

# Language Change over the Lifespan: A Test of the Apparent Time Construct

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## 1. The Apparent Time Construct

The concept of apparent time (Bailey et al. 1991, Bailey 2002) is a widely used (and very useful) construct in variationist studies. Sociolinguists regularly use it to make claims about the linguistic history of a speech community without going to the trouble or expense of a longitudinal study. In fact, a very quick glance through the sociolinguistic literature shows that, even since before Labov's (1963, 1966) initial apparent-time studies, there are many more studies that use apparent time than real time.

However, anyone using the apparent time construct needs to be aware that it is based on certain incompletely tested assumptions (Bailey 2002, in a spirited defense of the apparent time construct). This paper deals with one of them—the assumption that, at some point early in an individuals' adult lifespan, that person's vernacular is fixed, and no further significant and systematic changes are possible, absent some sort of major event that would result in extraordinary pressure being placed on the individual's system.<sup>1</sup>

Obviously, real-time data can be used to confirm or rebut this assumption—in fact, using real-time data may well be the only way to properly test it. However, as Tillery and Bailey (2003) point out, there are some problems with the use of real-time evidence that might not be immediately obvious. To begin with, if one wishes to use real-time evidence to test whether individual vernaculars really are stable across the adult lifespan, one must most immediately deal with sampling problems. Despite Chambers and Trudgill's (1998:47) claim that “there is no reason to suppose that speakers who refuse to take part in [linguistic] surveys have linguistic characteristics different than those who do,” and that therefore samples need not be precisely similar to be equivalent to each other or the population as a whole, the proof would be much clearer if the samples studied could be shown to be equivalent in some real way. However, longitudinal studies, whether panel or trend, will not necessarily accurately reflect changes in the wider speech community.

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<sup>1</sup> Exactly what sort of event would result in such pressure is not completely clear, but certainly a pathology affecting the speech system (such as, for example, a stroke affecting the muscles or the mental processes involved in language use, or cancerous growths affecting the structures of the vocal tract) would qualify.

One important note about panel surveys, however: Whether the sample can be considered representative of the wider speech community or not, individuals are always comparable with themselves (barring vocal tract pathology or a similar significant event placing major pressure on the linguistic system). That is, in testing whether individuals' vernaculars remain stable over the adult lifespan, it is worthwhile, perhaps necessary, to focus on individual behaviors, rather than the behavior of the group.

No matter whether a trend or panel study is used, or even if one simply compares studies conducted at different points in time to see whether changes in a particular age cohort have occurred, there are other difficulties faced in attempting to determine whether individuals' adult vernaculars are stable or not. For example, in cases where sociolinguistic or dialectological interviews are involved, changes in the interview instrument may result in individuals giving different responses, even though their vernaculars have not changed. In 1983, when Trudgill (1988) conducted a follow-up to his 1968 study of Norwich, England, he left the survey instrument unchanged to make sure the follow-up would be directly comparable with the original study, even though this meant that he had to forgo some of the methodological advances of the intervening years. Trudgill also had to deal with possible interviewer effects, since he (the original fieldworker) had changed in the intervening time, and he worried that his changes would influence his informants. Because of this, he chose a fieldworker for the restudy who was similar in many ways to the original fieldworker at the time of the original study. Such precautions may not be enough, however: One individual fieldworker can easily skew the results of a study, as documented by Nerbonne and Kleiweg (2003) for the LAMSAS survey and Bailey and Tillery (1999) for the LAGS survey. The issue of interviewer effects is such a difficult problem that it might not be possible to get around it if one wishes to use traditional sociolinguistic or dialectological survey methodologies when conducting a real-time study to test whether adult vernaculars are actually stable.

With these factors possibly influencing the results of any real-time study of a speech community, it is unsurprising that those that have been conducted to this point have come to conflicting conclusions on the question of whether linguistic systems remain stable over the course of the adult lifespan.<sup>2</sup> Studies of communities as disparate as Tours, France (Ashby

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<sup>2</sup> It is noteworthy that studies have reached conflicting conclusions even though they all at least implicitly—and probably correctly—abstract away from cases involving things like brain traumas, and at least implicitly assume that small changes in situation, such as moving a block (as opposed to thousands of miles) away from

2001), Texas as a whole (Bailey et al. 1991), a town in rural Texas, (Bailey 2002, reporting on an unpublished study by Patricia Cukor-Avila), southern India (Bhat 1970), and Philadelphia, Pennsylvania (Labov 1994:101-107) have given results supporting the claim that adult vernaculars are stable. On the other hand, however, Trudgill's (1988) restudy of Norwich, England gave at best ambiguous support for the claim, Nahkola and Saamilahti's (2004) study of southern Finland raised significant caveats about it, and studies conducted in Montréal, Québec (Blondeau 2001, Blondeau et al. 2002, Sankoff et al. 2001), Rio de Janeiro, Brazil (Callou et al. 1998), and Andalusia, Spain (Morillo-Velarde Perez 2001) have at least to a large extent contradicted it.<sup>3</sup>

## 2. Testing the Stability of Adult Vernaculars

Since part of the problem involved in testing the assumption that individuals' vernaculars remain stable throughout the lifespan comes down to problems of making sure that the samples taken at each point in real time actually are comparable, this study takes several steps to make sure that each point in real time is directly comparable to other points. In particular, this study focuses on individuals, rather than on an entire speech community—in effect, this is a panel study, though clearly not involving a representative sample of the wider community. In this way, we can see whether individuals change over their lifetimes, recognizing that if any individuals show significant linguistic changes during their adult lifetime, the assumption that adults' vernaculars are stable is disproven.

However, as Tillery and Bailey (2003) point out—and as mentioned above—if individuals on a panel are to be considered useful for this sort of test, they have to be carefully selected. For example, they can't undergo any sort of major physical or mental trauma that would affect speech production during the course of the real-time study. In addition, to provide the optimal situation for their vernacular remaining stable, they should experience neither major upheavals in their social standing during the course of the real-

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where one grew up, are part of the normal course of human existence.

<sup>3</sup> Others' interpretations of these results may differ—for example, Nahkola and Saamilahti (2004) list Trudgill's (1988) restudy of Norwich, England as offering support for the apparent time construct, while here it is listed as offering "at best ambiguous support" for it. Such differences can be seen as primarily a result of what the focus of this list is—the interpretations of the findings of the studies listed here are based on the very narrow (and very strictly applied) question of whether the studies' findings completely and unambiguously support the assumption that adults' vernaculars are stable.

time study, nor moves to a different region.

When designing a real-time study, it is impossible to come up with a cohort of informants who will be guaranteed to meet these requirements—knowing which potential informants would undergo these sorts of changes would require the ability to see into the future of their lives, and linguists do not have that ability. However, if one uses an archive of preexisting recordings of individuals extending well into the past, one can decide after the recordings are all made which individuals to focus on, by choosing the ones who did not undergo such changes.

However as Tillery and Bailey (2003) have also again pointed out, the use of preexisting evidence has its own problems—one has to make sure that the methodology used to collect the data is the same, that there is nothing about the fieldworkers or survey instruments or such that were used that would throw off the results, and so forth. Therefore, it seems reasonable to look for an archive of recordings that is not subject to such problems.

### 3. Data and Methodology

To do this, this study uses a collection of recordings that were not originally collected for linguistic purposes. A number of quantitative sociolinguistic studies have been conducted that have used archival data not originally collected for sociolinguistic or dialectological research, using data from oral history recordings, archives of early radio broadcasts, and so forth. Using such data gets around many of the problems listed above—in most such cases, for example, there is no survey instrument to have potentially changed. In addition, while some such archives (particularly of oral histories) may have data collected by fieldworkers, many others do not, thus getting around the issue of potential fieldworker effects.

This study uses an archive of recorded religious addresses.<sup>4</sup> The recordings in the archive are made up of somewhat formal semi-extemporaneous speeches made by religious leaders, delivered in person and via broadcast media to an audience made up primarily of believers.<sup>5</sup> All of the addresses were delivered in the same place (at the same pulpit, in fact) in Salt Lake

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<sup>4</sup> Specifically, the archived recordings of General Conferences of The Church of Jesus Christ of Latter-day Saints, archived at the Harold B. Lee Library at Brigham Young University in Provo, Utah. The archives include complete recordings of these twice-yearly conferences from the mid-1940s to the present, largely complete recordings from the early 1940s, and spotty coverage from the mid- to late 1930s.

<sup>5</sup> There is some other content, including budgetary and statistical updates, songs, and prayers. These items are not included in the analysis.

City, Utah, and the content and rhetorical style of these speeches remains consistent throughout the duration of the archive. In addition, since the archive spans several decades, with the same speakers appearing regularly, some individuals can be tracked over a relatively long span of time.

The five speakers analyzed in this study (listed in Table 1) were chosen because they are all from the Wasatch Front of Utah near Salt Lake City and lived in or near Salt Lake City (that is, they did not experience dialect contact through moving to a different region), they did not undergo any pathologies that would affect their speech production, and they did not change their social status greatly during the times their archived speeches cover (they all spent the entire time as full-time religious administrators at the highest levels of their church's bureaucracy). All of this means that the individuals tracked are excellent test cases for the assumption that individuals' vernaculars remain stable throughout the adult lifespan.

<b>Name</b>	<b>Birth year</b>	<b>Years recorded</b>
J. Reuben Clark, Jr.	1870	1940, 1960
David O. McKay	1873	1940, 1960
LeGrand Richards	1886	1940, 1960, 1980
Joseph L. Wirthlin	1893	1940, 1960
Mark E. Petersen	1900	1960, 1980

Table 1: Individuals sampled

Words were then selected from the recordings to provide samples of several different vowels in various environments for analysis. Each speaker's first up to thirty stressed tokens of each variable were taken for each year, with a limit of five tokens of any single word for any single year. A reasonable number<sup>6</sup> of each token type were obtained for statistical analysis for most speakers, except for some vowels in pre-lateral contexts and for / $\omega$ /; these vowels were, however, still analyzed, in order to provide as complete a picture as possible. F1 and F2 values for the vowels in these words were obtained using Praat 4.2 (Boersma and Weenink 2004), and some features (such as monophthongization and diphthongization, glide strength, and certain mergers) were coded impressionistically. Statistical testing was then conducted to determine whether the linguistic behaviors of any of the tested individuals were significantly different in different years. The implications of these tests were simple: If there were no significant<sup>7</sup> differences in the

<sup>6</sup> That is, more than ten, and in most cases around thirty.

<sup>7</sup> As is usually done in sociolinguistic studies, the cutoff for significance was arbitrarily placed at  $p < .05$ .

linguistic behavior of an individual at different points in real time, this was taken as supportive of the assumption that adult vernaculars are stable. On the other hand, since a change in any linguistic behavior could be taken as a challenge to this assumption, results showing a change in any individual's linguistic behavior merited a closer look.

#### 4. Results

We can begin with the simplest cases—those variables that did not show any intraindividual variation across real time. The majority of all behaviors tested fit into this group, lending some support to the assumption that individuals do not change their vernacular systems during their adult lifetimes.<sup>8</sup>

The variables in this group included the *feel-fill*, *card-cord*, and *pole-pool* mergers, /aɪ/- and /oʊ/-monophthongization, the raising of /aɪ/ and /oʊ/ before /t/, the centralization of /ɪ/, /o/-fronting, and the diphthongization of /æ/.

This is not terribly interesting, however—it is a finding, but a finding of a lack of a finding. Much more interesting are the cases where there is significant intraspeaker variation in different years. There are only three such items, but they make up a varied group: the *fill-fell* merger, the raising of /æ/ before nasals, and /u/-fronting. Given that there is intraspeaker variation for these features, we need to take a close look at the specifics of each of them; they will be presented in the order just given.

##### 4.1. The *fill-fell* merger

Of the three variables for which intraspeaker variation was found, the evidence for the *fill-fell* merger (or, more precisely, the lowering of /ɪ/ before /l/) is the weakest. The reason for this is the number of tokens—the number of tokens of pre-lateral /ɪ/ was low, in the worst case only fourteen tokens for a speaker (Wirthlin) in one year and seven in the other. However, the results remain interesting, and merit reporting.

Figure 1 shows the degree to which pre-lateral /ɪ/ was produced lower than /i/. The values on this graph are shown in a somewhat unusual format: The values are the average position (based on normalized<sup>9</sup> first formant values) of pre-lateral /ɪ/ between the average heights of word-final /i/ and

<sup>8</sup> In fact, some vowels (checked /æ/ and word-final /i/ and /u/) showed such a remarkable stability that they could be used as reference vowels for some tests.

<sup>9</sup> All vowel systems analyzed for this paper were normalized using so-called Nearey normalization (Nearey 1977).

checked /æ/, which were stable for all speakers across years; the result is expressed in terms of the percentage of the distance from /i/ to /æ/, which means that higher percent values reflect a lower position for pre-lateral /ɪ/.

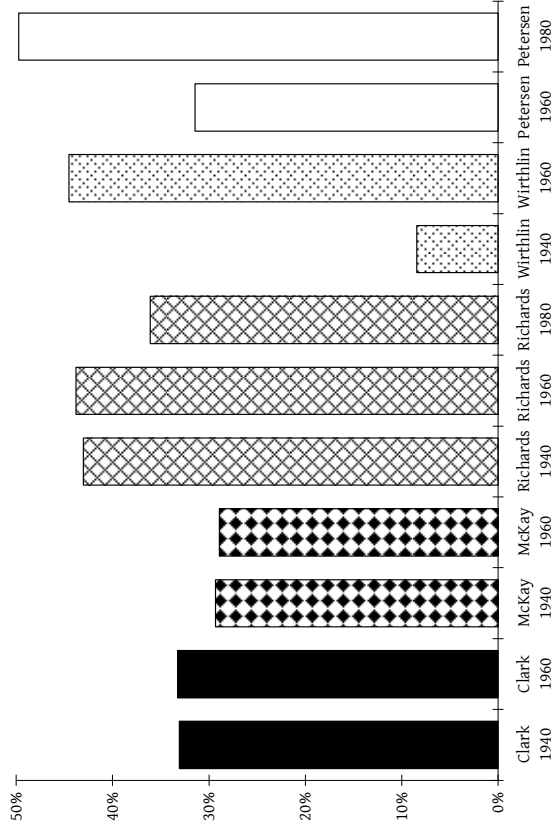


Figure 1: F1n of pre-lateral /ɪ/ relative to word-final /i/ and checked /æ/

If individuals' vernaculars really are stable across their adult lifespans, one would expect these values to be fairly constant not necessarily across speakers, but certainly across years for each individual speaker. This is the case for Clark, McKay, and Richards (the drop-off to the 1980 values for Richards is insignificant). However, for Wirthlin and Petersen, there is a significant difference in their behavior from one point in real time to another.

There are some comforting commonalities between these two speakers' behaviors, however. First of all, this variable clearly exists in Utah English, and it is socially salient (Lillie 1998). It has been found, though, that linguistic variables that are in flux in a speech community may be reflected by changes in individuals' vernaculars as they age, so that one variant gains dominance (Nahkola and Saamilahti 2004). Therefore, it is not particularly surprising that there are changes among individuals for this variable.

It is also noteworthy that in both cases, the change is from a higher position for pre-lateral /ɪ/ to a lower one. This raises the possibility that what is going on here is actually a case of age-grading, where in this speech com-

munity speakers have the option of lowering pre-lateral /ɹ/ as they grow older. However, even though the changes we see in Wirthlin's and Petersen's production are not mortal blows to the assumption that adults' vernaculars are stable, they are nevertheless uncomfortable facts.

#### 4.2. Raising of Pre-Nasal /æ/

Figure 2 shows the degree of raising of pre-nasal /æ/. The graph shows the average position of pre-nasal /æ/ (based on normalized first formant values) in relation to word-final /i/ and checked /æ/. The results shown are percentages of the distance from checked /æ/ to word-final /i/ for each speaker, so higher percent values reflect a higher position for pre-nasal /æ/.

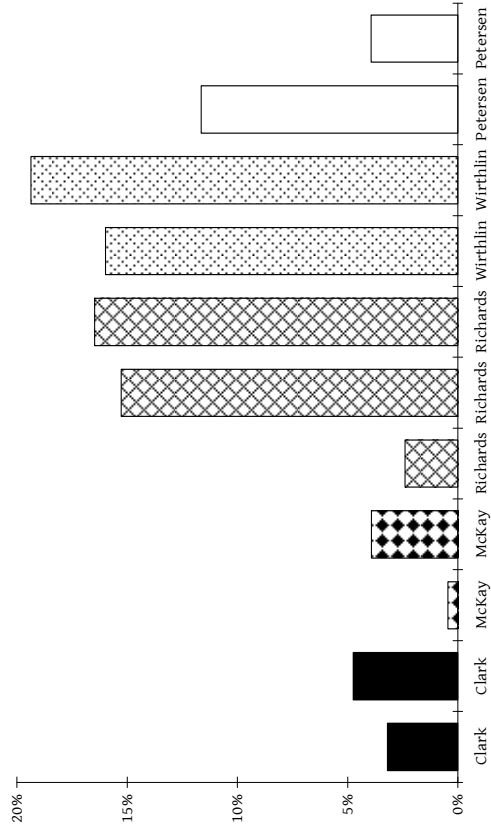


Figure 2: F<sub>1</sub> of pre-nasal /æ/ relative to checked /æ/ and word-final /i/

Not all of the differences shown in Figure 2 that look sizable at first glance are, in fact, significant. Specifically, Clark and McKay exhibit no significant raising of pre-nasal /æ/ above checked /æ/ for any year (so there are no significant differences from year to year for them), and Wirthlin exhibits significant raising of the pre-nasal vowel above the checked vowel in both years sampled with no significant difference from year to year.

Richards and Petersen, on the other hand, are more interesting. Richards's 1940 data shows no significant raising of pre-nasal /æ/ above the checked form, but there is significant raising in both 1960 and 1980 (and, therefore, a significant change from 1940 to 1960), though those last two years are not significantly different from each other. Petersen, on the other



hand, shows significant raising in 1960 but not in 1980, making for a significant change from year to year similar to Richards's change from 1940 to 1960, but in the opposite direction.

What makes this particularly interesting is that, unlike the lowering of pre-lateral /ɹ/, Richards's and Petersen's changes do not seem to be the result of age-grading. Since age-grading involves individuals in a speech community making certain changes as they grow older, accounting for speech community-wide changes, one would expect that age-grading would consistently occur in one direction. However, this is not the case here—the two speakers that show changes in raising of pre-nasal /æ/ raise in opposite directions.

Unfortunately, no previous study has looked at /æ/ in any environment in Utah, and as a result it is impossible to say whether this feature involves truly competing variants. Therefore, it is impossible to rule out individuals selecting one variant over the other as they age (with Richards selecting the raised variant, Petersen the unraised one), following Nahkola and Saamilahti (2004). While this would not be a particularly satisfying analysis on behalf of the assumption that adults' vernaculars remain stable throughout the lifespan—in fact, it raises more questions than it resolves—it does, at least, provide an explanation.

#### 4.3. Fronting of Word-Final /u/

The fronting of word-final /u/ presents yet another challenge. Figure 3 shows the degree to which word-final /u/ is fronted,<sup>10</sup> with higher values reflecting greater fronting. The values underlying the graph show the average percentage of the distance of word-final /u/ (based on normalized second formant values) from word-final /o/ to word-final /i/ (both of which were stable for all speakers across all years).

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<sup>10</sup> Of course, /u/ is most strongly fronted following coronals. Because of the way vowels were selected for analysis, there is essentially the same proportion of post-coronal and post-non-coronal tokens of /u/ for each speaker and year, making each sample comparable.

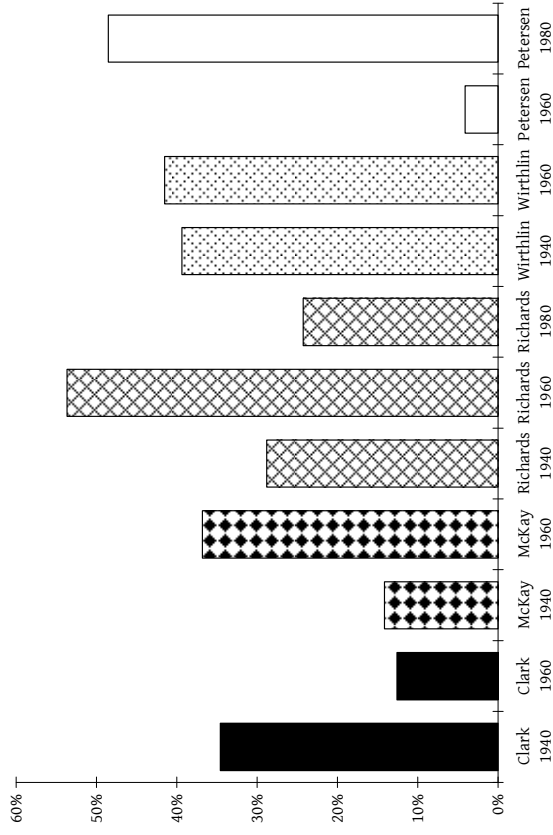


Figure 3: F2n of word-final /u/ relative to word-final /o/ and word-final /i/

Only one of the speakers shows no significant change across real time: Wirthlin. All of the others show significant changes in their /u/-fronting behavior, and the changes will be dealt with in this order: Clark, McKay, and Petersen shifting their degree of fronting (Clark to less of it, McKay and Petersen to more—Petersen changing, in fact, from essentially no fronting at all to a high degree of it); Richards showing a rather troubling pattern of exhibiting moderate fronting, changing to a significantly higher rate of fronting, and then two decades later effectively returning to the starting point; and certain social characteristics of Clark and McKay that make their linguistic changes particularly noteworthy.

Clark's, McKay's, and Petersen's linguistic changes can be explained in the same way as Richards's and Petersen's changes in pre-nasal /æ/-raising above: These three speakers were faced with a choice between competing variants in the speech community (fronted versus non-fronted /u/), and as they aged they made a choice between them (once again, see Nahkola and Saarnilahti 2004). Under this conjecture, the different directions of the changes can be seen as simply choosing between variants. This conjecture about /u/-fronting can be made with more confidence than that for /æ/-raising because some study has been made of /u/ in the US West, including the Wasatch Front. In short, Utah is in the Western United States, and one of the features of Western American English is that /u/ but not /o/ is fronted

(Labov et al., forthcoming)—exactly the pattern we see at least sometimes among the speakers analyzed here. Whether Western /u/ is actually a case of competing variants is unclear, and so this remains a conjecture, but at least there is a plausible explanation.

Richards's up-and-back-again pattern, however, is much more problematic. Richards shows significantly more /u/-fronting in 1960 than 1940, but in 1980 returns to the same level of fronting as in 1940 (the difference between his 1940 and 1980 behaviors is not significant). This sort of pattern does not lend itself to the explanations that could be proposed for any of the changes discussed above—in all of those cases, the change was unidirectional, while here it is not. In fact, even though for most of the changes previously discussed the change had to be unidirectional because there were only two datapoints, Richards's change in /æ/-raising had three points and showed a unidirectional change followed by later stability.

Given the pattern Richards shows for /u/-fronting, however, one must ask whether the apparently unidirectional changes seen elsewhere actually reflected unidirectional changes during the speakers' adult lifespans. Since Richards showed non-unidirectional change over a span of forty years, one has to wonder whether a resolution of ten year intervals would have resulted in similarly non-unidirectional patterns for others' apparently unidirectional changes. This is impossible to answer, of course, given the data we have, but this particular case leads one to think that it is possible.

However, the most important thing to note about Richards's /u/-fronting pattern, at least in the context of this paper, is that it provides solid counter-evidence against the assumption that adults' vernaculars are stable, or even that they are generally stable with occasional unidirectional shifting possible to reflect processes going on in the speech community as a whole (as proposed by Nahkola and Saarialahhti 2004, as previously mentioned).

However, even if we were to find some way to explain away Richards's /u/-fronting as an odd but explicable anomaly we would still have a problem with the opposite trajectories of change exhibited by Clark and McKay.

At first glance, this particular case does not appear terribly different from Richards and Petersen changing their degree of pre-nasal /æ/-raising in opposite directions, as already discussed. However, that Clark and McKay change their degree of /u/-fronting in opposite directions is more interesting because of the great similarity in social markers they exhibit. They were born two years apart, and they both were born and grew up along the Wasatch Front. They both grew up in farming communities, went to the same college, and received their degrees within a year of each other. Between 1940 and 1960 they were both full-time religious administrators, and in fact they had been working together on a near-daily basis at the same level of

administration in their church since 1934, and, by all accounts, were good friends with highly overlapping social networks. And yet, for this linguistic variable, they changed in opposite directions. Why?

Of course, without actual access to the speakers themselves that question, important as it is, is likely unanswerable. However, the fact that there are circumstances that require us to ask such a question means that attempts to salvage the apparent time construct in the face of studies showing that adults' vernaculars are (at least sometimes) subject to change face sizable challenges. In particular, at least at the level of the individual, it may be difficult to use social factors to explain away any postvernacular changes that may occur.

## 5. Discussion and Conclusions

So, now, the important question: So what? To begin with the obvious answer, these results pose a problem specifically for the assumption that vernaculars are stable across the adult lifespan, and more generally for the apparent time construct. Given the sizable changes that this study has documented over the course of time in individuals' linguistic behaviors, at least for certain variables, it is difficult to justify the assumption that the core linguistic behavior of a speaker actually reflects the same system from year to year. It may be—however much this may cause us to shudder—that we have evidence that the apparent time construct must be abandoned due to the real-time facts.

Do we need to abandon the apparent time construct, then? Probably not—after all, even this study found that there was no change from year to year, even over twenty and forty year spans, in speakers' productions of most of the variables that were analyzed. However, there are apparently some problems with the apparent time construct. Evidence has been mounting lately that this is the case—even studies supportive of the apparent time construct have concluded that we need to change our use of it somewhat for it to work (for example, Nahkola and Saanilahti 2004).

However, it would be foolish to abandon the apparent time construct entirely. It has served linguistics well, and it has been proven useful for determining the direction of changes in progress, since restudies of speech communities regularly confirm that changes in progress identified by previous apparent time analyses are, in fact, progressing as predicted. However, since individuals do change their production during their adult lifespans, we need to be more cautious in our use and interpretation of apparent time results.

To begin with, apparent time results need to be recognized as different from real-time results. It seems to be the current custom in linguistics to explicitly mark apparent time analyses, but to treat them as if they say the same thing as real-time analyses. We need to recognize, though, that an apparent time analysis conducted in 2005 cannot reliably tell us about the precise state of a speech community in 1975 or 1945 (although it may be able to tell us the general direction of changes from 1945 and 1975 to 2005). This study also brings up another issue, not obviously related at first glance—a sampling problem. Linguistic studies generally do not involve random samples (even when using what are claimed to be random samples, as pointed out by Davis 1990), nor do they usually involve samples large enough to result in a reasonably small margin of error. This is based largely on the reasoning that members of a speech community all have similar linguistic behavior, once relevant social factors are taken into consideration (probably most succinctly pointed out by Chambers and Trudgill 1998).

However, individuals can behave in ways not easily predictable from social factors, as shown in the case of /u/-fronting behavior exhibited by Clark and McKay. As a result, if we are taking small non-random samples, we can expect to be led astray in our findings about changes in speech communities, no matter how we use the apparent time construct.

So, to summarize: We need to treat our sampling methods and our use of apparent time data with a bit more care. Our previous ways of dealing with these issues have resulted in useful findings, but we cannot afford to allow those findings to lull us into a false sense of security about our methods. We need to learn to use statistically rigorous samples to provide us with better data on the ways that language changes, and to do so we need to learn to use apparent and real time analyses not as substitutes for each other, but as complementary methodologies with, perhaps, different applications.

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