

# How I got into linguistics, and what I got out of it

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*The following is an essay I first wrote for a 1987 publication addressed to undergraduates, which contained various answers to the question, "How did you get into your chosen field of work?" I recently revised it to answer the same question that I got in email messages from undergraduates in different parts of the world. -- W.L., October 1, 1997*

When I first went to college (it was in 1944, while the war was still on), I spent a fair amount of time trying to figure out what I wanted to do, and even more time trying to guess if I could do it. Some people say I'm a quick study, but it took me a good fifteen years to work up the answers. If you are in your sophomore year and don't have your own solutions yet, I'd encourage you to reflect that it doesn't always pay to be too fast in replying to important problems. The kids who sit at their desks with their hands in the air often don't know what the question is all about.

"What is success?" That's one of the questions that I asked people in the first linguistic interviews I put together. One man told me that it's figuring out what you want to do, and then getting someone to pay you to do it. Another man said it's making use of everything that ever happened to you. I like both ways of defining it, but I usually look at it another way: if you get to be 70 years old, and you can look back without feeling that you've wasted your time, you've been successful. Reflecting on how I got into the field of linguistics, and what I've been doing since, I seem to have been following all three ideas the same time, so they may turn out to be the same idea after all.

I was born in Rutherford New Jersey, a small town just far enough outside of New York City so that I'm not a New Yorker at all. That has a lot to do with my approach to the English language. I pronounce all my final r's without thinking about it, and I'm perfectly happy with the way my vowels fall out in words like *mad* and *more*. When I was twelve, I moved to Fort Lee, just across the George Washington Bridge from the big city. That's well within the New York city dialect area, where people don't pronounce their r's except when they think about it, and don't like the way they say *mad* and *more*. They also don't like kids with a big mouth from Rutherford, New Jersey, and my high school years were full of conflict (fights, which I usually lost; arguments which I usually won). A lot of these characters were pretty rough, and I grew up believing that most of the local families were on better than speaking terms with the Mafia. But the people you have the most conflict with are often the most important to you--your reference group, as sociologists say--and we were all good friends when we met in later years.

About this time, I must have seen Leslie Howard as Henry Higgins in the film version of *Pymalion*. I remember him leaning against a stone column and writing down every sound coming out of the mouth of Eliza Doolittle. I thought that was amazing: how could he do it? Now I know that he was only writing down a few of the sounds that interested him. It was a lot easier for me twenty years later, when I was doing field work in Battersea Park and Chelsea, London, because I had a tape recorder at my side instead of a pencil in my hand. Henry Higgins was explicitly modelled on Henry Sweet, the great English phonetician, whom I have since come to admire intensely: some of my own findings about the general principles of language change are a modern version of what Sweet suggested in 1888.

I never thought of becoming a linguist in my four years at Harvard, where I majored in English and philosophy and spent most of my time talking. But I remember a conference with my freshman advisor, John Wild, a philosopher with a strong leaning towards the Middle Ages. When he learned that I was taking one course in chemistry (inorganic), he sucked on his pipe, smoothed out his cord trousers, and said, "Just where did you get this *idolatry* of science?"

I've thought about that quite a bit since. Wild was perfectly right. I *did* have an idolatry of science then, and I never lost it since. but how did he know that when I didn't even know it myself?

After I got out of college, I had an idea that I wanted to write, like many other people who don't know what they want to do. I lost several jobs in rapid succession: writing blurbs for Alfred Knopf, writing boiler plate for *Drug Trade News*, writing down what people said for market surveys. But after a few years I wound up in something more practical, using my little knowledge of chemistry in the laboratory of a small company. I was an inkmaker. I specialized in formulating inks for silk-screen printing: on cardboards, on T-shirts, on bottles, on printed circuit boards. I really liked it. I was a good color matcher; I had a feel for how to do research; and I liked the men who made the ink. We ate lunch together, we argued about how long it took to get from New York to Miami, and everything else under the sun. Working with pressmen and millhands and truck drivers every day, I learned that there were a lot of people in the world who know what they were doing, but that salesmen earned most of the money.

I also picked up from my industrial work a firm belief in the existence of the real world. It often happens in work of this type that you coat a panel with an enamel and expose it southward to the sun. If you come back in six months and find the coating cracked and peeling, you know that you were wrong six months ago. You may not know *why* you were wrong, but you can be sure that some part of the real world has defeated your real effort to protect a metal surface. It also may happen that you find yourself standing between the rollers of a giant four-color press, with a vice-president telling you that if the ink does not print in fifteen minutes, he stands to lose a two million dollar account and you stand to lose a customer. If you can make the presses roll, you are right, and if you can't, you are wrong.

In 1961, I left the world of printing ink and re-entered the world of the university. I had found that small business was interesting and entertaining, but also agonizing and restricting. There are economic constraints that keep you from using all of your knowledge, and making the best ink you can; if you want to have an advantage over the competition, you can't very well generalize your knowledge and publish it.

When I decided to return to the university, I had in mind some research on the English language. From what I learned about the small, new field of linguistics, it seemed to be an exciting one, consisting mostly of young people with strong opinions who spent most of their time arguing with each other. When I found that they were also drawing most of their data out of their heads, I thought that I could do better. I would make good capital of the resources I had gained in industry. I would develop an empirical linguistics, based on what people actually say, and tested by the experimental techniques of the laboratory. I didn't realize it then, but I was also bringing to linguistics two other resources that were missing in the university: the belief that working class people have a lot to say, and that there is such a thing as being right or being wrong.

I found the university an attractive, exciting and receptive environment. But I was lucky: the head of the Columbia department of linguistics was a man of my own age named Uriel Weinreich. He was one of a new generation of secular Jews, a native Yiddish speaker from Vilna who had escaped the Russian seizure of Lithuania because his thirteenth birthday present was a trip to the International Linguistics conference in Copenhagen (the Vilna high school was a precocious environment!). Weinreich was the perfect academic:

passionately interested in the ideas of others, brimming over with intellectual honesty, vigor and originality. He protected me from every academic evil. When I visited other universities as a graduate student, the name of Weinreich always brought a special look of respect and awe. He died suddenly, of cancer, at the age of 39. Going through his papers in later years, I found that he had written up projects for research that anticipated most of the things I wanted to do. So to this day, I do not know how many of my ideas I brought to linguistics, and how many I got from Weinreich. I would like to think that my students are as lucky as I was, but I know better than that.

There were (and still are) two major branches of linguistics. One deals with the description of languages as they are now; the other deals with their history, how they came to be. On both sides, I saw that there were some big problems to be solved if linguistics were to make contact with what people said. Linguists wanted to describe languages, like English or French, but their methods only brought them in contact with a few individuals, mostly highly educated. Whenever someone raised a question about the data, they would answer, "I'm talking about *my* dialect." The current theories held that every individual had a different system, and they weren't making much progress in describing the English language and the speech community that owned it. Even more mysterious was the problem of accounting for language change. If language is a system for transmitting information from one person to another, it would work best if it stayed put. How do people manage to understand each other if the language keeps changing under their feet?

My first research was on the little island of Martha's Vineyard off Cape Cod. My friend Murray Lerner, the film maker, invited me up there. There I noticed a peculiar way of pronouncing the words *right*, *ice*, *sight*, with the vowel in the middle of the mouth, that was stronger among young people, but varied a great deal by occupation, by island locale, or by the speaker's background--Yankee, Portuguese, or Indian. I interviewed people all over the Vineyard, and among them I found some of the finest users of the English language I had ever known.

As I finally figured out, the Martha's Vineyard sound change was serving as a symbolic claim to local rights and privileges, and the more someone tried to exercise that claim, the stronger was the change. This became my M.A. essay, and I gave it as a paper before the Linguistic Society of America. In those days, there was only a single session, and you practically addressed the entire profession when you advanced to the podium. I had imagined a long and bitter struggle for my ideas, where I would push the social conditioning of language against hopeless odds, and finally win belated recognition as my hair was turning gray. But my romantic imagination was cut short. They ate it up!

My dissertation was a survey of the class differences in the dialect of New York City, where I introduced a batch of new techniques of interviewing, quantitative techniques for measuring change, and field experiments to pin down just which sounds triggered the linguistic self-hatred of New Yorkers. Since then, these techniques have been used to study several hundred other cities throughout the world. We've introduced the use of acoustic phonetics into the study of everyday language, and linguistics has begun to make the slow move from a qualitative to a quantitative science. The variation across individuals, and across time, that seemed so chaotic and so puzzling, was beginning to take on a systematic shape that could be described mathematically.

While I was teaching at Columbia, I proposed a research project to the Office of Education, to find out if the dialect spoken by black children in Harlem had anything to do with the failure of the schools to teach them to read.. This became one of the most fascinating intellectual and social adventures of my life. Though we thought we understood what the speakers of this dialect were saying, we had no understanding of the system they used to say it with. Along with black and white colleagues Paul Cohen, Clarence Robins and John

Lewis, I began a detailed study of all the social groups in South Central Harlem, with a combination of participant observation and mathematical analysis that revealed for the first time the internal variation that governs linguistic behavior. We came to the conclusion that there were big differences between black and white speech patterns, but that the main cause of reading failure was the symbolic devaluation of African American Vernacular English that was a part of the institutionalized racism of our society, and predicted educational failure for those who used it. I wrote a paper called "The Logic of Nonstandard English," which defended the home language of the black community as perfectly adequate for logical thought and learning. This has been reprinted hundreds of times, and the speech of black youth who I quoted have been reprinted many times more. But as much as we have gained ground for this theoretical position, the sad fact is that the Cobras and the Jets of the 1960s never benefited from our work; ten years later we learned that many of them were shot up, in prison or dead. We have not yet learned how to bring our knowledge to the teaching of reading. The enormous differential between minority and mainstream achievement in school continues to expand, year by year, and we have not yet repaid our debt to the youth who helped us on our way.

In 1970, I moved from Columbia to Penn, mostly because the Philadelphia dialect offers an ideal laboratory for the study of changes in sounds: two thirds of the Philadelphia vowels are involved in a complex game of musical chairs. Here at Penn, I was joined by my colleague Gillian Sankoff, who had developed these methods even further in her study of the French of Montreal, and broken new ground in the study of Tok Pisin, the newly formed national language of New Guinea. We developed the Linguistics Laboratory, a place people come to from here, there and everywhere to learn how to work with language in a scientific and realistic way. We work with one foot in the university, and one in the community. In the course on "The Study of the Speech Community," students learn how to cross the line that separates the university from the world around it. They make friends in the local neighborhoods, gather data on social life, and analyze it by quantitative techniques.

If this empirical approach were the dominant way of doing linguistics and linguistic theory, I would certainly have lost sight of the academic adventure that once inspired me. Fortunately, this is not the case. Linguists are still basking in their own ideas, still finding the answers by asking themselves questions, and most of them are frightened by any number larger than six. But the general impression in the field is that if you want to study how people actually *use* the language, and if you want to *measure* what you are studying, you should come to Penn and work with Sankoff, Kroch, Prince and Labov. We have a growing number of students who are afraid neither of people nor of mathematical symbols. The technology becomes more exciting all the time. We've derived equations that give some insight into why language keeps changing, and who changes it. And it's turning out that our knowledge of dialect diversity has important applications for automatic speech recognition. If a computer is going to understand how human beings speak, it has to understand Chicago speech as well as New York speech. And we've now succeeded in mapping these sound changes through Telsur, the telephone survey that has produced the Phonological Atlas of North America.

All of this technology could easily carry us away from the human issues involved in the use of language. From my point of view, that might win the game but lose the match. I spend a great deal of my time in the laboratory, at the office, or in class. But the work that I really want to do, the excitement and adventure of the field, comes in meeting the speakers of the language face to face, entering their homes, hanging out on corners, porches, taverns, pubs and bars. I remember one time a fourteen-year-old in Albuquerque said to me, "Let me get this straight. Your job is going anywhere in the world, talking to anybody about anything you want?" I said, "Yeah." He said, "I want that job!"

Once you find out what you want to do, you have to convince somebody else to pay you to do it. You can defend any piece of research by saying that it is "theoretical" and "basic" research, and you may be able to get

the grants you need. I myself have always felt that theory can only be justified if it fits the facts, and that some facts--the ones that affect people's life chances--are more important than others. Four years ago, I organized another research group to return to the problem of black/white differences in Philadelphia, and we discovered that the differences we found in Harlem are not growing less. On the contrary, the home languages of blacks and whites are growing more and more different from each other. This became a national news story, and we were able to use the facts to underline the dilemma that Ted Hersberg of the Penn Philadelphia Social History Project has shown us: that increasing segregation in the northern cities is depriving the black community of its basic resources, and is in danger of creating a permanent underclass. Sociolinguists have now discovered that this is true in every city in the country: while the white dialects are continuing to develop and diverge from each other, the black community of the inner city holds aloof from all this, and has developed a nationally uniform grammar that is more and more distinct from that of the surrounding white dialects.

This year the renewed controversy about African American English surfaced in the "Ebonics" Controversy. When all the furor died down, it became clear that the African American community of Oakland has finally decided, as a whole, that it is time to stop blaming children for the failure of the schools, and time to improve our methods of teaching reading by using our knowledge of the language that children actually speak. After thirty years of effort, there is now a distinct possibility that the knowledge we have gained can be put to work, and here at Penn we are once again putting our shoulders to the wheel.

In 1987, I had another opportunity to test the usefulness of linguistics on a matter that was vital to a single person. A number of bomb threats were made in repeated telephone calls to the Pan American counter at the Los Angeles airport. Paul Prinzivalli, a cargo handler who was thought by Pan American to be a "disgruntled employee," was accused of the crime, and he was jailed. The evidence was that his voice sounded like the tape recordings of the bomb threat caller. The defense sent me the tapes because Prinzivalli was a New Yorker, and they thought I might be able to distinguish two different kinds of New York City accents. The moment I heard the recordings I was sure that he was innocent; the man who made the bomb threats plainly did not come from New York at all, but from the Boston area of Eastern New England. The problem was to prove this in court to a West Coast judge who could hear no difference between Boston and New York City speech!

All of the work and all of the theory that I had developed since Martha's Vineyard flowed into the testimony that I gave in court to establish the fact that Paul Prinzivalli did not and could not have made those telephone calls. It was almost as if my entire career had been shaped to make the most effective testimony on this one case. The next day, the judge asked the prosecuting attorney if he really wanted to continue. He refused to hear further statements from the defense. He found the defendant not guilty on the basis of the linguistic evidence, which he found "objective" and "powerful."

Afterwards, Prinzivalli sent me a card saying that he had spent fifteen months in jail waiting for someone to separate fact from fiction. I have had many scientific results where the convergence of evidence was so strong that I felt that I had laid my hands on the reality behind the surface, but nothing could be more satisfactory for any scientific career than to separate fact from fiction in this case. By means of linguistic evidence, one man could be freed from the corporate enemies who had assailed him, and another could sleep soundly on the conviction that he had made a just decision.

William Labov



