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Interview with William Labov

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This interview was conducted by Matthew J. Gordon on July 19, 2006. The text of the interview presented here has been edited somewhat from the original verbatim transcript of the conversation.

MG: *I'd like to begin by asking about a familiar linguistic variable. Do you pronounce your last name [ləbov] or [ləbav]?*

WL: [ləbov]

MG: *For a student who is new to the field of sociolinguistics and who wants to understand William Labov's contribution to this field, what would you recommend as the most important works of yours to be read?*

WL: Well, *Sociolinguistic Patterns* of 1972 is still current and particularly the chapter on the study of language in its social context. Then there are the volumes on *Principles of Linguistic Change*; two have appeared so far [1994, 2001], and I hope to finish the third this year. And now we're just about to put out a second edition of *The Social Stratification of English in New York City* [1966]. That'll be out this year with Cambridge.

MG: *What's going into that second edition?*

WL: There's quite a lot of comment and additions from me. At each point, Labov 2006 is looking over the shoulder of Labov 1966 and commenting on what's happened since and what the significance of this and that was, and then there's a final chapter which brings up to date the study of the speech community. It reviews about thirty-seven different studies that followed from this book.

MG: *That sounds wonderful. What are some of the other works that you consider essential readings for students of sociolinguistics?*

WL: I think of the work of Peter Trudgill, not only his work on Norwich but his general writing on dialects in contact and the creation of new dialects, along with the studies of new communities by Paul Kerswill. There's the work of Tony Kroch on historical syntax; Greg Guy's analysis of variation in the group and the individual; Shana Poplack's work on variation in Spanish and on borrowing and integration of loanwords; the work of Penny Eckert in the Detroit high schools; the work of Walt Wolfram over the span of years, not only the early work on African American English and Appalachian English

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but the current studies of the Outer Banks and other places in North Carolina. For work on African American English, I think immediately of John Rickford and John Baugh, as well as Poplack's studies of expatriate communities. Then I think of Gillian Sankoff's overview of the field, particularly the changes in the use of language across the life span.

MG: *I guess the list could really just go on forever.*

WL: The field has grown. It's bound to happen; it grows underneath you, and you find it hard to keep up with all the good work being done.

MG: *In your essay "How I got into linguistics, and what I got out of it," you write that your entry to linguistics was motivated in part by a desire to bring some empiricism to the field. Could you describe the state of linguistics as you saw it when you were in graduate school in the early 1960s?*

WL: Well, what I actually said was that I would develop an empirical linguistics based on what people actually say. The term *empiricism* to some people implies a skepticism about abstractions and a hard-nosed approach to data, which is not at all the approach that I've taken. The idea is that language is an abstract object, and it has to be treated with abstractions. So the question is, on what database do you form your conclusions? How do you know when you're right and how do you know when you're wrong? I first saw this field of linguistics as an assemblage of young people with all kinds of exciting ideas arguing vigorously with one another, but they didn't quite have an idea of how to prove whether they were right or wrong. There were people behaving as if there was no need for double-blind experiments, as you have in medicine, and that you could produce the data and theory from your head at the very same time. So I thought that it was possible to move this field into a more scientific basis by grounding it on the use of language in everyday life.

MG: *It's now forty-plus years later. Obviously, your own work has introduced that kind of responsibility to the data, to empirically collected data, but have attitudes and methods in other areas of linguistics changed in this regard?*

WL: There's no question about it that quantitative methods and the principles of accountability have appeared in many areas of linguistics, not necessarily spreading from sociolinguistics, sometimes in parallel developments. Sociophonetics is now a growing field. Studies of the acquisition of language have become increasingly quantitative and accountable to the data. Probabilistic theories of grammar and of learning are appearing on all sides, and corpus linguistics, with no sociolinguistic sample, but simply collections of data, has still been found very useful for linguistic investigation. So I think that the movement of sociolinguistics into quantitative work has been paralleled or has helped move many other areas of the field in this direction.

MG: *But I assume that in the generative tradition, the school following Chomsky, there has not been much of that kind of movement there, has there? Would you*

consider something like Optimality Theory a major shift in formalist attitudes toward variation?

WL: Well, Optimality Theory is quite attractive to us because it was a way of dealing with variation between languages, but there has not been any consensus on the use of Optimality Theory for variation within a language. The work of Hayes and Anttila and a number of other people has pointed in that direction, but you don't see people actually beginning OT analyses with studies of everyday speech. In many areas of generative syntax, quantification of everyday speech may not be appropriate—the data are not frequent enough. It's not as if every aspect of our field is open to quantification. Your concepts have to become clear and solid and countable. There are areas, not only abstract arenas of grammar but areas of discourse analysis, where the attempts at quantification may be quite premature.

MG: *Your mentor at Columbia was Uriel Weinreich who died in 1967 at the age of thirty-nine. What are some of the main strains of influence Weinreich had on your thinking about language and the practice of linguistics?*

WL: That's hard to assess because Weinreich was like an influence all around me. In our sessions dealing with my dissertation, he rarely intervened in a direct way, but later on, I found that his influence was quite profound. I once went back to something I wrote before I entered the field called "Essays in Experimental Linguistics" because it seemed those were the ideas I had when I started, and yet I looked over some of Uriel's papers after he died, and I found that he had anticipated many of the things that I was saying. He had projected a study of New York City as a center for multilingual activity, as a city in which many different languages are spoken, a project that has not really ever been realized except in the area of Spanish, most recently by [Ricardo] Otheguy and [Ana Celia] Zentella. If Weinreich had lived, I think two things would have happened. Studies of languages in contact would be pursued much more vigorously as a part of sociolinguistics. And, most important, dialect geography would have advanced much more strongly in the past forty years. Weinreich wrote four papers in the early stages of the *Language and Culture Atlas of Ashkenazic Jewry*, when it was first being planned, and those four papers are still seen as responses to profound problems; they raised the intellectual standards of dialectology. Since Weinreich died, his *Atlas* has moved forward, but no analytical papers of that sort have appeared. So the effort to bring dialect geography into the forefront of linguistic theory would have certainly benefited by Weinreich's presence.

MG: *Your studies of Martha's Vineyard and New York City, which were the bases of your master's thesis and your PhD dissertation, respectively, are widely and justifiably acknowledged to have established a new research paradigm*

which, in the U.S. and U.K. at least, has become virtually synonymous with “sociolinguistics.” When you began these early studies, did you have a sense that your work was innovative or even revolutionary? When did you come to realize the tremendous impact you were having on the field?

WL: I’m afraid that my personal tendency is to always want to do something different than what’s being done around me, and I expected that there would be a long and very romantic struggle against the established forces before the work would be recognized. That didn’t happen. When I gave this paper on Martha’s Vineyard, it was to address the whole LSA [Linguistic Society of America]—we had a single meeting at the time; there were no parallel sessions—and it was very well received. Bill Moulton in particular got up and said, “When is this paper going to be published?” I found that there were many people who were ready for this approach, not only the quantitative approach but were ready to take social contexts into effect. That doesn’t mean that it suddenly became the mainstream of linguistics, far from it. The approach that we follow in NAWV [the New Ways of Analyzing Variation conference] is still only a part and not at all the dominant part of linguistic studies.

MG: *Was there a point after your work had started to be circulated where you realized that you really were founding a new field, where you realized “I really have shaken things up here”?*

WL: No, I think that I saw from the beginning that this would be a different way of doing linguistics. In two ways: It would be based upon fieldwork and everyday life, getting as close as possible to the way people talked when they weren’t being observed; and second, using quantitative methods to deal with that kind of variation. I did see this as a new approach, but I never really wanted to put a label to it. I experimented with “secular linguistics” for a while, but that was never serious. The argument I made that there shouldn’t be a separate label for sociolinguistics was not too successful either, because it turns out that it’s useful to approach the field through a subfield; most linguists want to have some form of sociolinguistics taught in their department. But today, it seems the actual field we’re talking about is best called the study of variation and change. Sociolinguistics is a large and unformed area with many different ways of approaching the subject that aren’t necessarily linguistic, whereas the study of variation and change describes pretty well the enterprise we’re engaged in.

MG: *When we look at your work in New York City and in Martha’s Vineyard, much of the innovation was methodological. The whole idea of the sociolinguistic interview was, I think, your own creation more or less. Of course, people had done interviews before but nothing like that. So do you have any recollections about where these methodological ideas came from? Were there models in other fields at the time?*

WL: There's no question that the sociolinguistic interview as we practice it today comes out of dialect geography and dialectology. As I listen to the early interviews in Martha's Vineyard, I find a lot of emphasis upon individual words and asking people direct questions about language which came from dialect geography. That's true of the New York City study too. A lot of time was wasted asking people about crullers and pot cheese and other local terms. They have their uses, but the central theme of the interview as it developed was the effort to get people involved in important topics that influence their lives and are of great emotional concern to them, and that's been the central strategy, though not always recognized. When people talk about "the standard sociolinguistic interview," I'm not quite sure if that's what they mean. The work I did on narrative with Joshua Waletzky has become quite widely used in the field; it all came from the effort to approach the vernacular in the sociolinguistic interview. And that depended upon an understanding of what's important to people, what their interests are, and what creates the flow of speech. I made one effort to put this in writing in the article on the field methods of the project on linguistic change and variation [1984], but a general treatment of what the sociolinguistic interview is all about is something that has yet to be done. We teach this in our course on the Study of the Speech Community, and that's the main theme: how we go about making language flow. The importance of the individual interview has been reinforced over the years, because even though group sessions and participant observation get closer to the vernacular, the only way to study a population is to have a controlled and comparable data basis. The first interview gives us a way of comparing people across the entire social spectrum. We can never do that with group sessions, no matter how much we'd like to. So the individual interview and the ways of approaching the vernacular in it are still the central theme of our work . . .

The interview has really got two aims: one is to get language flowing as freely as possible; the other is to control what people are saying. So depending upon what we identify as the linguistic variables of interest, we want to intervene with all kinds of formal devices, not only minimal pairs and word lists but subjective reaction tests, family background tests, self-report tests, and other field experiments that allow us to control what people are saying. Even in spontaneous speech, there is an art to get people to talk in a way that will concentrate the linguistic variables we're interested in, and as you get into syntactic constructions, that becomes the major problem. The great success in this area was Lavandera's [1975] work on *si*-clauses in Buenos Aires: she got 1,587 tokens in spontaneous speech.

MG: *Are there any stories from your fieldwork that you still find yourself sharing with students today before they head out to conduct their first interviews?*

- WL: Certainly, a great deal of lore and knowledge has accumulated over the years about how people deal with it. I'm not necessarily the world's greatest interviewer; there are all kinds of other good fieldworkers that have worked with me. I remember when we were working on how to ask questions about astrology, which is quite important to most people. You might be tempted to say to somebody, "Do you believe in astrology?"—a typical outsider's question. Arvilla Payne was talking to a husband and wife in King of Prussia. She said, "Do you believe in the planets and stuff?" This started an argument between them that lasted for about twenty minutes. The wife was a believer and the husband was a skeptic. The wife said, "Well, how come it turns out that more children are born when the moon is full?" and he began disputing that question and said, "What do you think the moon reaches down and makes your belly move back and forth?" She said, "Honey, your belly moves more than you'll ever know!" He gave up on the argument and said, "Let's get back to Arvilla's interview." There's an art to asking questions: it's the ability to move people by formulating what worries them in the terms they use themselves. That's the art we're talking about.
- MG: *Is it fair to say that fieldwork is a trial and error kind of method, that it really takes practice, and that the first couple of interviews a fieldworker conducts are rarely going to be totally successful?*
- WL: Well, it varies. Some people will talk a blue streak no matter what you say, and others will give you one-word answers no matter what you do. But you can always improve your batting average by using the feedback principle. When you first approach a community, you don't know that much about it, you're an outsider, and you ask an outsider's questions. As you get to know more and more about it, the questions you ask go right to the heart of the matter, and you try, just as in any anthropological study, to ask questions in terms that are meaningful to the people you're dealing with. In Harlem, we were concerned with how people played the numbers. An outsider's question is, "Do you play the numbers?" Well, everybody plays the numbers. After a while, you stop asking that and you say, "How do you get a number to play?" Someone tells you that they use a dream book to help interpret their dreams. So for the next person, you start with, "Do you use a dream book?" or "Which dream book do you think is the best one?" and as you get to know more about it you find yourself starting off with, "What do you think of the Red devil dream book?" That's an insider's question. So you feed back what people tell you to ask about things that they're concerned with.
- MG: *Much of your work has been concerned with the study of language change. What drew you to this area of inquiry? Why is the study of change valuable for our understanding of language?*
- WL: In Martha's Vineyard, that was the first topic: What is responsible for this regressive movement of [ay] to [əy]? It seems to me that change is the most

challenging aspect of language because we understand that language is primarily an instrument of communication, and change interferes with communication. We've confirmed that with many experiments over the years [e.g., Labov and Ash 1997]. So that raises the question as to what is the basis for language diversity, or what makes language change. It touches on the problem of how rational people are, because if we try to explain human behavior in terms of our rational goals, we come up against the most serious problems. What I call the Darwinian Paradox is that language evolution is parallel to biological evolution in dozens of ways except for the crucial question of natural selection and adaptation. The driving force behind change is still an open question and quite challenging. Now, if you think of the goal of most linguistics today, it's to find out the general properties of the language faculty. There's no question that the search for universal grammar is very important. How much success we've had is an open question, but certainly a central program in phonetics, phonology, morphology, syntax, is to find out what the general capacity of the human being is. That all concerns the part of language that doesn't change, that's been there from the beginning. We've been involved in a complementary study of everything that does and can change. So it's a question of how large and important those two parts of language are. How much of language is open to change and how deeply do languages differ from each other? I think that sociolinguistics follows the principle that Jespersen laid out that to understand something, you have to understand how it came to be. The other question is: What is the essence of this object, its unchanging essence? These are two different approaches to the subject. We're following the historical line. The other thing we might bear in mind is the relationship of our studies to reality. There are certain branches of linguistics that I call "materialist." They're interested in what actually happens, not in what might happen, and they're interested in making as close contact as possible with what actually happened. Those branches are historical linguistics, dialectology, and sociolinguistics, and that is the common theme that unifies these enterprises as materialist studies of the real world.

MG: *You mentioned general principles, and that's something that distinguishes much of your work on language change. You often work from particular data toward the construction of general principles to describe broader patterns. I'm thinking of, for example, your general principles of chain shifting. You mentioned universal grammar; which leads me to wonder whether you see your work on language change as a counterpart to the search for universal grammar that's pursued in other areas of linguistics?*

WL: In both cases, it's an effort to get an understanding of the largest principles you can find. But principles of chain shifting raise the further questions of how do we account for and explain them, and so there's almost an unlimited regress. Universal grammar as an object is of course of central importance,

and yet in some ways of thinking, it's an inexplicable development that happened suddenly in time projecting backwards to a time that we can't really study. So that search is bound to have greater difficulties in finding a solid basis in empirical evidence. On the other hand, if you look at that branch that we're so close to, phonetics, which is rooted in the physiology of human beings, there we can find the general principles in a much more direct way because we all have the same speaking apparatus, and when we go out and study language change, we're always using everything we can find out about the physiology of speech.

MG: *One of the consequences of postulating general principles is that you really put yourself on the line and challenge other researchers to identify counterexamples. Has this ever been much of a concern for you? Are there general principles that you've formulated that later were found not to be so general?*

WL: I've never been interested in universal principles—that is, 100 percent principles that can't be violated. When we study language and social dialects, it's hard to say that there's anything that people cannot or never will do. Given the right social circumstances, they'll do it. The perfect example is the irreversibility of mergers. That principle was stated in the strong form as "Garde's Principle"—that merger cannot be reversed by linguistic means [see Labov 1994]—and yet Baranowski [2006] has recently found that in Charleston [South Carolina] the merger of *fear* and *fair*, *beer* and *bear* has really been accomplished—it seems to have been quite solid in traditional dialect—and now it's been effectively reversed. There are a number of other examples that I've talked about around the world where we'd say that under certain social circumstances, with enough social pressure, it is possible to reverse a merger. That doesn't mean that the principle has changed radically because it was not based originally on the idea that it's impossible for people to do it; it's just that given the arbitrary character of the linguistic sign, it is extremely difficult to reverse a collapse of two classes in the vocabulary.

MG: *This reminds me that one of the things that your work has done is provide an agenda of important issues to pursue. You put forward a general principle, and people can see to what extent it applies in the communities they study and in the data they have.*

WL: The early studies of social stratification were somehow misunderstood, especially by people from very different communities, as arguing that in all countries in the world, we will find social stratification according to occupation, education, and income. The sociolinguistic principles that we find are much more general. They say that for a certain stage of linguistic variation, style shifting and social stratification will operate on the same variables. That's what produces these extraordinarily regular patterns that we see. Now, that doesn't mean that for every single linguistic variable it has to happen. It doesn't mean that the particular type of social stratification will be the same in every society. The

principles of linguistic change have been easier to formulate than the principles of social stratification in everyday life, and that's to be expected because we still are struggling with the problem of finding out how language satisfies the need for communication in everyday life and also distinguishes people by bringing out their differences rather than their similarities.

MG: *I wonder if I might ask a more technical question about chain shifting, particularly about whether it's possible to directly prove their existence in the sense of identifying the causality between putatively related stages in a shift.*

WL: This is a topic that I'm right in the middle of. A lot of people have been wondering about causality in the Southern Shift and the Northern Cities Shift. Causality is a very difficult concept; it doesn't really have any scientific basis because all we see is conjunction, just covariation and correlations to indicate that two processes are tightly linked. Now, any given chain shift can often be looked at in two ways. One is that the movement of A causes B, and the other is that A and B move jointly as a generalization of the same principle. So the chain shift connections that are most important are where you can't do that second formulation and say they're moving together jointly because they're two very different types of change. In the Southern Shift, the triggering event was the monophthongization of /ay/, and that was followed by the lowering of /ey/ towards the position of /ay/. Well, those are such different operations that there's no way they can be subsumed into a single operation. So the causal connection between them is much clearer than the causal connection between the lowering of /iy/ and the lowering of /ey/. I'm writing a chapter in this current third volume [of *Principles of Linguistic Change*] called "Triggering Events," and the question is, Can there be a triggering event in a linguistic series? It's a difficult question because it seems that every linguistic state is preceded by another linguistic state. Yet there will be linguistic events that are so different from anything that follows that they can be seen as the initiating principle and the cause of what follows. The two ways in which causality can be detected are (1) that movement A is of a completely different character than movement B but creates the conditions under which B will happen, and (2) that an event can be a series of historical accidents that almost come together by chance and thus create a whole new situation that couldn't have been predicted. So those are the two kinds of triggering effects that I've been trying to identify to reinforce the notion that there is a necessary connection between two events.

MG: *I was wondering about this issue in part because I noticed in your Atlas of North American English, you examined the allophonic conditioning of the two stages of the Southern Shift you mentioned: the monophthongization of /ay/ and the lowering of /ey/, and you didn't find any correlation there. That is, it's not the case that the same phonological contexts favor both of these changes as we might expect if they were operating in a chain shift. So should*

we expect to find that kind of connection at the allophonic level or even, maybe, at the level of the individual speaker? Do you find that the heaviest monophthongizers are also the most progressed in their lowering of /ey/?

WL: That topic is a central issue in this chapter on triggering events. In fact, it's a whole other subject itself: Is there allophonic chain shifting? I have a paper on the "binding principle in phonology" [2006] that looks at that question. The current explanations of chain shifting should predict that if one allophone moves far enough away from other allophones, there should be chain shifting of neighboring phonemes for just that allophone. It turns out that in the Northern Cities Shift and the Southern Shift that does not happen. Just as you said, you don't get lowering of /ey/ before voiceless consonants more strongly than lowering of /ey/ before voiced consonants, even though the monophthongization of /ay/ is so different. Some allophonic chain shifting does exist, however, for those contextual conditions that are radically different, as with chain shifting before /r/, as you know. There are all kinds of chain shifts before /r/ which are not echoed in other environments, so much so that it's almost impossible to say which vowel before /r/ corresponds to which vowel not before /r/. So there will be points where the binding force that holds the phoneme together is ripped apart by coarticulatory pressures, but that only happens in certain special environments, particularly before liquids where the binding force is not powerful enough to hold the phoneme together.

MG: *And what about at the level of the individual? Should an individual speaker model the causality of a chain shift?*

WL: Not necessarily. If you look at the Northern Cities Shift data in the *Atlas*, you'll find that there's a lot of individual variation in whether five or six of these elements all move together. If you look at any given community, we find that the pattern is quite regular. So, there is slippage at the individual level. Particularly, the lowering of long open-*o* [i.e., the vowel of *caught*], which is quite variable. The map that shows speakers with all elements of the Northern Cities Shift displays about two-thirds of the people in the Inland North, mostly in the bigger cities. So there is enough room for differences. Now, I suppose that as we get more and more studies like your own, as we get deeper into local communities, we'll understand more about the causes of variation, but I think that individual systems are still going to show a certain amount of variation in the sense that all elements of a chain shift will not be represented equally . . .

On the other hand, the individual doesn't exist as a unit in the framework that we're approaching. We study individuals because they give us the data to describe the community, but the individual is not really a linguistic unit. Many of the people in sociolinguistics disagree with me on this point, and they think that reality lies in the individual speaker, and I take the position that's just the reverse. There are no individuals from a linguistic point of view.

MG: *Because language is the property of the community and that's what we're studying?*

- WL: Exactly. It's an objective characteristic of the real world outside of the individual, and what the individual has is the capacity to grasp this pattern, but people of course do not always grasp it in exactly the same way. The pattern is the same, but the individual grasp of it may vary.
- MG: *Another subject that you have devoted much of your research career to is African American English. What was the state of knowledge about this variety when you began your work in Harlem in the late 1960s?*
- WL: This situation is typical of linguistic disagreements. There are two very intelligent and able groups of linguists who are arguing diametrically opposed positions. My own approach has been that linguists are very intelligent people and that if a lot of them believe something, there must be a basis for it. So you had the creolists, people like Bill Stewart and Beryl Bailey, who were arguing that African American English was a creole just like the Caribbean creoles. And then you had dialectologists, like Juanita Williamson, arguing that African Americans spoke just like the white people from their local area. I think that our New York City studies and those that followed have indicated that it's necessary to understand that African American English is a product of the history of African American people who came from Africa to the South. There is a substratum effect—as Walt Wolfram has demonstrated quite clearly—but there was also an alignment towards the surrounding dialects for many linguistic features like the copula and some degree of convergence in the rule systems, as in consonant cluster simplification. So there one can say to each group, you were right. I might also mention that one of the approaches to AAVE [African American Vernacular English] has pretty much disappeared. There were linguists who approached black speakers with the methods of formal morphology or syntax. They would ask, "What's the name for the leader of the United States?" and if the person would answer, "President," they would ask, "OK, so what's the adjective that goes with *president*?" They would write down, "Adjectival form of *president* in AAVE, *presidential*" and so on through the vocabulary. Well, that approach has disappeared, thank God, and people understand that you don't find out about a minority dialect by asking people such questions. Nobody does that anymore.
- MG: *In terms of your initial motivation, is it fair to say that your interest in African American English was motivated as much by social justice concerns as by intellectual curiosity about the linguistic structure of this variety?*
- WL: Yes, the work we did in Harlem was funded by the Office of Education to try to answer the question, is there any relation between the failure to learn to read and the dialect differences we were looking at? And we came up with some answers on that, and a lot of important theoretical conclusions came from the study of African American English. But we didn't improve the reading of the guys we were working with in Harlem in the 1960s. This is a problem that I've returned to. I wrote a paper on objectivity and commitment in linguistic

science [1982] and talked about the debt incurred of the scholar who gets information from the community. We're still trying to repay that debt. So over half of my effort over the last ten years has been devoted to trying to raise the reading levels of African Americans and other minority groups in the inner city. If I do make a contribution to that, it will certainly weigh very heavily on the scales.

MG: *Since you've done a lot of work in the schools, I wonder whether you've seen much of a change in attitudes toward African American English among classroom teachers?*

WL: No, and this is in itself an important sociolinguistic finding. In the early days of the studies of African American English, the thought was that since this dialect is so different, children will learn to read better if they learn to read in their own dialect—the dialect readers. But in one case after another, it turned out that the public reaction to this position was so strong that it defeated and overwhelmed the effort. So even though this might be useful and a good approach in some areas, it doesn't seem to be a practical approach. It's not necessarily all teachers and parents who object, but it doesn't take many to derail an educational program. If someone wants to use African American English in the classroom as a medium instruction, I'm glad to support it, but I'm afraid it might not work out on a large scale. You may succeed in changing the point of view of a given group of people, but in the meantime, ten or twenty thousand others will arrive in the school system with the same attitude towards everyday vernacular language.

The work that Walt Wolfram has been doing in the schools in Baltimore and North Carolina is very important in combating prejudice against dialects. He's taken the position that he's not going to focus on one particular dialect but talk about dialects in general. I think that's bound to have a positive effect. There has been a certain amount of change at the higher levels of instruction, at the university level, but if you get down to local schools, in Philadelphia certainly, we find that the negative reactions to the use of AAVE are pretty constant. There are broad general principles controlling people's reaction to everyday language, and the struggle to change them should not be confused with the struggle to improve the reading and writing of the children you're dealing with. Those are two separate enterprises. I don't want to engage in a quixotic effort to change the attitudes of teachers and parents about something that they believe as fundamentally as anything else in their lives. Rather, we're going to use our knowledge of African American English to improve the teaching of reading.

MG: *Along those same lines, do you think there's been greater progress made in areas such as speech pathology and language testing?*

WL: Yeah, the group at UMass Amherst has produced a volume that will help speech pathologists detect normal behavior for African American children, and it seems to be very helpful. I think in the field of speech pathology, there is a greater

knowledge and understanding of what we'd expect to be normal. Some twenty or thirty years ago, I heard a speech pathologist in a Philadelphia school—95 percent African American—recommend treatment for hearing disability for quite a few children, and every example that he gave had to do with the ability to hear the difference between *pin* and *pen*. I think ignorance of that kind has been receding.

MG: *Let me turn now to talk about Philadelphia. Since coming to Penn in 1971, you've been engaged in a multidimensional study of Philadelphia speech, much of which is reviewed in the second volume of your Principles of Linguistic Change [2001]. This research has documented a number of stable sociolinguistic variables as well as a remarkable set of sound changes. Philadelphia seems to have the most dynamic vowel system of any dialect in the world. Of course, it's probably also true that the Philadelphia vowel system is the most thoroughly studied of any in the world. Is this a coincidence? Is Philadelphia unusual in its sociolinguistic complexity, or have we just not studied comparable speech communities in enough detail to uncover variation on par with Philadelphia?*

WL: Well, I did come to Philadelphia because two-thirds of the vowels seemed to be changing, as opposed to New York where only a third of them were involved in change, and that's been very fruitful for our studies of change. Philadelphia vowels don't show the intricate chain shifting that we observe in the Northern Cities Shift or the Southern Shift. They're moving but not necessarily in such close harmony, and that produces a separate set of questions. What you suggest is right: we know more about the Philadelphia dialect than most others. We've had many restudies over the years. The project on Linguistic Change and Variation in Philadelphia did its work in the '70s, with Arvilla Payne, Anne Bower, Greg Guy, Matt Lennig, Don Hindle, Liz Dayton. Since '73, groups of students in our class on the Study of the Speech Community have entered Philadelphia neighborhoods, studied social and linguistic behavior, and placed their final reports on the shelves. Every sociolinguist who has passed through Penn has left a contribution to this program, and each group entering the city can find a parallel neighborhood that we studied five, ten, fifteen years before and compare it to what they're getting.

Then, we have Jeff Conn's recent restudy of Philadelphia as a whole, which showed where we are after thirty years, and Suzanne Wagner is now in South Philadelphia doing further restudy. So we have considerable time depth, and we have a cabinet with many thousands of recordings of Philadelphians which are available to those who are joining into this enterprise. So yes, we know a great deal about this dialect, but there are quite a few puzzles about Philadelphia. As stable as the system is, we find that it was essentially a southern city and followed the Southern Shift in many ways until 1940. Now the short vowels are moving in the opposite direction, and Philadelphia, as far as the short front

vowels are concerned, has resigned from the South and joined the North. What demographic facts or social forces led to this is still a mystery.

MG: *If you had ended up in another city, say Chicago or San Francisco, and applied your same level of study there, would you have uncovered an equally complex system or is Philadelphia, because of the historical factors that you alluded to, uniquely complicated?*

WL: No, I think that almost any city could have been used as the basis for such kind of study with the exception perhaps of the West, where we find that dialect differences are not so clearly formed. The section that we call the West in our *Atlas* is still a challenge. Because the general characteristic of North American dialectology, as you know, is that we're writing upon a tabula rasa; new sound changes are occurring; whole systems are being formed, but it takes a century for this to reach fruition. In the studies that I'm now doing of the *Atlas* findings, it seems if a community developed its settlement pattern in the middle of the nineteenth century, it's in the middle of the twentieth century that linguistic changes began to move more strongly. So that hundred-year time depth has not yet been achieved in some areas.

MG: *You mentioned the work of your students, so I wanted to note that in addition to your direct contributions to the field, you've also trained a great many students, including several who have gone on to become leading figures in their own right, such as John Baugh, Greg Guy, John Rickford, and Penny Eckert. I'm sure you could add to that list.*

WL: Shana Poplack, of course.

MG: *Yes, of course, right. How would you describe your general approach to mentoring students?*

WL: The course on the Study of the Speech Community has been the central training ground that brings students together in groups. It tells them, "Look, you're not that different from most other people. You're a little bit shy about plunging in and talking to strangers. Here are some of the principles that operate for everybody, and you're going together as a group to do this." And people then get direct experience by entering a community without any introductions, without any help from friends or relatives, making their own contacts, and learning that the world is open to them to a large extent. The effect of group work has been essential for us. Groups from this class form bonds that last over decades. People work together as a group in a synergistic way that goes beyond what any individual can accomplish. Also, activity usually precedes reading. Reading is very useful once you've done some work and can interpret it. There are many universities in the United States where people teach a course in sociolinguistics which is essentially, "Today, we'll do chapter 5" or "Today, we'll read chapter 2 and discuss it in class," as if reading was actually doing linguistics. In most introductory linguistics courses, we know that we

learn only by doing the work, and in sociolinguistics, we learn only by entering the community. Students over and over again have found that's true. They read about style shifting, but it wasn't until they actually went out and put a microphone in front of somebody that they realized that they weren't getting the kind of result they wanted. Personal experience is the essential formative influence. I think that all of the people you mentioned have been through that experience and are capable of transmitting it to their students.

Also, I would say the matter of identifying a dissertation is an interesting problem. Some people come to graduate school with a powerful interest in a particular subject, and they just never lose their interest in that narrow field. That can be very fruitful, but it's very limiting. I think the students who have been most effective are the ones who got some experience in the field and then looked over the general problems and located an area where the most important questions could be answered. Frequently, that took them to different parts of the world. Poplack's many projects show the capacity to locate strategic research sites, which are not driven by her personal history but by the need to find out more about language and language change.

MG: *One of the fields that appears to be closely aligned with sociolinguistics in terms of methods and objects of study is linguistic anthropology. How would you characterize the relationship between these fields?*

WL: I have to be a bit cautious here because I'm not right in the middle of linguistic anthropology, but from what I can see, relations are not good. When I entered the field, there was no barrier or distinction almost between linguistic anthropology and linguistics. So you have many people who are trained in anthropology in our field; people like Tony Kroch and Gillian Sankoff got their degrees in anthropology, and many students trained in sociolinguistics have entered anthropology departments like Niloofar Haeri and Norma Mendoza-Denton. So, what has happened? My impression is that the connection between anthropology and linguistics is not as strong as it was. One reason may be, and I can only speculate on this, the isolation of linguistic theory from the description of languages. For a long time, people who wanted to describe a language found that they were isolated from linguistic theory because the theory was not designed to help them do this job. Furthermore, the search for a language and terminology that could be read in fifty years by students to come was impeded by the instability of linguistic theory. So this is where things stand at the moment. I'm hoping that they will change, and the emphasis upon the study of endangered languages in our own field may help.

MG: *I'd like to talk about dialectology, which we've mentioned before. It is another field with close intellectual connections to sociolinguistics. You have referenced dialectological studies in your work since your Martha's Vineyard project, and your recent Atlas of North American English is clearly modeled*

in part on the tradition of American dialectologists like Hans Kurath. How would you characterize the influence of dialectology on your own work and the field of sociolinguistics in general?

WL: From the very beginning, I've drawn upon Kurath's work and McDavid's work and benefited from it. It'd be easy for sociolinguists to be critical of traditional dialectology and say that NORMs [nonmobile older rural males] weren't necessarily representative of the mainstream of the community or to complain that dialectologists didn't take linguistic theory into account, didn't even ask for minimal pairs. So one could find many limitations in the field, but the strength of American dialectology was always clear to me, and I was careful to point that out, that Kurath's work was the solid base to build on, particularly the distinction between North, Midland, and South that he established. I know some of his own students like Bill Kretschmar have been rather critical and felt that there wasn't a scientific basis for the Kurath maps. But the *Atlas of North America* has found that, for example, the North/Midland line was a deep and important division in American language and society. When Kurath finally produced his book, *Studies in Area Linguistics* [1972], he devoted a chapter at the end to sociolinguistic studies of cities and was very positive. So we avoided the development of a polemic between two groups of people who didn't understand one another, and the recent relationship between the sociolinguistics and dialectology has been quite positive. As I mentioned before, we have this joint material interest in the real world. Today the American Dialect Society is a very healthy and strong institution where sociolinguists have joined with traditional dialectologists in their meetings. At the Linguistic Society [of America's annual meeting], it's very difficult to know how to distribute your time since so many of the important papers are given at ADS.

MG: *I'd like to talk in more detail about The Atlas of North American English, which you coauthored with Sharon Ash and Charles Boberg. You must be extremely relieved and, of course, proud to see this work in print after so many years of research. The geographical scope of this work is unprecedented as it surveys the U.S. and Canada from coast to coast. Also impressive is your decision to provide the raw data from your acoustic analyses in the accompanying CD-ROM. Could you talk about your philosophy in preparing this project for publication and about what you see as the major contributions of this work?*

WL: I don't see it as so many years; it was almost an eye blink in terms of most dialect projects, which can last sixty to a hundred years. Our idea was to do it quickly—because of the rapidity of sound change, that was important—and the telephone survey accomplished that. Well, it wasn't the five years that we first talked about because it came down to maybe seven or eight before all the data was collected. But the general philosophy was that through a telephone

survey we could accomplish the important task of getting all of North America into one view. Up to that time, despite the excellent studies of the eastern United States, only a few regions had produced results dealing with pronunciation, and only one or two had any maps. And studies of states like Colorado weren't reasonable units because language change and boundaries don't stop at state boundaries. So it was important to do the whole country and include Canada as well. That meant giving up a lot of the principles of sociolinguistic work because speed was important, and we were approaching people and talking directly about language, so we don't have a wonderful two-hour session that explores the person's whole life but a shorter interview in which the most exciting thing we talked about is what's happening downtown. That turned out to be pretty exciting in many ways. But the important thing about the *Atlas* was that up to that time, we knew only what was happening in a few big cities. We had some exploratory studies in Rochester, Buffalo; we had representative studies of New York, Philadelphia, Detroit, and Memphis; but the rest of the big cities were more or less shrouded in darkness. When the *Atlas* was finished, the lights were turned on, and we saw to our astonishment that there wasn't a Chicago dialect, there wasn't any Buffalo dialect, but rather there was this vast area of eighty thousand square miles and thirty million people who all have the same vowel system and are all moving in the same direction. We also found that in the South, the local differences were beginning to disappear and monophthongization of /ay/ was solidifying across the South with its consequent effects upon other vowels. So in the *Atlas*, the big view is important. In one direction that sociolinguistics is heading, people say, "Let's get down to the micro level and look at a couple of people talking to each other and see how they're influencing each other." That can have important results only when it's set against a clear view of the larger speech community.

I think of two major contributions of the *Atlas* that regional dialects are getting stronger and more diverse as language change is continuing and that the structural divisions between them are very sharp, with very tight bundling of the isoglosses. All that was unexpected and extraordinary.

MG: *Your comments remind me of Jack Chambers's use of the term dialect topography, which provides an overview of the landscape, which then allows for follow-up research to concentrate on the most profitable areas.*

WL: Right, I think that his Golden Horseshoe study is very effective in that way. It goes back to the principle that came out of the New York City study, which is that the individual is very difficult to understand; even the small group is very hard to understand unless you know something about the community or group they're coming from, because the individual's behavior is determined by the social forces that intersect with their individual lives.

MG: *The way that you've presented the work is really facilitating further research. I'm thinking about the maps in chapter 10 of the Atlas that simply lay out the F1 and F2 measurements for every vowel and then also the raw data you provide on the CD-ROM that comes with the Atlas.*

WL: We did that in the spirit of dialectology, which is to say, "Let's first give people the data without our analysis." But actually I think that chapter 11, which provides the theoretical framework for the dialect identification, will be the most important because it's saying that there have to be principles behind the identification of dialects. That means finding out what sound changes are at the heart of the dialect area and then seeing what the limits of these processes are. So we've done both, and I'm just preparing now a spreadsheet, which we'll make available to everybody—the 134,000 measurements of vowels with all the demographic information they need to utilize that information themselves, and then there's another database which I'm going to get up on our Web site, which is the actual segments themselves, the words themselves, so that people can hear the sounds. Right now if you go to the Web site, you can hear a lot of individual words and up to a minute of running speech for most people. So the drive of the *Atlas* is to make that data available.

There's a bit of a contradiction here because there's been an enormous amount of publicity about the *Atlas*; I've spent a lot of time talking to reporters and participating in talk shows. People have told me all kinds of fascinating and interesting things about the history of certain areas, and I'm benefiting from it. But the *Atlas* is not readable by the average citizen who's so passionately interested in these dialect differences because it's full of measurements and discussion of chain shifts among front vowels, back vowels, rounding and so on, and without a course in linguistics, the book itself is not readable. So there's been a lot discussion of what can be done to make this information available to the general public, and we're working on a number of different approaches. It is a problem because the interest of dialectology to the general public can't be overestimated.

One of the things that we've found, and I've argued from the beginning through my work with Weinreich, is that the linguistic evidence of spatial distributions is, if anything, more powerful than the evidence we draw from the internal distribution of language in the city across social groups. So what the *Atlas* is studying is not variation in the sense of the New York City study but rather the distribution of the framework for variation, the framework which defines communities. A lot of people have talked about speech communities as being defined by norms, and that is something I emphasized in New York. But these norms and the patterns of style shifting are carried out within a set of constitutive rules, a framework which is constant across the community, and lexical distributions that are more or less constant. The *Atlas* is looking at invariance rather than variation, the distribution of those invariant frameworks

that define the speech community. Now we have Baranowski's sociolinguistic study of Charleston with a hundred speakers and we find that, as far as the patterns of interest to the *Atlas* are concerned, one hundred people confirm what we found for four people. That's because we're not studying in the *Atlas* variation across social classes but rather that framework in which social variation takes places.

MG: *It's interesting that you mention that follow-up study because one obvious criticism that one might make is to question how you can get a representative sample of, say, St. Louis with six people.*

WL: Actually, four. I'd answer that if you look at most of our maps, you'll be surprised at unexpected uniformity of red circles or blue circles or whatever they are instead of a pepper and salt distribution. For the average city, the first two persons who answer the phone and say, "Yes I was born and raised in this town" are accepted as representatives of that city. The uniform result is only possible because of the fact that these structural patterns are so compelling upon the regions. We're not studying social variation; we're studying the structural basis for it.

MG: *As you survey the field today, what are some of the most exciting research trends you see?*

WL: In the field of phonetics, we've always felt that there was a tension between articulatory explanations and acoustic measurement, and that the explanations for many of the sound changes that we worked out were found in the study of articulation. The development of ultrasound is a new and exciting possibility where we can actually study the movement of the tongue that lies behind these principles, and even take it into the field. The growth of historical corpuses, the Penn corpus, the Helsinki corpus, and others has given us an opportunity for looking at principles of change over long periods of time. I mentioned the development of spatial arguments and dialectology, which has been an exciting area. And certainly the development of true ethnography, deep and thorough studies of human behavior by participant observers like Eckert, Dayton, and Mendoza-Denton is a new and exciting area, as well as the number of close studies of social networks. I think that the integration of all these studies and areas into a single perspective depends upon the concept of the speech community as an overarching social reality. The notion of a social fact—that language exists in the community exterior to the individual—is our central theme. The way in which this social pattern is grasped by the individual speaker and the way it changes over time is our central problem.

MG: *Are there areas of sociolinguistic research today that you think are underexplored?*

WL: I think that our major problem is to encourage people to do studies of large communities, of the big cities. Much of what we've learned has come from the studies of big cities, most outside of the United States: Norwich, Helsinki,

Copenhagen, Panama City, Rio de Janeiro, Amman, Tehran, Tokyo, and Seoul. We have to encourage people to study the community as a whole so that the micro study of individuals can be understood in broader terms. If you look at the United States, very few cities have been studied with representative samples, perhaps fewer than you'd expect, considering the enormous output of sociolinguistic activity in the past few decades. I hope the *Atlas* will provide the framework to identify the cities that offer the biggest reward for major studies of this kind.

MG: *It's been a pleasure speaking with you. Thank you for your time and your thoughtful responses.*

WL: You've asked just those questions I would have liked to be asked. So, thank you, Matt.

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