

Entrevista

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Current trends in sociolinguistics and applied linguistics: An interview with William Labov

Tendências atuais na Sociolinguística e Linguística Aplicada: Uma entrevista com William Labov

Interviewee

William Labov has been Professor of Linguistics at the University of Pennsylvania for over 40 years, and throughout his scholarly career has been a leading figure in the study of social diversity in language, language change, narrative, and related areas. He is the author of many papers and books on language and linguistics, including *Language in the Inner City* (1972) and *Principles of Linguistic Change* (three volumes). He was one of the first linguists to work on applied issues such as the teaching of reading, the education of children who do not speak the standard dialect, and public policy towards linguistic and ethnic minorities. He has also been involved in several prominent forensic/legal applications of linguistics, such as the Ann Arbor Black English case in the United States.

Interviewer

Gregory Guy was inspired to study linguistics by reading Labov's work on African American English. Guy grew up in a racially, socially, and linguistically diverse neighborhood in Philadelphia, and found in Labov's work an inspiring approach that recognized popular linguistic reality, analyzed it with scientific rigor, and passionately advocated for the rights and recognition of speakers of any language variety. Guy completed his doctoral studies at Pennsylvania with Labov, bringing the Labovian approach to his dissertation research on Brazilian Portuguese, done in connection with Anthony Naro and Miriam Lemle's research in Rio de Janeiro. He has done further research

on Portuguese, English, and Spanish, in Brazil (with the Varsul project), Australia, Canada, and the Dominican Republic, and is the author of a number of papers dealing with variation and change in language. He is co-author with Ana Maria S. Zilles of *Sociolinguística Quantitativa*.

Gregory Guy (GG): I'd like to begin by asking you about your current professional activities.

William Labov (WL): I'm involved in two very general presentations right now. One is the Franklin Institute symposium, preceding the awards [Labov will receive the Franklin Medal for Cognitive and Computer Science in an award ceremony in April]. What I want to talk about is how technology has been applied to answer the big questions. Then I'm going to Copenhagen to give a talk at the Royal Danish Academy of Sciences and Letters – the first inaugural lectures of a series that is designed to inspire people in the humanities, who don't have a technological focus and don't have a Nobel prize.

I was going to start with the invention of the tape-recorder. It's unbelievable how long it took linguists to use the tape recorder. The wire recorder was invented in 1928, and the German patents for the tape recorder weren't released until after World War II, about 1945, so in 1951 Voegelin and Zellig Harris wrote a paper about two ways of recording data, either asking the informant, or recording the speaker (Voegelin and Harris, 1951). They thought that this new invention called the tape recorder would make it possible to put emphasis on what people actually said, superior to transcription. So that was the first technical step I took, in Martha's Vineyard.

The Kay Sonograph was another instrument that created our field. And the next step was to count, to quantify what we were observing. It's true that Jack Fischer did some counting in his 1958 article (Fischer, 1958), but most of the things people counted were trivial and uninteresting. What was needed is the linguistic orientation to the closed set. When you quantify with that approach, the principle of accountability comes to the fore.

GG: Did Gauchat (1905) count anything in his study of the Swiss village?

WL: No, but Hermann (1929) did. Gauchat was 1905, Hermann came back in 1929.

GG: And what about your paper showing the use of the FAVE procedure (forced alignment vowel extraction) to analyze massive amounts of data demonstrating sound change across the forty years covered by your Philadelphia corpus?

WL: That paper is going to appear in *Language*, by me, Joe Fruehwald, and Ingrid Rosenfelder, and they have just selected it as the article that they wanted to release to the general public (Labov *et al.*, 2013).

GG: What's most useful about that kind of work for the linguistic world, is it facilitating the testing of hypotheses?

WL: Well, the amount of data on spoken English is expanding rapidly. I've just learned about John Coleman's huge project in Britain, the Spoken British National Corpus (BNC; <http://www.natcorp.ox.ac.uk/>), a huge program of recording everyday conversation. Whatever was going on, they'd just turn on the tape recorder. You can download that stuff now yourself. Of course, it has very bad sound in most cases, but this material is being transcribed, so it's available as a text grid too. So it is one of many huge data bases that are just now becoming available.

GG: Do you find this encouraging, since you've always been working in that direction? Do you feel like you were the prophet and finally people have listened to you?

WL: Well, it depends on what your purpose is. Sociolinguistic interviews give you a certain type of data which are useful for many things, Switchboard [a corpus of recordings of telephone conversations collected by the Linguistic Data Consortium <http://www ldc.upenn.edu>] for a lot of other things. So it's a question of being sensitive to the impact of social pressures on language that lead people to change their speech when they're being recorded. We want to be able to get good sound and large volume from people we want to know a lot about.

GG: But you said the sound quality is not so good? Does that mean there are many interesting issues that can't be examined when the recording isn't great?

WL: Well we find ourselves at present in Ling 560 [Labov's course at the University of Pennsylvania on the Study of the Speech Community] doing recording in a bar. So it is noisy, but given the great improvement in vowel analysis, if a human listener can understand it, we can get good measurements on it.

But the big difference between our work and public data bases is this. We tell people when we record them, that no-one will listen to it other than people on the research project. If, however, the data are included in a public corpus, the question arises, what kinds of content will be affected by telling people the recording is public. One crucial matter is attitudes towards race. We find that people are certainly going to modify their expression of attitudes towards race if it's going to be public, because the biggest change that has happened in the last thirty years in American attitudes about race is that it's no longer acceptable to use racist expressions in public. You can get fired for calling somebody a *coon*. But how many interviews are really crucially concerned with race? Well in Philadelphia race is a crucial issue everywhere. So what I'm trying to ask right now it this: Is the trajectory of changes in the Philadelphia dialect, such as the reversal of the back up-gliding vowels connected with attitudes towards race? What is assumed as the default position of somebody who is a strong dialect speaker? I don't have that information. It could be that the movement of college educated people away from the local dialect is simply due to the fact that they think that 'local' is equivalent to uneducated and limited. The other possibility is that they really associate local dialect with negative attitudes toward black people. This is a puzzle that we'd like to unravel.

But to go back to the original question, the important technological steps for our linguistic research were the tape-recorder, the random sample, the Kay Electronic Sonograph, then the development of LPC (Linear Predictive Coding). When I was inducted into the National Academy of Sciences, at the same time B.S. Atal was inducted, who developed the LPC algorithm. That was a great step forward. And then we switched to the Kay Electronic Lab, and then Praat [an acoustic analysis package written by P. Boersma and D. Weenink for personal computers and laptops, available at <http://www.fon.hum.uva.nl/praat/>] was another big step forward, and now the FAVE program (fave.ling.upenn.edu). So at each point when those new technical developments arrived, we were able to ask bigger questions. And of course, the Varbrul program which made logistic regression generally available to people back in 1969, was a major technical advance for data analysis [this was developed by D. Sankoff and others, currently available as Goldvarb: <http://individual.utoronto.ca/tagliamonte/goldvarb.htm>].

The fact that we got the Atlas of North American English done in five years was crucial, because in the last 60 years the traditional methods of dialect geography had

not been pushed beyond the Eastern US and a few other states. So to get a language changing as fast as American English registered for the entire North American continent was possible only because we got the software that allowed us to do vowel measurement efficiently.

GG: There has also been great progress in visual displays. Your talk at the Georgetown NWAV conference (October 2011) used animated graphics showing the change of Philadelphia vowel system across time.

WL: Joe Fruehwald is the leader in this case, and his major claim is that there are things that can be better displayed graphically than numerically. His motion diagram is based on Hans Rosling's work. Rosling dazzled the whole world by giving pictures of social and economic development spreading across Europe and the US, using floating circles that moved and showed development over time in almost a three dimensional transparent system. The motion diagram that Joe developed was based on a website that generalized Rosling's diagrams. But there are actually a lot of other graphic developments that have helped, such as the use of R (<http://www.r-project.org>) and GGPlot (<http://ggplot2.org>), and Joe Fruehwald has on his website special instructions for GGPlot (<http://www.ling.upenn.edu/~joseff/workshop/avml2012.html>). So what these techniques do is allow you to go beyond linear regression and straight lines and if something is changing, moving, the curvilinear pattern can be reflected in an accurate way, with 95% confidence intervals, so you know what's significant. But it only gives you a chance to show three or four independent variables with contrasting panels, so if you've got more than that, multiple regression is still the way to go.

GG: On the graphics and visual presentation of data, I've always seen you as a leader. But I was thinking that as linguists, we're accustomed to analyzing language, but maybe those analytical skills are applicable to the visual domain as well. Have you ever thought analytically about graphical display?

WL: Well right now I'm developing a form of Plotnik [William Labov's program for plotting vowels, <http://www.ling.upenn.edu/~wlabov/Plotnik.html>] which takes the 380 people we've analyzed for Philadelphia and projects the mean values for each of them, and then as we interview new individuals, to place them in relation to the evolution of the Philadelphia system, we can add them two, three, four or five at a time to see their relationship to people of similar ages, similar socioeconomic background, similar ethnicity. So you can learn a lot with graphic displays, but I think the sophistication of mixed level regression allows us to move forward in other ways. One of the things it has done is to include random effects for person or neighborhood or words or whatever. This has confirmed what you and I agree on, which is that the

community is the basis of analysis, because in the great majority of cases, adding those random effects doesn't change anything. People will report the few cases where there is an effect, and argue that such results justify the importance of lexical or idiosyncratic effects, but the real pay-off is in the cases where adding the random factors doesn't change anything in the fixed factors.

GG: Yes, I think that's a striking finding which confirms what we have believed, but another question is what about the people who don't conform to the community pattern, and you've highlighted the importance of exceptional individuals as leaders of linguistic change.

WL: Well in the NYC data (cf. Labov, 1964) there were a couple of people who were exceptional. One was Nathan B, but he was a person whose limitations prevented him from participating in the community patterns, and the same was true of Molly S., who was almost blind and spent all her time listening to the radio. But for me, the important people are the prototypical ones. People like Donald P. in Martha's Vineyard, Larry H. in Harlem, Celeste S. in South Philadelphia – these people are prototypes. Allowing them to speak freely allows you to get at the human values that a full exemplification of the community pattern shows you. And that is not common in linguistic or social science research. I would say that John Rickford's study of Reefer in Canewalk, Guyana (Rickford, 1979) was exceptional, but even people who are talking about the social meaning of individuals don't portray the people as fully as they might. The people disappear.

GG: That's one thing I'm struck about in your new book, 'The Language of Life and Death (Labov, 2013).' You feature individual narratives and life stories, but since you always tell the informants what you're going to do with the recording, did you go back to all these people and ask their permissions to publish these stories in the book?

WL: A lot of the recent materials are anonymized, so you can't identify the people, names and place names have been changed. But the older ones, most of the speakers are no longer with us. I think I'm speaking for them and the book is a way to getting to appreciate the human race, and I don't think that any of the people I interviewed thirty years ago would mind.

That book is not a book of scientific inquiry. The study of narrative is a part of the humanities, where it's very difficult to prove anything. The arguments that are most effective are the things that are logically so. The paper that I gave at the Georgetown Round Table, 'Where shall I begin?' (Labov, 2010a) is based upon the fact that it's impossible to tell a story without beginning somewhere. Every person has to decide where they're going to begin. So there are lots of analyses that can be done from the nature of the materials that you're dealing with. I do emphasize a great deal the linguistic impact of causative

verbs and permissives like *make*, *let*, *drive* and the way people use linguistic devices to change the impact of their stories. And I have done some experiments where people hear a story and then I ask them who was to blame in the situation that was described, and depending on which linguistic form was used, you get differentiation of their responses. But it's not part of the major trend in the study of narrative to use technical devices to solve questions. Rather, it's a way of asking certain human questions about the use of language which are interesting and persuasive, but not definitive in the same sense.

GG: But I felt that that book is illuminated by a lot of serious linguistic analysis. Your discussion of the difference between oral narrative and epic poetry lays out the linguistic differences that you attribute to high style: the elaboration, the more complicated plot structure, and keeping the reader engaged, rather than the coda returning it to present.

WL: But you can't deny that it's quite possible for people to reject any statement that I make about this material and say, "I don't believe it." The major impact of this book is the *interest* of the stories. So suppose somebody says "I'm not interested." There's nothing proved. I'm making an appeal to universal properties – that all people respond pretty much the same way to the narratives.

One small technical thing that I've done most recently, which is not in the book, is to take the story about the first man killed by a car in this town, which begins, "This lawyer, must have got drunk, because he drove through town with a chauffeur in the middle of the night." And the zero causative *drive* is effective enough so that even though the **chauffeur** is the person who got killed (the steering wheel hit him in the heart) over half the people we've asked believe that the person to blame was the lawyer. The word 'drive' makes him an agent, even though he may have been asleep in the back of the car.

So that was one exploration I've done. And several times I've done experiments asking people about interests and impacts but on the whole the book carries its own persuasive message. And anything that makes you more aware of how people tell stories, and what are the human values involved, is a step forward in understanding language.

Today, we have a huge field of NARRATOLOGY. About 50% of the articles in the journal *Narrative Inquiry* refer to the paper by Labov and Waletzky (1967), but very few contain narratives that are interesting enough to hold the attention of an audience.

GG: Probably because I've been influenced by you as my teacher, but I've come to a very broad conclusion about narrative – that that is the basic way that human beings make sense of information in the world, in terms of stories, causation, sequences, relationships, structure.

WL: Jerome Bruner (1991) has a paper on just this point. He sees narrative as a screen through which we see the world. Again his papers don't contain any interesting narratives, they're just about narrative, but that is quite an insight he has. That's just what you said.

GG: Well I've found that when I'm talking to someone and having a problem understanding them that it's usually because there are a couple of things they've said that don't hang together with this narrative I'm constructing about what they're telling me.

WL: I've recently been developing with my current class on narrative a notion that remembering is very important. And there has been a lot of emphasis about what has to be remembered in order to tell a story. In one form of remembering you are still reacting emotionally to the events and feel guilty or uncertain about them, and the other is that you've come to grips with the events and explain what you did in a way that is acceptable to your image of yourself. And if you don't do that, you get a story that is incoherent, with memory lapses, confusion; stories that are hard to follow. I've got some striking examples of that. For example, this story that one of my students just collected had to do with a childhood friend who died when they were 11 years old. The friend died because his mother shot him, and then shot herself. It's the most incoherent story you ever heard. He keeps saying all the time, "This is bad. I can't tell it. I can't tell this, I don't remember." And the crucial event is not related in the narrative. Hearing something like that, I think, what is it like for a kid to hear that his best friend was shot by his mother?

GG: I had a relative who was in the US army when World War II started and his unit was one of the first to be sent overseas when the US entered the war, to North Africa, and they were shot up pretty bad, and he never told stories about the experience of being under fire. He told stories about afterwards, when they were pulled back to Ascension Island and then back to the US, but never about actual combat.

WL: That is what we've found generally. People say that they can't tell you about war because you'd have to have been there to understand it. There's a book by Randall Collins (2008) called *Violence* which tells us that in combat, no more than a quarter of the soldiers actually shoot their weapons. Very few people can deal with the violence of wartime. People don't behave in a way that they think they would like to see themselves behaving.

So anyway, much as I enjoy this book on narrative, it's a separate activity. It comes from our interviewing, but it's quite different from the analysis of the questions of why language changes and how variation is structured. It's a different enterprise.

Now in trying to account for the Northern cities shift and the Philadelphia sound changes, we posit certain

social and emotional attitudes, but the evidence to prove that these correlations are explanatory is not as strong as the evidence of the actual language changes. For example in your work on changes in Brazilian Portuguese, the moment you enter the field of correlating linguistic behavior with social attitudes, you can't make that decisive proof.

GG: I think that's an area that's gotten a lot of attention in recent sociolinguistic work, and I see hopeful signs of progress. In Kara Becker's work on the Lower East Side (Becker, 2010), she found that r-less rates went up in portions of the interview when people were talking about the neighborhood. When talking about NYC topics in general there was no effect, but the talking about specific local characters and events of the neighborhood pushed up people's rate of r-lessness.

WL: Well that is a big step forward. The talk about the social meaning and the social agent frequently assumes that the particular use of a particular form is to establish local identity or to establish membership in some group, or to show dominance or submission, but nobody has ever actually showed that's true. What happens is that when you look at a conversation at the most advanced forms of a vowel change, they aren't related at that moment to a particular social interaction. It's the probabilities that change. There's a probabilistic character, so you can't say that a person is using a particular form in order to achieve a certain social result. Rather, what happens is that the whole base of variation shifts.

This has to do with what we call the sociolinguistic monitor. When people talk about social agency, or a particular social agent, the assumption always is that each utterance is carrying a certain social meaning, and that's never been shown. Don Hindle as you know tried to do this with the Carol Myers data, where she was recorded over a whole day. He came up with the result that the larger units, like the travel agency where she worked, the conversations at home, at the bridge game, were the only ones that really affected her production (Hindle, 1979). This finding of Kara Becker's is really important because it takes one step further in the study of social meaning, but I don't think that you're going to get down to the level where you're explaining individual utterances.

GG: When you're telling a particular narrative and you're adopting a particular voice, that would include a set of probabilities.

WL: Yes, it's a persona, but it's not the same thing as to say, 'I'm using these variables as I would use a lexical item', like saying 'fuck you.' There isn't any phonological or linguistic variable that would be equivalent to saying 'fuck you'

GG: But people do use phonological means for very specific effect in narratives. Your book talks about

the end of the story about the guy who robbed the gas man, and he concludes with a long drawn out vowel, "living on that gaaaassss money".

WL: But that's one of a whole series of long vowels. In his 'toasting' mode, he signals what he was doing by exaggerating long vowels, so this is just an extreme example of a general pattern of lengthening vowels. It's not that the particular word 'gas-man' is associated with this.

This goes back to the matter of probabilistic behavior, and what people learn. The work of Jen Smith (Smith *et al.*, 2009) is a striking confirmation of the fact that kids get socialized into using probabilities. And what I'm trying to get people to look for is that period between the first socialization into the use of probabilities and entering into the broader world where they're maximizing the group of people they can communicate with.

I've given a paper called 'What is to be learned' (Labov, 2010b). The main argument is that when children emerge from their parents idiosyncrasies and neglect them, they don't necessarily just adopt the forms of their friends, they adopt a much broader range of behaviors which allow them to communicate with the largest group they can reach out to, which includes a lot of hostile people.

GG: Your work on teaching reading. Can you say something about how it is informed by your sociolinguistic research?

WL: The work I did in Harlem back in the 1960s was designed to answer the question, are the differences in language responsible for the differences in reading success? And we found the differences are quite precise and very well structured, but we really didn't do anything that answered the question, and it seemed at the time that attitudes towards African American English were actually more important than any structural differences. The article "The Logic of Nonstandard English" (Labov, 1969) was said to have a big effect in the field, but it was essentially just trying to change people's attitudes towards the dialect. But what I found out when I was a member of the National Research Council Committee for the Prevention of Reading Difficulties in Young Children was that feeling better about yourself and your language does not help you learn to read. It was clear that by-passing the alphabet in teaching reading was a fatal mistake which the 'whole language' people made. So in 1996 when I started teaching a course at Penn on African American English, I made it obligatory that students in the course had to tutor children in the local schools who were way behind in reading.

As sociolinguists, we brought three things to the program that was developed over the years. First, linguists know a lot more about the alphabet than the average reading method shows. For example, exceptions to the silent /e/ rule occur entirely before three letters, v, m, and n [cf. *love, some, none*, which have a short-u

vowel instead of the long-o which this spelling indicates in words like *rove*, *home*, *cone*]. And that's because 12th century scribal practice was to change u to o to make it more readable. And second, we know more about kids' language, and that does influence the crucial ability to distinguish between a difference in pronunciation and a mistake in reading, which is to get the wrong word. But I think the most important thing, which we brought from our work with kids in Harlem and elsewhere, is to know what kids are interested in, and what's crucial for their daily lives. And that goes along with our basic principle that 'nice' is not interesting. Most of what they have to read in school is not only nice, but vacuous.

So that program, which is now called The Reading Road (<http://www.ling.upenn.edu/pri/readingroad/aboutreadingroad.html>), we put together on the basis of those three things, and I wrote a lot of stories, that I think kids find very interesting, having to do with the conflicts in their daily life. Some of these kids are now in the third generation of prison re-cycling, where their fathers are in prison, and their lives are full of such grave conflict that they can't really concentrate on anything to do with reading. I've quoted one seven year old girl, Latasha. She said, 'I wish I lived in some other world.' Both her cousin and her brother had been shot dead in the past year. But in this world, she had to keep fighting with other kids who would say nasty things about them.

GG: Is that program being used in schools now?

WL: The Reading Road has been tested on a nationwide scale and it was quite successful. Then two things happened: first, I adapted it for a commercial Houghton-Mifflin-Harcourt Language Arts program called *Portals* (Labov, 2010c) of which I'm a senior author, and all the stories I've written for that are similar to the Reading Road. And that was widely used in California and Texas.

GG: As I understand it, in addition to being interesting and engaging, the stories are supposed to focus on particular orthographic patterns.

WL: Right, they're called decodable stories. The California rule was that in every story 75% of the words had to be decodable by rules already presented. But the decodable stories they had weren't at all interesting, like "Run, Jane, run" and couldn't be used for tests of comprehension. The stories I wrote follow their rules, and if they're in a chapter dealing with the silent e rule, they are loaded with silent e words. And we had a brilliant group of illustrators from Barcelona, who did a wonderful job.

But going back to the Reading Road, the program that we illustrated and developed ourselves. That was associated with courses I was teaching, LING160-1, and three years ago, an amazing thing happened. It morphed into an entirely student run program, with over 60 or 70

students tutoring in the local schools. It has amazed me. They administer themselves, they recruit, train, coordinate, schedules, all done by a yearly succession of student volunteers. It's called the Penn Reading Initiative and it's in great shape; we have a website for it: pri.ling.upenn.edu. And I've been to other universities that are interested in running such programs and I'm trying to figure out how this could be made to happen elsewhere. This has to do with grass roots organizations, and how they occur. I'm not even sure how it can be generalized. There are a lot of institutional-run programs like Head Start (<http://www.acf.hhs.gov/programs/ohs/>), but this has its own momentum. Our program is being used some other places, but that's a small number of cases compared to what the actual applications might be. Meanwhile in West Philadelphia, the Penn Reading Initiative is a great success.

GG: So to have a practical impact on society, is that the way that's more likely to achieve that, as opposed to academic research?

WL: Well from my contact with the reading field and the many people in it, I see that they have developed useful and powerful methods. But the fundamental problem of lots of kids not learning to read has not been solved. And it is the cycle that is producing unemployment, poverty, and mass incarceration, which is now in its third turn since 1970. I think it's the most serious social problem in the United States. Now the idea of Penn's Netter Center for Community Partnership, which has been supporting what I've been doing, is that the university is usually the biggest single employer in every city, and the force of the university directed to the solution of community problems can be quite decisive. But I'm still searching for the way that the Penn Reading Initiative can be generalized, so that it's not reaching just one out of 140 Philadelphia schools with this problem, but many of them.

GG: What about your experience with forensic linguistics, as a witness in legal cases. Have we seen some real social or practical implications of that sort of work?

WL: I've only been involved in a few cases, but some have achieved some notoriety. Like the one in Los Angeles. Bomb threats were being made to the Pan American company. And some executives at Pan American said that the voice sounded like Paul Prinziavalli, who was considered a disgruntled employee. He was arrested and spent 9 months in prison, insisting he was innocent. The UCLA phonetics lab sent me recordings of the bomb threats and the prisoner saying the same words, and it was immediately evident that he was innocent! The bomb threats were made by a speaker with a Boston accent, and Prinziavalli was a clear New Yorker. The people from Los Angeles who had misidentified him were unfamiliar with East Coast dialects. So I was able to testify as an expert witness in a way that went beyond just giving an opinion,

but presenting facts that were so clear that the judge in deciding the case —acquitting Prinzivalli— paid tribute to the objectivity of the linguistic evidence. So that made it clear to me that the law is looking for objective evidence. Lots of times we give testimony that is only an opinion. But in any case where you have evidence that demonstrate facts that must be true, you're doing the law a great favor.

GG: My recollection of the Ann Arbor case [a lawsuit in Michigan that Labov was involved in] was that people thought it had considerable impact to improve the education of African Americans. Do you think it worked out like that?

WL: Geneva Smitherman did a great job of organizing the case and bringing together the testimony and Judge Joiner was very effective in summing up the decision, but what was the end result? Teachers were instructed that they had to learn more about African American English in after-school classes. But it was widely misreported that the teachers had been told they had to learn African-American English (AAE) to communicate with their children. So it was one of the many cycles that led to widespread public misunderstanding, and a backwards step. So in doing the reading program, we decided that we didn't want to lead to more public misunderstanding.

However, I think the Ann Arbor decision did have an effect, it was helpful to the Oakland School board when they tried their Ebonics program. I think that the political climate may be changing slightly, with all the work by Ann Charity-Hudley and Christine Mallinson (cf. <http://charityhudleymallinson.com>), writing books about teaching people about dialect differences, and the work of Wolfram and Adger (1999), and so on, the public climate may be changing enough that it will be possible to do introduce AAE for contrastive analysis in the classroom.

GG: What about other kinds of utility for linguistics in social questions like, for example, linguistic profiling?

WL: Well the work of John Baugh has been extremely effective (Baugh, 2003). Certainly there are a lot of great applications of linguistics to social problems, and I think the most important one lies in the area of the immersion program, starting with the work of Lambert and Tucker (1972), and now various types of bilingual programs.

Our work on reading has contributed something to that. Children who learn to read first in Spanish have a different attitude to the alphabet. Unconsciously they use alphabetic relationships. A good example is the soft-c rule ['c' represents [s] when followed by 'e' or 'i']. Native speakers of English have to be taught the soft c rule, they don't apply it to new words unless they've been taught it, and once they're taught it, they show great improvement. People who learn to read first in Spanish don't have to be taught the soft c rule, they automatically apply it. So it's

clear that learning to read in Spanish first makes you a better decoder. Now you may be so far behind in vocabulary, that we haven't yet found a way to use that, but any time we have findings that show that multilingualism is good for you, linguists are happy. Ellen Bialystok's work has shown lots of ways that multilingualism is good for you. So some of our work on reading is useful, and reinforces that position.

GG: Has sociolinguistic work had any impact on teacher training in the USA?

WL: Well there is *Understanding English Language Variation in U.S. Schools* by Anne Charity-Hudley and Christine Mallinson (2011,) which has been very widely used. The publisher has asked for a second volume, and they might have an impact on teacher training. I don't know enough about the numbers of the trainee-teachers that would have to be reached but that's been very impressive. And Anne has coupled that with large-scale activity at William and Mary College, where she's educating a whole generation of teachers. So it might have a national impact.

GG: Dell Hymes, when he was Dean of the School of Education at Penn, was trying to structure the whole education and research program of teachers around a linguistically informed curriculum. How do you think that worked out?

WL: Well there were aspects in the ethnography of communication which were applied to it, but the methods that were developed there weren't clear enough to prove anything in a way that would really influence people. Let's take the matter of eye contact. One thing that was pointed out was that some black children are trained not to look teachers in the eye, but teachers tended to think that if you don't look people in the eye, you're being dishonest. But how general is that? The fact that things like that are observed or reported in some cases is not decisive enough to say that nationally all teachers should be trained to recognize that African American children who don't look you in the eye are not dishonest.

Stanton Wortham's work on *Learning Identity* (Wortham, 2006) has been quite impressive to me, but I don't think the results of the ethnography of communication have had a decisive impact on education. There are a lot of papers that say 'wouldn't it be wonderful if these techniques were brought into the classroom.' Let's take another example, which I think is quite true but hard to prove. When white kids are interested, they keep quiet — I found this out when retelling stories. Total silence is what I get from the white audience. When black audiences are interested, they start talking, and this rouses a negative response among teachers. And the only way I've found to prove this to people is to appeal to what happens at the movies when whites and blacks are together in the audience. White people are frequently annoyed by vocal responses that come from the black audience — "Right on! Do it!"

GG: I've experienced that in black churches, people make comments like "Tell it, brother!"

WL: If this is true, it has a profound implication for educating black children. Teachers have to be told, don't try to keep everybody quiet! Exactly HOW to manage such an audience is another matter!

The problem for the researcher is that the field of education demands more and more definitive, quantitative demonstrations of claims.

Now we are involved, my colleague Tina Baker and I, in a large scale program to submit a contract to the board of education here in Philadelphia, to turn around schools that have had a bad track record. Whether or not it will succeed I don't know, but this involves a great deal of technology and experience with how to organize schools that I don't have. The fact that we have a reading program and can fit it into a certain slot is good, but you have to organize the program around use of the arts to reinforce education, economic, social and political education. Reading is only one element, and we as linguists have concentrated on raising the reading to a level where it can be used for other learning.

That's where the connection between applied linguistics and our study of language change in progress is; we found out ways to take advantage of the uniform character of dialect patterns. And the orderly variation that you find is an element in our program. So we tell kids for example 'not everybody pronounces these words the same way, not everybody says the /t/ in 'fact', but there it is.

GG: Do you talk about things like 'going to' vs 'gonna'?

WL: Not so much those; in fact we find that kids have a hard time reading 'gonna' or 'wanna'. But I have one paper called 'What is a reading error' published in *Applied Psycholinguistics* (Labov, 2010d), where I raise the point that most of what we're dealing with is potential errors. For example, if somebody reads 'I passed by' as 'I pass by', that could be just a difference in pronunciation, or a reading error in the sense that they didn't get the past tense message from the written form. So this paper shows that you can never prove for any individual case whether the potential error was a true error or a variant pronunciation, but by looking at the frequency of errors the reader makes in the **following** text, you can get a sense of what's happening in the reader's mind. So, for the -ed suffix, marking a past tense verb, the frequency of following errors is almost identical for correct readings and potential errors, but for an omitted third singular -s, it's much closer to the frequency of errors in the text that follows true errors, where there's no ambiguity as to whether or not the speaker made a reading error – like if they get a word completely wrong. So it's a complicated argument but it is an attempt to resolve the question, has

the child made a mistake in decoding the written text or is he or she just using a different pronunciation?

GG: Do you think there is a meaningful field of applied linguistics, and if so what is it?

WL: Absolutely! Speech pathology for example is a vast area, bigger than linguistics, and it has produced a lot of interesting findings. But there is one limitation to applied linguistics that is quite surprising. I gave a plenary talk at the American Association of Applied Linguistics (Labov, 2007) which pointed out that despite their magnificent progress in many areas, there is no stream of reading research; research on reading is NOT considered a part of applied linguistics! And everybody cheered and said 'yes, you're right, reading should be treated in our field', but nothing has changed. In the United States, reading is a separate field from applied linguistics. I don't think that's true in other countries.

GG: That's also true in Australia. When I worked there, there was a big reading conference in which Michael Halliday and several of my applied linguistics colleagues participated, and they were somewhat despairing over the attitudes that prevailed. The slogan of the day among the non-linguist reading researchers was 'reading is caught, not taught', in other words, they thought it was pointless to identify patterns and rules and teach them to children, so reading was supposed to be a holistic way of being and thinking or behaving that you could not analytically decompose and teach.

WL: Well my hope is that the Penn Reading Initiative succeeds, because it does reconcile the important parts of the phonics and whole language approaches. Direct instruction, which focuses entirely on phonics has almost no connection with the extraction of meaning, and once you use the word "meaning", you're seen as having aligned yourself with the 'whole language' people. So, that seems wrong, because the rewards of reading have to be in the meaning that the child gets from page, and if it's meaningful information that applies to their own lives, that's even better. So in emphasizing these decodable stories that are dealing with the same problems that people have in everyday life, we are drawing from the whole language approach their essential idea, which is that's what reading is all about. We are using at the same time, I think in an artful way, the alphabetic principles with an emphasis on decoding and the recognition that sight word learning is seldom successful.

GG: My son had a reading disability, and he made no progress at all until he went into a special education program that drilled him explicitly on all these rules for decoding, and at the same time gave him lists of words that are don't follow the rules, and then he began to learn to read effectively, in grade three!

WL: Well, that leads me to the point that study of reading intersects with a major issue in phonology which has to do with the nature of exceptions, and how they should be treated, and we haven't really done the research to solve the problem. Take for example the 'ee' spelling in English, in 'see', etc. It mostly seems quite regular, pronounced always as [i], but there's one exception, 'been', pronounced [bIn], which is a very common word. This is a dialectal variable in English, so some dialects actually say [bin], but for American English speaking kids, how do you teach this exception? Do you first say 'ee' is [I] in 'been', and elsewhere it's [i]? That's the linguistic approach, the 'elsewhere' condition, to state exceptions first. But I'm not sure it's the right one for teaching reading. There's evidence to suggest that it should be the other way round, that exceptions come last. Because the most important thing is for the child to get confidence in the alphabet, and the existence of regular symbol-to-sound patterns.

Another problem is unpredictable vowel pairs. We have to tell children, look there are exactly two ways to pronounce 'ow': half the time it's going to be [au] (e.g. *cow*, *now*), the other half the time [ou] (e.g., *mow*, *tow*). So try one, if it makes sense, you've got it, otherwise, go for the other pronunciation. I have no idea whether this strategy works, but we've been using it.

GG: Do you think linguistics has had an influence on public policy regarding language teaching in the US or elsewhere? For example, in the education of linguistic minorities, immigrant groups, etc.?

WL: I don't know. I've asked a lot of my colleagues in language teaching, in TESOL (Teaching English to Speakers of Other Languages), etc. 'is linguistics useful?' And I get a variety of answers. Some people say it's a very great tool for learning a language, but not necessarily for teaching it. I just don't have any personal command of the data on this.

GG: Several fields or approaches seem to have emerged that deal with sociolinguistic issues: variationist sociolinguistics, the ethnography of communication, interactional sociolinguistics, discourse studies, the sociology of language. How do you see these approaches as related? Are they different perspectives on common issues, or do they focus on different issues?

WL: Well regarding discourse analysis, I wrote a book called *Therapeutic Discourse* with David Fanshel (Labov and Fanshel, 1977), who recently passed away. He was a professor of social work. And that book has been widely used, but I think it had two problems. One, I don't think it built enough on Grice's work, though it used similar rules for felicity conditions, for requests, and so forth. What it did was to examine 50 minutes of speech of a therapeutic interview with an anorexic patient. And it showed that at all times there are streams of connectivity in

the discourse at different levels of abstraction. The highest level of abstraction is the interpersonal one. When you really understood the interpersonal level, you saw what threats, what challenges, what defenses are being made and how the patient and the therapist are getting closer or further apart. And the closer you get to understanding what's going on, the harder it is to prove anything, because you've dealing with levels of abstraction where your inferences are only probable, but without being measurable. So discourse analysis is a huge field, in which, as in my study of narrative, you can get a deep understanding, but you can't prove anything to people who disagree with you. I think the big advances that we've made in the study of change and variation is that we can prove things to people who don't want to believe what you're saying, whereas in discourse analysis, we're still dealing with a persuasive mode of argumentation, not with proof. But this is not to say that it is not interesting, and I believe that many people in the field are on the right track.

GG: Well is there a potential that it could be brought into the domain of scientific proof?

WL: This raises a question that you can answer as well as I can: Are there closed sets that dictate the possible variables in discourse analysis [like the closed sets of possible phonemes or possible tense markers in linguistics]?

GG: There may be some, but not a lot.

WL: When we really understand what's going on in a discourse we'll be able to solve problems like this. Let's say there's a new discourse marker that has emerged in the last couple of years (such as 'basically') and people are now using it quite frequently. And although we may not know exactly what it means, we can certainly count it. But can we say when does it NOT appear? For example, there are people that use 'like' as a discourse marker in English, in ways that have been studied by D'Arcy (2012) and Tagliamonte and D'Arcy (2009), but we're down to just counting things when they occur, and we don't know enough about their functions to count when they could have been used but don't occur. Or take habitual *be* in African American English. People count its frequency in texts as a whole, but can't say when it could have occurred but didn't.

GG: Well Ronald Mendes did a great piece of work on Brazilian Portuguese on the emergence of the *estar* + gerund construction in place of the *ter* + past participle, which involved discourse-level analysis (Mendes, 2005). Both of these constructions have usages that don't overlap with the other, but in the meaning of habitual or repeated action, one appears to be replacing the other. So the thing that was most compelling in Mendes work was to show the diachronic relation between them, when one goes down, the other goes up. The alternative interpretation of

these results, that people are expressing different meanings across time, seemed nonsensical. Rather, it seemed fairly clear that one form was replacing the other as an expression of the particular meaning of iterativity.

WL: Well I'm going to an international conference in Buenos Aires in honor of Beatriz Lavandera. And I've discovered that not only did she write that extremely influential article (Lavandera, 1978), but she founded a movement, a whole group of people who are continuing in that tradition, and who say they have escaped from the Labovian trap of counting things that are not necessarily equivalent. It's not just the Spanish *si* clauses that Lavandera found to have different meanings in the subjunctive and the conditional; they claim that it's impossible to have different constructions that mean exactly the same thing, so all choices imply a different meaning. They argue further that there's a conflict in society, that different people mean different things by the same words. Take the word 'school' for example: it is argued that the word means different things to the lower working class and the middle class. And they have looked at a lot of high-level political discourse in this regard. So I'm going to give a talk that says that Lavandera's observations about covariation in form and meaning apply to many cases of variation in tense, mood and aspect, but a vast body of work on historical syntax by Tony Kroch and his associates has no reflection of change in meaning (Kroch, 2001). So for example, changes like the emergence of auxiliary *do* in English, or the change from SOV to SVO, do not involve a change in meaning. But the covariation of form and meaning does happen. John Myhill (1996) has a paper showing in the study of 19th century English plays that use of modals changed radically. He argues that the meaning 'must' was more common in the earlier period, indicating a pattern of community obligations, but 'got to' which largely replaced 'must', deals much more with individual choice, a subjective compulsion. So that was a rather brilliant demonstration that people (at least playwrights) are changing their whole mode of expression and orientation. I don't think that kind of event is extremely common.

My experience is more that when people implant forms like causative verbs in their stories, they can rely on the fact that everybody interprets them in the same way. So it relates to the notion again that some people take a Marxist approach, arguing that the variation that we studied in New York City reflects a conflict of competing norms and, and some people take the consensus view, which is one that I still adhere to, which is that people pretty well understand the meaning of variables in the same way throughout society.

GG: Well I've always told my students that your NYC results show two different things at the same time: The common directions of style shifting show that people all interpret the prestige variant in the same way, as

associated with more careful speech and higher status. But at the same time, people are showing that they have other concerns about how they speak in addition to that, because if showing social prestige was the only thing going on, then why wouldn't the lower working class use high rates of /r/, and thereby lay claim to the status and social capital associated with prestige variants? Instead, they are concerned as well about constructing local identity and linguistic solidarity with their peers. What would high /r/ rates mean in their community? It would alienate them from their friends and neighbors.

WL: Well this is a puzzle, Greg, and covert norms, which we proposed and others have talked about, are difficult to demonstrate experimentally, because experiments are conducted in a framework that favors the overt. So people could take the position that it's a matter of personal choice: 'I talk this way cause I want to'. Or they could talk that way because that's the way the people speak that they most often talk to. Or there's a third possibility, that to use the elevated forms you need lots of practice, which people have different experience of and access to. I've just been talking to a student who's interested in Madurese, which, like Javanese, has many high levels, complicated lexicon for honorifics and special inflections, etc., but the average person doesn't control them, because they have very little practice in the use of the elevated forms. Similarly there are native speakers of Japanese who don't control the use of honorifics, because they haven't had the experience to do so. So the third possibility is, it's not just the frequency of interaction, but the amount of practice you've had with certain forms. I think in the case of our most common variables, your approach is quite right, and it shows how different people are and how similar people are. I've switched myself from saying that the biggest problem is to explain how different people are, to saying that the biggest problem is to explain the enigma of uniformity. Throughout Philadelphia, in every neighborhood, they're all doing the same damn thing, the changes are coming at the same rate. All through the 88 million people in the Inland North dialect region of the US, they're all doing the same thing without realizing it. And how does that work?

I keep quoting Valerie Fridland (2003), who asks how can we account for the fact that in Memphis, whites and blacks are moving in the same direction, even though they don't communicate very much. So face-to-face interaction is not necessarily the explanation of these cases of uniformity in the direction of change. Rather there's "a strongly circumscribing historical environment" which leads to the same result. How those general social norms spread is something that your work and my work don't address, because we don't study interactions between different groups.

GG: But you did that study of people in Philadelphia who had strong cross-racial ties, like my brother, who was one of your subjects.

WL: Yes it is true that there are some people like politicians, musicians, confidence men, who regularly have interactions across racial groups, but we weren't actually observing those interactions. We have to study influential speakers, who convey social norms across the community. It's extraordinary to see that the people in Kensington, Port Richmond, Second Street [neighborhoods in different parts of Philadelphia] are all doing the same thing. Maybe one group will be a little bit ahead of the other. But how does that come about?

GG: Celeste's family spanned South Philadelphia and Kensington. [Celeste S. is one of Labov's principal informants in Philadelphia, who is described in his books as a leader in ongoing sound changes in her community]

WL: Yes, but is intermarriage between neighborhoods sufficient to explain the city-wide uniformity? Probably not.

GG: I have a graduate student from Poland at NYU (Luiza Newlin-Lukowicz) who is studying Polish Americans in New York, and she's made the remarkable discovery that, when her speakers use a stop [d] in place of English voiced 'th', they use the VOT (voice onset time) pattern of Polish, with prevoicing of the stop, and when they use [t] for voiceless 'th', it's the same thing: they use unaspirated forms, so *think* is pronounced [tink], not [t^hink]. But their [t,d]s in ordinary English words follow English VOT norms (with [d] showing voice onset shortly after stop release, and [t] aspirated in stressed syllables). So this looks like persistence of at least one phonetic characteristic from the language of their parents or grandparents, even among subjects who are monolingual English speakers.

WL: That's an unusual and interesting case. I have this paper called 'mysteries of the substrate' (Labov, 2008) that talks about substrate effects that we don't understand, but there are certainly cases where the substrate effect is understandable. Now James Walker gave a remarkable paper at the Linguistic Society of America Meeting about the so called 'ng-click' (where an English word ending in the velar nasal is pronounced with an audible [g] at the end). In Toronto it's an Italian stereotype, but in New York it's a stereotype of Jewish speakers. And we came across a speaker of Russian background who does it very strongly, and she's almost 100% saying *thing*, *ring*, with final [g]. And it turns out that her town, Clifton Heights, had a very substantial Russian population. So I gave my students the assignment to find out what Russian has to do with this pronunciation.

But let me come back to the question of the unsolved puzzles in our field. I mentioned the problem of uniformity. Another is the problem of incrementation. When kids move out of their families and the influence of their parents, and enter the community and adopt the patterns of the local speech community, they must be

following models of kids who are older than they are. But when there are ongoing changes in the community, somehow these younger kids wind up ahead of the productions that they're hearing from older kids. Now we could say that this is some form of hypercorrection – that is, you try to match someone's usage, and you just keep going further in that direction, overshooting the mark in trying to accompany the change. But it's still mysterious, that language learners must take as their target someone who is older than they are, and yet they bypass them.

GG: But they only bypass them in adolescence, not in early childhood.

WL: Yes, but that's what keeps the changes going.

GG: Would it be profitable to study individual adolescents? Presumably not everybody overshoots the mark – the average for the age group is ahead of their older peers, but presumably not everybody is ahead, there must still be some leaders and some laggards.

WL: Yes, well Suzanne Wagner (2008) is coming back to follow up on the students she studied for her dissertation. Originally she followed them over one year, as they moved from high school to college. Now she's going to track them after 6 years, after they've finished college. But I think the most interesting age group is the transition from middle school to high school [usually occurring around age 13-14].

I've talked to Penny Eckert about this problem. She has a deep understanding of this from her work in middle schools, and I asked her, does one year make a difference? Are there big changes over the course of one year? She said 'absolutely!' The kids in 8th grade talk a great deal about what they're going to wear and do when they go to 9th grade.

GG: Paul De Decker (2006) followed young people as they moved from a small town in Ontario, Canada to big cities like Toronto and London, and tracked how this affected their usage of changes like the Canadian shift [lowering and backing of front lax vowels].

WL: Well that particular change, lax vowel lowering, is also a big focus for the linguists in California. I thought in the 1970's that we were seeing a tendency for this change in Philadelphia, people saying *left* so it sounded like *laughed* but it turned out not to be so. There is no significant shift of lax-e in our data. The Atlas data show a few speakers in the western US with a back shift of short-a, but in Canada it's everybody (Labov *et al.*, 2008).

GG: That follows from the general principles of Labov *et al.*, (1972), doesn't it? In dialects that have the *cot-caught* merger like Canada and the Western United States, the /æ/ has to lower and back to fill the gap left in the vowel space.

WL: Yes, but don't forget there's a different development in Pittsburgh, where the wedge [schwa] vowel drops down to fill the space and /æ/ stays up and front. Which poses a nice question of why that happened in Pittsburgh but not elsewhere. Pittsburghers pronounce 'duck' like 'dock'. And they also have monophthongization of diphthongs, like /aw/ to /ah/. But what the researchers are finding there is almost a folklorization of the dialect. There are t-shirts for sale where 'downtown' is printed as 'dahntahn.'

GG: Kara Becker's data in New York (Becker, 2010) shows a retreat from all the extreme values of the classic New York City English variables, the raised /aw/ is being lowered, the /æ/ tensing pattern is being restructured in the direction of a nasal system [i.e., the vowel is tensed and raised before nasal consonants, but not elsewhere].

WL: That's exactly what we're finding in Philadelphia. There are three things that are happening. Changes that move in the direction of the Northern dialects (other than New York City), are advancing farther. Changes that aligned Philadelphia with Southern dialects are receding. And third is parallel with Kara Becker's work. Not only is the nucleus of /æ/ lowering, with women leading, but for those with higher education there is a strong shift to the nasal system for short-a. That is something that astonished me, because this means that despite the overall lowering of this vowel, some speakers are actually RAISING the vowel in three contexts that historically had lowered, lax vowels in Philadelphia: before velar nasals (e.g. *hang*, *bang*), before intervocalic nasals (e.g., *hammer*, *manner*), and auxiliaries (*can*). That's a kind of linguistic reorganization which doesn't correspond to the pattern that I found in New York City speakers in careful styles, where people's avoidance of local characteristics in formal speech was disorganized and apparently random. So I was trying to think, is this recession from local characteristics in Philadelphia a movement away from the most salient characteristics of the local dialect? Or did speakers actually identify this way of speaking with New York? If New York is doing something similar, are people actually identifying the Philadelphia form with the New York form? I don't think so.

GG: In NYC I think that the extreme linguistic diversity of the city is a factor in the loss of traditional features of the dialect. The fact that roughly half of the city's population are not native speakers of English means that you're hearing a lot of different accents and influences, and it may be tough to identify out of all that what the local features are. How would you acquire the detailed complications of the allophonic split of /æ/? Now in Philly, there aren't too many people who speak other languages, but there's a very large African American population (a majority of the city's population). So could it be that a complicated system is just hard to learn when there

are many people around using different systems, and the nasal system is spreading just because it's easier to learn?

WL: Well, I have a research proposal pending at the National Science Foundation where we're going to try to study the influence of higher education on local phonology, which would get at some of the issues you raise. The idea is to compare nationally oriented universities, versus locally oriented universities, versus community colleges, to see if the shift to the nasal system differs according to the dialect composition of the people at the institution. Now at the high school in Philadelphia where my granddaughter just graduated, Masterman, kids come to study from all over the city. Students who come to Penn from there show a clean nasal system for short-a words. So what I want to figure out is exactly when that re-organization happens. Is it in high school or when the kids enter from grade school to middle school?

Students who come to Penn from Roman Catholic High School, which also draws students from a wide area, also tend towards a nasal system but it's not quite as clean as Masterman. So the fact that it's happening in New York also, could be explained by short-a being perceived as the most salient feature of the local dialect, and it's being corrected towards a sort of default system. But the fact that this involves raising the vowel in several contexts, contrary to the general direction of the correction, is amazing.

The koiné that I was suggesting for Western New York State, which became the basis of the Inland North dialect, where /æ/ raises in EVERY context, is not the koiné that is winning out elsewhere. It could be that it's a national orientation that's driving it, that it has become perceived as a national symbol to have a tense vowel in *bank* and *banana*, just like the fronted nucleus of the phoneme /u/, which is now found nationally.

GG: Yes, as a native speaker of Philadelphia English, I know in my bones that *planet* and *planning* have different vowels, and when I hear people say *planet* with a tense vowel, I think, 'what language are these people speaking?' And auxiliary *can*, I have trouble parsing it when I hear somebody say it with a tense vowel. I hear someone say "I **can**!" and I think, what, 'you can tomatoes?' [This word has historically had a lax vowel in Philadelphia and New York, making a minimal pair with the word *can* meaning *lata* in Portuguese.]

WL: Well when I grew up in New Jersey, where that distinction didn't exist, that sentence was commonly followed up by the question, 'Did you say 'c-a-n' or 'c-a-n-t'?', because with deletion of the final /t/, you couldn't tell the difference [whereas in New York and Philadelphia, the negative *can* has a tense vowel, and the positive *can* has a lax vowel].

The other thing that has happened in Philadelphia that has turned around my way of thinking is the relation

of structural to functional factors. The Northern Cities shift was highly functional, and maintains maximal dispersion of the vowels in the vowel space, but all these changes that are happening in Philadelphia defy maximal dispersion, and make different vowel phonemes acoustically more similar: /ey/ is getting closer to /iy/, /ow/ is getting closer to /aw/. There isn't anything that's happening in Philadelphia English that can be explained on functional grounds. It has to be socially motivated. So I'm back on the issue of the social motivation of sound change, which may be primary in these cases.

GG: So back to the big picture, is that an important future direction for sociolinguistics?

WL: I think so, more subtle and more precise matched guises tests are called for, to help to understand people's social perceptions of the variables and the changes, and more studies of population movements in relation to linguistic changes. The other thing we found in the Northern Cities pattern was that the communication patterns between cities run right across dialect boundaries. There's a lot of communication for example between Buffalo, Rochester, and Syracuse [in northern New York, sharing dialect features with the Great Lakes region of the US] and New York City [which has a distinctive dialect], but there's no linguistic communication, in the form of the spread of linguistic features. And Chicago is connected with Indianapolis and Cincinnati, and Columbus [which have completely different dialect features]. So these lines of communication run right across the dialect borders that we see today. When I did my earlier studies [of communication patterns in Pennsylvania] I found that lines of communication matched dialect boundaries fairly closely, with minimal communication across dialect borders. But in this expanse of the Great Lakes area, it doesn't look that way.

GG: Well there are also cultural patterns that define a dialect region.

WL: Yes, I did this study using telephone advertisements for local food items that showed that the Philadelphia region is well defined by cultural patterns, specific foods like the hoagie [a type of sandwich] (Labov, 2003). It showed that hoagies spread from Philadelphia to Pittsburgh. For example, the first advertisement in Pittsburgh offering hoagies for sale appeared in 1962, which was seven years after advertising for hoagies in Philadelphia was completely generalized, and then every year after that the number of places advertising hoagies in Pittsburgh increased. But the initiator for this word in Pittsburgh was Village Pizza, which I found was also selling ovens to other restaurants. So presumably the knowledge of the food item was being spread by the salesmen who were supplying restaurant equipment. This bears on the mechanism of the cascade model for the spread of

change [from larger centers to smaller ones], following supply chains and communication networks. But that model doesn't apply in many cases. The Northern Cities shift doesn't show any effect of city size within the Great Lakes region. If you take the country as a whole, there is an effect of city size, because so many big cities are located in that region. But within there's no evidence of the local spread of this change along the cascade model lines.

GG: So to wind this up, what future directions do you see in your own work?

WL: We're definitely going to pursue this question of the spread of the nasal system for short-a, by looking at universities and colleges that draw from different regions or are mainly local. My greatest interest is in solving the problem of how uniform variation comes about. So the notion of orderly heterogeneity that Weinreich *et al.* (1968) first developed, is what has been reinforced as a community pattern. I've just published a paper on 'What is to be learned?' (Labov, 2012), arguing that the nature of human language is such that the drive towards uniformity is overpowering compared to the drive towards differentiation. The effect of local identity as a driving force of linguistic behavior, that turned up in my work on Martha's Vineyard [an island in Massachusetts], and has been reinforced in so many places, is now seen as a considerable overstatement. The drive for local identity doesn't explain why people are doing the same kind of linguistic innovations in such broad regions, like the ones we have been talking about.

GG: I often think about the other side of that question, when people don't conform to local identities, or have multiple different identities belonging to different communities. Even in Martha's Vineyard you had tensions between local people and summer vacationers, between the Portuguese and the Indians and the Yankees. So this kind of tension and multiplicity of competing identities is probably a factor in all human communities, and could be implicated in both uniformity and differentiation.

WL: Right, so the argument is that the INDIVIDUAL is not the appropriate unit of social analysis, rather, it's the community that is the point of reference and analysis. The individual is best understood as the intersection of all the different communities and social variables that they are involved in and groups that they deal with. And that presents a challenge because we know that people are involved in so many intersecting organizations: flower raising clubs, dog societies, church organizations, and so on. We have never in interviewing people even touched on all the different groups that they participate in. There isn't even any way to enumerate all the possible social influences and affiliations. But we should make an effort to find out more about the whole range of groups people belong to. But if the influence of

any one of these groups is sufficient to impact a person's linguistic behavior, that's important to find.

So in our research, we should make a detailed effort to find out all the organizations or gatherings or networks that a person tends to participate in, perhaps in the course of a month. And that might be the best way of solving the problem of how connections are maintained across the city, but we'd have to ask the question, are any of these connections important enough to change the linguistic output of the individual? Do they change their behavior in that group, when they're at the flower club? The model that we've pursued doesn't take us deep enough into that kind of information to be able to find those things out. But I think that we could develop methods that make it possible. Ask people what groups they belong to and ask if we could follow along with them and meet some of the other people there.

GG: My paradigm New Yorker was an undergraduate who had an assignment in one of my courses to do a neighborhood study. This young man was a Filipino-American, and he spoke Tagalog as well as English. But the neighborhood he went out to study was Brighton Beach, which has a large Russian immigrant population. So he comes back with a report on the neighborhood that reveals that they conducted some of the interviews in Russian. And I asked him, 'who did the Russian interviews' and he said, "I did". Turns out he studied Russian in high school and college, and was good enough to do interviewing in it! So if you heard him speaking English, you'd probably just think he was a middle class New Yorker, but he also functions as a member of the Filipino community, and has good enough Russian to converse with Russians! I was thinking, this could only happen in New York.

WL: Well the thing about NY is that there aren't many places where you can be surrounded only by native New Yorkers, without some immigrant population. And yet, every time you hear a working class New Yorker on the radio or television, he or she has a solid NYC accent. So how do they maintain it? That's pretty mysterious!

GG: Thank you so much for your willingness to do this.

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Submetida: 01/04/2013
Aceita: 02/04/2013

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