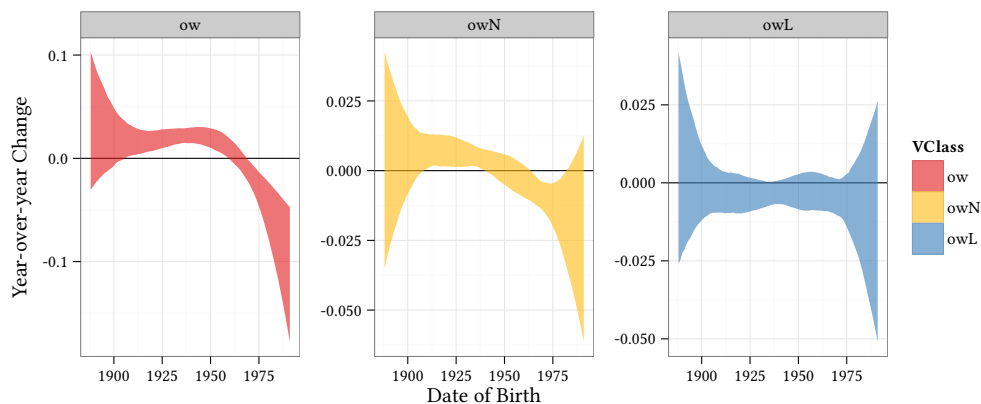


Figure 6.4: Year-to-year differences for variants of /ow/

why [owN] stalled in its fronting. The answer may be that the phonetic effect differentiating [ow] and [owN] is different from the kinds considered in Chapter 4. It should be noted that the effect a following nasal has on the F2 of /ow/ (backing) is the *opposite* of the effect it has on the F2 of /aw/ (fronting). Figure 6.5 plots density distributions in unnormalized Hz for /aw/ and /ow/, contrasting oral and nasalized variants. It appears as if the effect of nasalization biases F2 away from the vowel system center, rather than consistently in a particular direction along F2. For /aw/, which is fronting and raising, the direction away from center is essentially unbounded, allowing [aw] and [awN] to move in parallel without any apparent ceiling effects. For /ow/, on the other hand, as it fronts, it is minimizing its distance from center, perhaps amplifying the phonetic effect of nasalization. That is, the fact that [owN] slows down and stalls sooner than [ow] may also be due to a ceiling, or barrier, effect introduced by nasalization.

If we reconsider the divergence of [owN] as being due to a phonetic barrier, rather than due to a phonological reanalysis, then in fact *none* of the phonetic effects investigated in Chapter 4 became phonologized. Categorical allophones were excluded from the sound change from its very outset, i.e. they were phonologized from the very beginning. Phonetic variants moved in parallel with each other until their trajectories were perturbed by other phonetic factors, like ceiling or barrier effects.

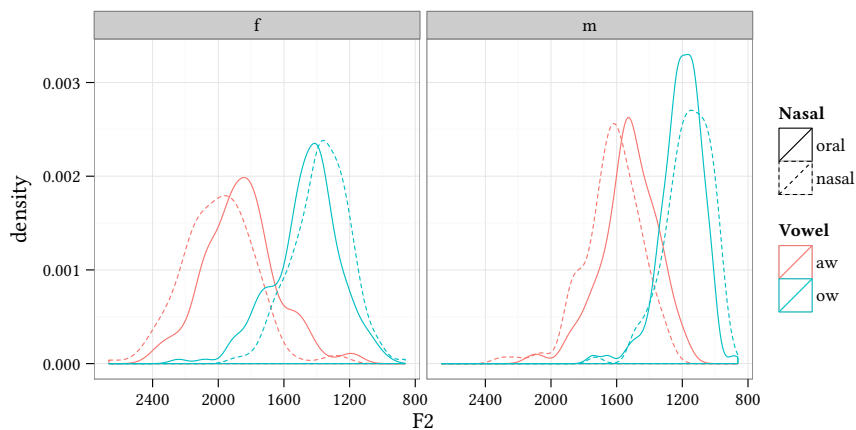


Figure 6.5: Density distributions of /aw/ and /ow/ along unnormalized F2. Dashed lines indicate nasalized vowels, solid lines oral vowels.

The results in Chapter 4 argue most strongly against a gradual process of phonologization. In Chapter 5, I found that the factors which categorize contexts as undergoing or not undergoing a change are best defined on phonological, not phonetic, grounds, at least for /ey/ and /ay/ raising. Perhaps the most surprising result is that /ay/ raising has applied opaquely with respect to flapping from the very outset of its phonetic change. Despite the demonstrable phonetic differences between surface /t/ and /d/, and their flapped forms, /ay/ raising has always applied according to the underlying voicing of the following segment. An alternative explanation to this phonological one based on lexical analogy would have to somehow take into account that /ey/ raising interacts transparently with its context. While every lexical item for /ay/ has only one or the other allophone in all contexts ([ɑɪ]: *ride, rider*, [ɪɪ]: *write, writer*), this is not true for lexical items for /ey/, which may have one or the other allophone depending on their context ([ɛɪ]: *pay, paying, pay off*; [eɪ]: *pays, paid, pay me*). The opaque interaction of /ay/ raising with flapping is surprising beyond just the fact that it is unexpected in the model of gradual phonologization. The Lifecycle of Phonological Change, for example, predicts that new phonological processes ought to interact transparently, and at the phrase level, like /ey/ raising does. I'll briefly discuss an analysis below which harmonizes this intuition from the Lifecycle with the /ay/ facts in Philadelphia.

The surprising result from the analysis of /ey/ raising is that the phonetic context which appeared to favor the direction of the change the most, a following /l/, did not undergo the change itself. If /ey/ in the other pre-consonantal contexts never reached the degree of fronting and raising as [eyl], it might have been possible to describe [eyl]’s non-participation in the change as a ceiling effect. However, around 1925 pre-consonantal /ey/ clearly crosses over [eyl] and continues raising, as Figure 6.6 shows. This cross over is unexpected under the accumulation of error model of phonologization. If [eyl] was higher and fronter than /ey/ in other contexts, then we should expect errors to accumulate in this context sooner and faster than in the other contexts. The non-participation of [eyl] can only be accounted for if some other factor besides its phonetic properties distinguish it from other pre-consonantal /ey/, and I argue that those properties are its phonological representation.

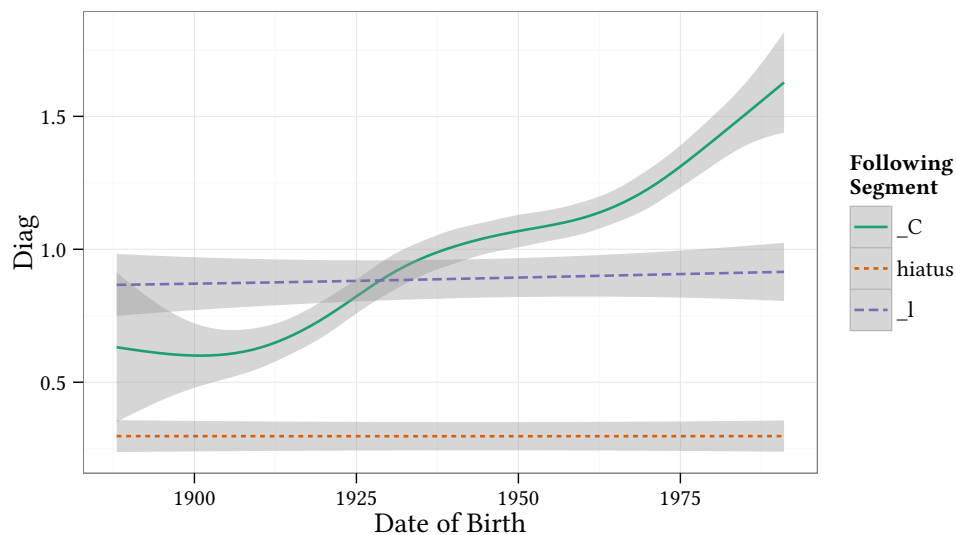


Figure 6.6: Trajectory of word internal /ey/

The preponderance of results in this dissertation so far are at least unexpected under the model of gradual phonologization. At least to the degree those models of gradual phonologization make predictions about how the process of phonologization ought to appear in diachronic data, my results have not conformed to those predictions.

Pre-Existing Phonological Processes

One possible explanation for my results which is still consistent with gradual phonologization is that in all of the cases of abrupt phonologization in this dissertation, there were actually pre-existing phonological processes in the grammar which created categorical allophones along some other phonetic dimension. For example, a different phonological process could have created two allophones of /ay/ which differentiated them along duration, and all that I observed was the further differentiation of these two allophones along an additional phonetic dimension. In fact, this is what Bermúdez-Otero (2004, p.c.) proposes to account for the surprising opacity of /ay/ raising at the outset of the change, which is either not predicted, or predicted not to be possible by the Lifecycle of Phonological Change (Bermúdez-Otero, 2007). This would mean is that I have not observed any instances of phonologization in this dissertation at all, just shifts in the phonetic realizations of pre-existing allophones.

Briefly, the Lifecycle would predict /ay/ raising to progress in the following stages, if it existed in a phonological vacuum. First, once phonologized, the new phonological process raising /ay/ to [aɪ] ought to interact transparently with the surface phonology. This would predict raising in *write*, but not *writer*, and only if the /t/ wasn't flapped in a phrasal sequence like *right on*, as well as raising of word final /ay/ triggered by a following voiceless word onset. Opaque interaction with flapping, producing raising in *write* and *writer*, would come about through subsequent domain narrowing, but the exclusion of raising in phrasal context, like *lie to* would be harder to account for.

The results in Chapter 5 demonstrate fairly conclusively that this predicted sequence of historical events is *not* what happened in Philadelphia. Instead, raising always applied opaquely with respect to flapping, and the onsets of following words were never triggers for raising word final /ay/. Bermúdez-Otero's proposal is that there was a pre-existing phonological process which created two allophones of /ay/ that had the same distribution as the raising process: pre-fortis clipping (i.e. pre-voiceless vowel shortening). Pre-fortis clipping is a long standing phonological process, is present in most dialects, and crucially, as Bermúdez-Otero (2004) argues, shares the same distribution as /ay/-raising, including the opaque interaction with flapping. The argument

is that before /ay/ began to raise phonetically, there were already two phonological allophones: [ai] and [ǎi], and the phonetic change raising pre-voicless /ay/ targeted only the clipped allophone, [ǎi]. Whether or not an additional phonological innovation needs to be posited is an open question. Perhaps the only phonological process in the grammar is stem-level pre-fortis clipping (6.1), and all that is changing is its phonetic realization.

(6.1) ai → ǎi / ___ –voice]_{stem}

Or, as has been suggested to me by Bermúdez-Otero, a new phonological process is added at the phrase level which targets just [ǎi].

(6.2) ǎi → Δi]_{phrase}

This analysis preserves my core argument that phonetic changes operate over surface phonological representations, but does weaken the argument that phonologization is an abrupt process, because phonologization has not actually been observed in this case.

It might be possible to make a similar kind of argument for /l/ blocking the fronting of /ow/ and /uw/, because as I pointed out in Chapter 4, glide deletion for /aw/ before /l/ is a long attested feature of the Philadelphia dialect (Tucker, 1944), and the effect of /l/ on /ow/ and /uw/ could be seen as an extension of that process. For the conditioning of pre-consonantal /ey/ raising, though, it would be more difficult to propose a pre-existing phonological process. Unlike /ay/ raising, which is conditioned by just following voiceless consonants, /ey/ raising is conditioned by all following consonants, including /w/, /y/ and /r/, but not /l/. There isn't any precedent for /ey/ to split along these phonological lines reported for other dialects in the Atlas of North American English, and there isn't any other kind of phonological process I am aware of which differentiates allophones based on whether they are followed by a consonant or a glide, versus a vowel or /l/. In many ways, /ey/ raising conforms much more closely to the expectations of the Lifecycle of Phonological Change, especially in that it applies at the phrase level. Yet, it still appears to exhibit abrupt phonologization. Despite having the phonetic effect of shifting /ey/ in the direction of the change, a following /l/ never actually conditions the change itself.

It is worth considering, though, what it would mean if /ey/ raising were actually parasitic on

a previously existing phonological process. There is no evidence for such a process, but let's say that it is principled to say that one must have existed in order to explain the apparently abrupt phonologization, and that this original process entered the grammar through a mechanism of gradual phonologization. Furthermore, let's say that this is a principled explanation for any case where phonologization appears to be abrupt, which is, in fact, every case analyzed in this dissertation. The consequence would be that I have failed to observe any true instances of phonologization in this dissertation. If this is true, it would be disappointing, but would also cast doubt on the observability of phonologization. The answer to the question "What kind of data is necessary to observe phonologization?" would be "A corpus with a deeper time depth and broader coverage of the speech community than the PNC." As it is, the PNC is unparalleled in these respects, and a corpus with even an equivalent time depth and broader coverage of any speech community is unlikely to be developed any time soon.

6.2 Big Bang

My argument for an abrupt and early process of phonologization is in line with the proposal by Janda and Joseph (2003) for a "Big Bang" model of sound change, with some modifications. Their outline of their Big Bang model is quoted here in (6.3) (Janda and Joseph, 2003, (3)).

- (6.3) A "Big Bang" Theory of Sound-Change –
- (a) sound-change originates in a very "small", highly localized context over a relatively short temporal span;
 - (b) purely phonetic conditions govern an innovation at this necessarily somewhat brief and limited point of origin;
 - (c) this brief "burst" of (an) innovation partially determines its future trajectory as it spreads through an individual's usage and through a speech community;
 - (d) the purely phonetic conditions of (b) are rapidly supplanted during spread – stage (c) immediately above – via speakers' imposition of phonological and sociolinguistic conditions, with the result that the future course of the process is thereby deflected;
 - (e) further reanalyses wholly or partially in terms of morphological and/or lexical conditions (= morpholexical – i.e., "grammatical" – ones) represent commonly occurring ultimate divergences from the initial unity of the

closely contextualized original innovation (regarding the later stages of at least one such development, see Janda 1998 on High German umlaut).

It's not exactly clear whether the examples Janda and Joseph (2003) describe can be accurately be described as being purely phonetic in origin. For example, in their example of Romance prothesis, whereby Latin word initial /sC/ clusters became /esC/ in Spanish and French, they reject "word initial" as being a possible phonetic context, because word boundaries are properly considered a phonological domain. However, they argue from evidence that the vowel prothesis was sensitive to the final segment of the preceding word, only applying if preceded by a consonant, that prothesis began "in the form of a syllable-structure-driven repair strategy." Both syllable structure and the notion of "repair" seem properly phonological. A much more phonetic explanation would probably involve something like perceptual reanalysis of consonant release as a vowel, like Blevins (2004, p. 156-7) suggests is the case for some innovations of epenthesis. It appears that for Janda and Joseph (2003), "phonetic" conditioning means something more or less like "phrase level phonology," while their process of phonologization is more analogous to domain narrowing for Bermúdez-Otero (2007), or to rule generalization.

However, Baker et al. (2011), in their critique of the error accumulation model, examine a case which does appear to go from narrow phonetic conditioning to phonological conditioning. They looked at inter-speaker variation of /s/ retraction in /str/ clusters. They classified their subjects into "retractor" and "non-retractor" groups, measured the centroid frequencies of these speakers' /s/ and /ʃ/, and compared this to the centroid frequency of /s/ in the /str/ contexts. Using ultrasound data which was also collected, they found a positive relationship between the speaker specific similarity between canonical /s/ and /r/ articulations and their degree of /s/ retraction in /str/ contexts for only the non-retractors. Their reasoning was that the more similar a speaker's /s/ and /r/ articulations are, the greater phonetic influence the /r/ should have on /s/ in /str/ contexts. The positive relationship between the articulatory similarity of /s/ and /r/ articulations and the degree of retraction in /str/ contexts suggests that for the non-retractors, /s/ retraction is simply the result of combining two independent phonetic properties in one context. For the retractors, however, there was no relationship between /s/ and /r/ articulatory similarity and

degree of /s/ retraction in /str/ contexts. Instead, these speakers produced very /ʃ/-like tokens in /str/ contexts uniformly, suggesting that /s/ retraction for these speakers is unconnected to the independent phonetic properties of their /s/ and /r/ pronunciations. In summary, they found that for some speakers, their degree of /s/ retraction was strictly proportional to independent articulatory properties, and that these speakers exhibited a broad range of phonetic variation, while for other speakers, their degree of /s/ retraction was unconnected to other articulatory properties, and that these speakers exhibited a narrower range of phonetic variation.

Baker et al. (2011) propose that the broad range of interspeaker phonetic variation among non-retractors provides the seeds for an eventual sound change. However, the sound change is not destined to happen, as it would be under the error accumulation model. Instead, they propose that the sound change leading to phonological /s/ retraction will only occur once there is an accidental alignment of speakers from less to more phonetic retraction along a relevant sociolinguistic dimension, such that speakers with more phonetic retraction are likely to be emulated.

While this proposal is attractive in that it successfully addresses the actuation problems of “Why now? Why here? Why not before or elsewhere?” as defined by Weinreich et al. (1968), it does not really address how /s/ retraction jumps from being purely phonetically conditioned to being phonologically conditioned, and unchained from the speaker specific articulations of /s/ and /r/. It is not hard to imagine at least two possibilities for how this comes about, though. The first possibility is that once the speakers with a high degree of phonetic /s/ retraction are accidentally socially situated such that most other speakers try to emulate them, the only way for speakers with little phonetic /s/ retraction to emulate them is to resort to phonological strategies. That is, the speakers with little /s/ retraction don’t naturally produce retracted /s/ in /str/ contexts, so the only way for them to emulate speakers who do is to substitute in a different phonological target, namely, the one they usually have for /ʃ/.

The second possibility is that the model of actuation proposed by Baker et al. (2011) actually requires two rare events to occur. First, some proportion of speakers must spontaneously reanalyze phonetic /s/ retraction as phonological, and second, these speakers must be socially situated such that the change spreads. This modification still relies on sporadic interspeaker variation as

the seed of change, but in this case it would be *phonological* variation, not phonetic.

This second possibility is most in line with my results, because the specific proposal from Baker et al. (2011) doesn't fit with the facts of phonetic change I'm investigating. If phonologization began first with broad phonetic variation, followed by social convergence on a particular phonetic target, we should expect to the range of interspeaker variation to be very broad near the beginning of a sound change, and then begin to narrow. In fact, the interspeaker variation at the beginning ought to include in its range the eventual phonetic target that the speech community settles on. Looking at the raising of pre-voiceless /ay/, we can see that this is plainly not the case. Figure 6.7 plots the height of /ay0/ with quantile regression lines overlaid. The darkest central line represents the estimated median tendency of the speech community over time. The first set of slightly lighter lines above and below the median line are the estimated 25th and 75th percentiles over time. The outermost lines represent the 2.5th and 97.5th percentiles, so that the area in between then represents the 95% probability range of interspeaker variation. The range of interspeaker variation has remained fairly constant over time, and it was certainly not broader at the onset of the change. In fact, the 95% probability range of interspeaker variation from 1975 onwards does not overlap with the 95% range of interspeaker variation prior to 1905. The essentially constant range of interspeaker variation observed in this change remains a big mystery for the incrementation problem of how the entire speech community of Philadelphia can move in the same direction year over year, at essentially the same rate.

To recap, my proposal is that the process of phonologization appears to be more similar to the Big Bang proposed by Janda and Joseph (2003) and Baker et al. (2011) than it is to models of gradual phonetic error accumulation, like those discussed above. However, additional modifications to the Big Bang model seem to be called for on the basis of my results. The "brief" period of pure phonetic conditioning of sound change appears to be so brief as to be undetectable. In fact, this phonetic conditioning should probably not be considered part of the change itself. As Baker et al. (2011) illustrated, the phonetic conditioning of /s/ retraction was merely the product of the combination of two independent phonetic properties, and didn't really involve any innovation, in terms of a difference in linguistic competence between generations. Once an innovation is

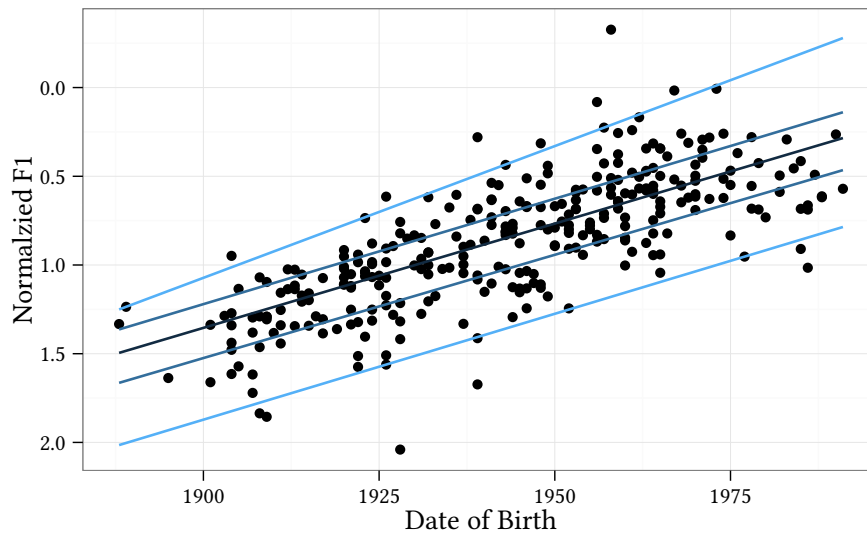


Figure 6.7: Quantile Regression over Speaker Means for [ay0]

observable, it is already phonological.

6.2.1 Plausibility

I am making two specific proposals that in this chapter, and in this dissertation, that may strain credulity.

(6.4) The initial innovation in a conditioned sound change is phonological, thus abrupt.

(6.5) The phonetic correlates of this abrupt phonological innovation are not necessarily large.

However, there is evidence in the literature on language acquisition, phonetics, phonology, and sociolinguistics which suggest that these two proposals *are* plausible.

To begin with, it may appear strange that a phonological process should appear in a speaker's grammar *ex nihilo*, out of nothing. However, if we first accept that the origins of sound changes which cannot be attributed to dialectal borrowing result largely from native language acquisition errors, then this is not so surprising. The language acquisition literature in general is dotted with examples of how children exhibit patterns divergent from the target grammar. When it comes

to phonological processes in particular, a few case studies have identified consonant harmony in children acquiring English (Smith, 1973; Pater and Werle, 2001; Gormley, 2003), a phonological process decidedly not part of the target English phonology. Of course, consonant harmony has not become a language change in progress in English, meaning that either most children abandon consonant harmony grammars before they exit the critical period of language acquisition.

Why would children adopt a phonological process for which there is no evidence in their linguistic input, only to abandon it later? Yang (2002) proposes that similar mismatches between children's syntactic grammar and the syntactic grammar generating their primary linguistic data can be attributed to their probabilistic evaluation of all possible grammars. An example from Yang (2002), most children acquiring English go through a stage of pro-drop because it is possible grammar provided by UG, and in fact most data in children's PLD is consistent with a pro-drop grammar. Only as data incompatible with pro-drop accumulates do children abandon the pro-drop grammar.

The modeling by Yang (2002) is based on a Principles and Parameters model of syntactic grammar, in which the parameters are fixed and finite. The closest existing analogy to phonology can be found in "Classical OT" (Prince and Smolensky, 2004), in which the ranking of a fixed and finite set of constraints is learned (Boersma and Hayes, 2001). However, it isn't necessary for the rules or constraints themselves to be fixed endowments of UG if instead there is some fixed and finite principles by which language learners can hypothesize new rules. Yang (2002) implicitly assumes this second possibility when modeling the acquisition of the English past tense. No one would seriously propose that a rule like $i \rightarrow \text{æ} / ___ T_{past}$ (*sing* → *sang*) is a primitive parameter of UG, but if we assume there are UG principles which constrain hypothesizable rules (Bergelson and Idsardi, 2009), then there is no problem in treating the probabilistic evaluation of language specific, idiosyncratic rules in a way similar to the probabilistic evaluation of UG parameters. Recently, Blaho (2008) and Samuels (2009) have made specific proposals for a set of minimal principles by which more complex and idiosyncratic phonological constraints and rules can be formulated. Blaho (2008) explicitly formulates her proposal in a model of phonological representation which is radically substance free, meaning she does not assume a universal feature set or characteristic

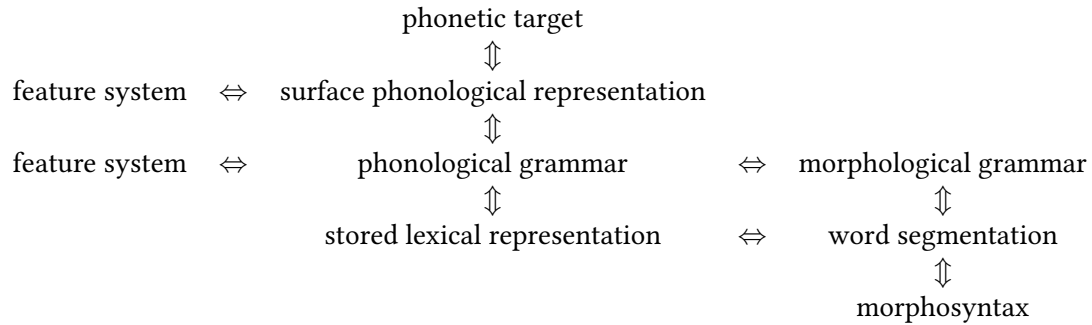


Figure 6.8: Interdependent Language Acquisition Tasks

phonetics for those features, a position to which I am sympathetic in this dissertation.

Some may still balk at the complexity of the complexity of the language acquisition task I am assuming. Not only do children need to learn the association between phonetic targets in a continuous phonetic space and categorical phonological representations, but also the phonological feature set and the set of phonological processes.³ Moreover, I'm arguing that phonological knowledge is not simply the codification of reliable phonetic patterns, so the probability of a phonological process being present in the grammar is not related to the size of observable phonetic differences. However, phonetic and phonological acquisition may be aided by the fact that they are embedded in a network of larger language acquisition tasks, sketched out in Figure 6.8. For example, Lignos (2012) proposes a model of subtractive word segmentation which crucially relies on gradually accumulating lexical representations stored in the lexicon. Of course, what the stored lexical representations *are* depends on the phonological grammar which processes the surface phonological representation. A spontaneously hypothesized process in the phonological grammar could then have the coincidental effect of boosting performance on word segmentation, which could then reinforce that process. In the same way, any new hypothesis at any location in the grammar can have a cascading reaction through this network of interdependent acquisition tasks. So while any single acquisition task may be highly complex, its interdependence on other simultaneous acquisition tasks has the effect of further narrowing the range of possibilities.

³This is, in fact, the exact objection of (Hale and Reiss, 2008, pp. 116-7) to language specific phonetics.

In summary, my proposal is exactly that language acquirers can hypothesize new phonological processes *ex nihilo* because they can freely generate hypotheses which then compete. This has the interesting result that Bermúdez-Otero's (2007) observation that most new phonological processes apply at the phrase level is generalization without as strong an explanation. One possibility, though, may be that most hypothesized phonological processes which become language changes are hypothesized early in phonological acquisition, before learners have mastered word segmentation.

Errors in native language acquisition are likely to be distributed sporadically throughout a speech community, however, and are likely to be highly idiosyncratic compared to something like syntactic acquisition. Looking at the literature, *some* children acquiring English are reported to hypothesize consonant harmony, while most children undergo a stage where they pro-drop some proportion of the time (Yang, 2002). The question arises, then, how a phonological process that a language learner spontaneously hypothesizes becomes a language change, and more importantly, how the entire speech community could suddenly possess the same phonological process. In answer to this, I think it is instructive to look at a case where a speech community has *not* converged on the same phonological process.

Mielke et al. (forthcoming) examine idiosyncratic differences between speakers' articulation of /r/: bunched or retroflex. The articulatory difference between bunched and retroflex /r/ is large, but there is no, or minimal, difference in their acoustics. While most of their subjects were either categorical bunched or retroflex /r/ users (16 out of 27), the remaining subjects exhibited variation between the two variants. Mielke et al. (forthcoming) observe a considerable amount of idiosyncratic constraints on the distribution of bunched versus retroflex /r/. In total, they propose 22 constraints in order to account for the distribution of /r/ variants across all speakers in their study, and of those 22 constraints, 13 (13/22 = 59%) were represented by only a single speaker, and 86% (19 out of 22) were represented by three or fewer speakers. However, some constraints were more common. For example, the constraint *RETROFLEX/CODA, which disallows retroflex /r/ in codas, was the most common, present in 9 out of 11 variable speakers.

The fascinating fact about Mielke et al. (forthcoming) is that speakers who vary between bunched and retroflex /r/ have any structured constraints regarding their distributions at all. Because there are not reliable acoustic cues to which articulation is being used, there is no data available to learners as to which variant they are hearing in any given case. But rather than sporadically distribute bunched and retroflex /r/ across all contexts, speakers appear to develop internally consistent grammars. In some sense, the distribution of /r/ articulations is a constrained version of “the forbidden experiment” of language deprivation. Language learners are deprived of information about which option to take for a decision which is not strongly constrained by UG principles, and the result is internal consistency, but with a high rate of idiosyncratic variation when speakers are compared. Even more fascinating is that *some* patterns are more common than others, a fact that Mielke et al. (forthcoming) attributed to general tendencies in American English for exaggerated anterior gestures in syllable onsets, like that observed for light versus dark /l/ (Sproat and Fujimura, 1993).

We know from language acquisition research that children will spontaneously hypothesize phonological processes, and from Mielke et al. (forthcoming) that these phonological processes may not result in any measurable phonetic difference, *and* that in effective isolation, speakers will spontaneously hypothesize the *same* phonological process. As Ringe and Eska (2013) point out

In any major city in the world there must be at least tens of thousands of children in the [native language acquisition] developmental window at any given time. If only one child in a thousand persists in a learner error until the period of [native language acquisition] is past, that type of event will be too rare to be recognizable in any sociolinguistic survey, yet there will be a steady stream of new variants brought into the speech community as the children grow up.

And importantly, drawing from the results presented by Mielke et al. (forthcoming), many language learners will spontaneously hypothesize and persist in the *same* “error,” or mismatch from the grammars of the previous generation. In order to persist into a speaker’s adult grammar, following the logic of Yang (2002), all the newly hypothesized phonological process needs to do is not lose, meaning there just has to be little enough data inconsistent with it.

In order to be identified as a language change, the new phonological process must diffuse throughout the speech community, and that process imposes its own narrowing effects on the

change. Following the reasoning of Ringe and Eska (2013) and Baker et al. (2011) and the results of Mielke et al. (forthcoming), I'll suggest that there is a constant stream of children with idiosyncratic phonological grammars surrounding some potential innovation, and that occasionally the distribution of speakers with the innovation will coincidentally correlate with sociolinguistic dimensions which promote its spread through the speech community. Labov (2010b) argues that language learning is largely outwardly oriented, meaning children are socially motivated to coordinate their grammars to conform to their peer group. Citing work by Payne (1980) and Kerswill and Williams (2000), he argues that children abandon the models of their parents in favor of their peer group. This tendency to conform to the consensus of the peer group would, in most cases, eliminate the idiosyncratic phonological innovation of any single individual, which is why it is necessary to propose that in order for a language change to take place, it would have to be independently innovated by many children, which again appears to be plausible given the results of Mielke et al. (forthcoming).

This outward orientation of language acquisition may also play a role in the small phonetic correlates of categorical phonological innovation. The fact that Mielke et al. (forthcoming) found phonological variation which correlated with nearly uniform acoustics is, I believe, enough of a plausibility test to demonstrate that phonological differences don't necessarily correspond to large acoustic differences. But with the case of bunched versus retroflex /r/, the fact that there is no acoustic difference is due to the fact that the two articulations produce the same acoustics, and we might not expect this to be the case for all phonological innovations. Moreover, a shift from bunched to retroflex /r/, or vice versa, has not become a change in progress, for the very reason that there is no acoustic difference for speakers to attend to.

However, let's say that a learner hypothesizes a phonological process which creates two allophones of /ay/: [ay₁] and [ay₂]. The speaker now has to decide what the phonetic realizations of [ay₁] and [ay₂] ought to be. If they're living in a speech community for which most speakers have only one allophone of /ay/, then the best way to conform their two allophone grammar with the broader speech community is to decide that [ay₁] and [ay₂] have very similar phonetics targets. This is similar to the argument that Dinkin (2011) makes for the backing of short-o (the LOT

vowel) in Upstate New York. Most of Upstate New York participates in the Northern Cities Shift, which includes the fronting of short-o towards [a] or [æ]. However, Upstate New York is bordered to the South by Western PA, to the North and West by Canada, and to the East by Northern New England, all of which have the low-back merger of /ɑ/ and /ɔ/. Dinkin (2011) finds that in Upstate New York, the phonetic difference between /ɑ/ and /ɔ/ has been decreasing in response, he hypothesizes, to contact with merged dialects. This mirrors the famous “Bill Peters Effect,” Labov (1994) whereby a speaker living in a merged dialect region still produced a reliable phonetic difference for the phonemic contrast between /ɑ/ and /ɔ/ in free conversation, but produced them merged in a minimal pairs task. My conclusion on this point is that even if there were a natural tendency for language learners to posit large phonetic difference to go along with phonological differences, these phonetic differences could get reduced by sociolinguistic homogenization.

6.2.2 Big Bang Summary

In conclusion, my results are more in line with a “Big Bang” model of conditioned sound change in which phonological innovations occur at the *onset* of the change, rather than as a reanalysis later on. Both the facts that this means that speakers are innovating a new phonological process *ex nihilo*, and that towards the beginning of this change the phonological innovation corresponds to a small phonetic difference are plausible given what we know about language acquisition, phonology, phonetics and sociolinguistics.

6.3 Similarity to syntactic change.

It is worth noting that a debate between gradual versus abrupt phonologization closely mirrors a similar discussion in syntactic change. Hyman (2008, p 398-9) actually draws the connection between “phonologization” and “grammaticalization,” drawing the four part analogy “phonetics : phonology :: pragmatics : syntax.” In a review of grammaticalization and gradualness, Traugott and Trousdale (2010) describe the basic position on gradualness in much of the grammaticalization literature:

Gradualness refers to the fact that most change involves (a series of) micro-changes, an issue which is sometimes overlooked in considerations of more general patterns of language change. As Brinton and Traugott (2005: 150) observe, although change is sometimes understood (or at least formulated) as $A > B$, studies of gradualness in linguistic change attempt to uncover “the tiny local steps between A and B that the arrow ‘>’ encompasses”.

This is very similar to Kroch’s summary of the field of historical syntax in 1989.

The idea that language change proceeds context by context, with new forms appearing first in a narrowly restricted context and spreading to others only later, has been widely accepted. It has seemed obvious that the ordering of contexts in the spread of a change reflected the linguistic forces causing the change.

Of course, Kroch (1989) was arguing against this position on the basis of the evidence of the constant rate effect. Instead, he argued, syntactic change is abrupt and catastrophic, meaning all possible contexts are included in the scope of the change at its onset. More recently, Denis (2013) has made the same argument for the distribution of a change across pragmatic contexts. Denis (2013) examined the frequency of use of utterance final particles (UFP) (e.g. *right*, *you know*), and found that even though younger speakers appear to use the new UFP, *right*, in a broader range of pragmatic contexts than older speakers, this appearance is strictly modulated by their baseline usage frequency of *right*. That is, in his data, the fact that older speakers are only observed to use *right* in 2 out of 10 possible pragmatic contexts is quantitatively indistinguishable from the hypothesis that they can and do use *right* in all possible pragmatic contexts, but they use *right* at such a low frequency to begin with that it would take more data than is feasible to collect to observe them doing so.

The results in my dissertation further cement the position of Fruehwald et al. (forthcoming) that the mechanisms of phonological and syntactic innovation are fundamentally similar. Innovation in both sound change and syntactic change is abrupt, and does not take place through the gradual reanalysis of phonetic or pragmatic phenomena, respectively. In both cases, after the original innovation, most of the observed change involves either increasing phonetic differentiation or increasing frequency of use of the innovation. There is no reason why sound change and syntactic change *must* have been subject to the same dynamics, but it does appear that they are.

6.4 Additional Challenges, and Directions for Future Research

There are a number of interesting research questions which I have been unable to address in this dissertation which I will have to reserve for future research. For example, I believe that the difference between phonological allophones and phonetic variants ought to have broader sociolinguistic consequences than I have been able address here. I conclude that the difference between [ow] and [owl] is phonological in origin, while the difference between [aw] and [awN] is phonetic. From this, I would assume that it is possible for [ow] and [owl] to have disconnected stylistic usage, while it would be impossible for [aw] and [awN]. That is, a speaker could not raise [awN] to an extreme level for a stylistic purpose that they could not also raise [aw], whereas a speaker could front [owl] for a stylistic purpose which is separate from the stylistic fronting and backing of [ow]. It has already been established that pre-voiceless /ay/ has this property, where the backing of [ay0] indexes masculinity and toughness while the frontness or backness of low [ay] has not been reported to have any similar indexical purpose (Conn, 2005; Wagner, 2007). At the moment, however, this reasoning is completely speculative, and requires more careful studies of stylistic variation which take the distinction between phonological and phonetic variants into account.

Another interesting direction of research would be to investigate how phonological allophony for one vowel can influence others. Labov (2010a), for example, argues that allophonic chain shifting is impossible. To support this argument, he looks at dialects which have a large difference between pre-nasal and pre-oral /æ/. In those dialects where pre-nasal /æ/ is extremely raised and fronted, he finds no concomitant fronting of pre-nasal /a/. However, in the Northern Cities Shift, where /æ/ is uniformly raised and fronted, there is concomitant fronting of /a/ to [a] or [æ]. Labov (2010a) attributes the lack of allophonic chain shifts to the “Binding Force” of segmental phonology, or the dictum that “phonemes change.” While the absence of allophonic chain shifting cast doubt on loosely structured phonemic representation like those proposed by Exemplar Theory, they may not be entirely impossible. Many North American dialects exhibit a phonological process distinguishing pre-nasal and pre-oral /æ/, but none have been reported to do something similar for /a/, so while there may be two phonological allophones of /æ/ ([æ̃] and [æ]), there is

only one for /ɑ/. The fact that pre-nasal /ɑ/ does not front in reaction to pre-nasal /æ/ raising could simply be because there is no relevant allophone to front. If Labov's assertion that allophonic chain shifting is impossible is true, a more complex model of the phonology-phonetics interface than the one I've pursued here will be necessary. Specifically, the interface will need to define a relationship between a phonetic target, a surface phonological representation *and* its phonemic identity. On the other hand, if an example of an allophonic chain shift is discovered, then the explanation for the lack of pre-nasal /ɑ/ fronting in the dialects Labov (2010a) investigated will have to shift to the distinction between phonological and phonetic variation that I just described.

A more important problem to address, however, is the challenge of identifying phonological innovation, specifically the assertion of Ringe and Eska (2013) that it will be "too rare to be recognizable in any sociolinguistic survey." However, if it is the case, as I have argued, that the phonological innovations which become sound changes are those innovations which multiple speakers are likely to independently produce, then they shouldn't be *impossible* to detect. The focus and methodology of the sociolinguistic surveys aimed at detecting these innovations may need to be adjusted. For example, the Peaks model of language change incrementation (Labov, 2001; Tagliamonte and D'Arcy, 2009) places most of the action in incrementation squarely on adolescents, specifically between the time they first enter their peer groups and the end of adolescence. Focusing on this demographic of speakers will be necessary to identify new phonological innovations.

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