

WILLIAM LABOV. AN INTERVIEW

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1. On language change

1.1.

— **Although we know that language acquisition is not isomorphic with change, can you develop the statement that “language acquisition can be a fruitful source of explanation for change”?**

— The explanation of change comes from children’s acquisition of language, and nothing that we have done really bears on this. A lot of people look at the mistakes that children make, and in so doing, they think that studying an individual behavior is going to explain the language, but we do not think that an individual change has anything to do with the language, because language change occurs when people adopt the behavior of others. So, what a child does in relation to the acquisition of language does not have any immediate effect upon language, which is a social fact. But, the work that was done in *King of Prussia* in connection with the Project¹ on the sound change in Philadelphia shows that there are two radically different modes of acquisition: one pretending to be *low-level, phonetic patterns*; the other, the *high-level, more abstract, phonological patterns*, and people entering the community will learn the low-level phonetic rules, but unless their parents were born in the area they do very badly at the high-level patterns. So, then the question remains how is it that the system ever changes, and to understand that, we have to follow the peer groups through the pre-adolescence, the adolescence high-school pattern. One person, who has done a great deal of work in this area, who really has great explanatory variables is Penelope Ecker, on the story of the socialization of children in the Michigan High School. Now, the fact is that the whole area of research which has to be developed concerns the relationship between the socialization of the child in the peer group and its ability to remove the pattern that was acquired from the parents.

1.2

— **In 1983, in the article that you wrote in *Locating Language in Time and Space*, you affirm that “how a sound change is first set into motion and its**

momentum maintained is still a riddle". Why a riddle? Would you still accept the statement, or is there any more evidence to overcome the riddle?

— The problem of the actuation of sound change, how a sound change is first set in motion and is maintained is certainly still a riddle. That article in 1983 gave a hypothesis, which since then we tend to confirm, which looks back to the ideas of Meyer, saying that the explanation of the sporadic feature sound change really depends upon finding some other factor, which is also sporadic, and that cannot be a universal property of language. So that, it is the entrance of new groups into the social situation which seems to provide us a primary cue or motivation for ongoing sound changes, particularly the newer sound change. That characterizes most of the theories we know, that they are subject to the entrance of new groups from different areas, and the older groups react against the entrance of these groups, and then, language becomes a symbolic means of ascertaining local rights and privileges. This is still our general view, but it is far from proved that it raises the question as to whether we could find a city which has maintained a steady development of sound change, which has been isolated from the entrance of other groups. But in one sense there are always other groups entering society, that is, the adolescents, children, who have to make their way into society and find a position when they have none, So that, the tendency of these new groups to enter, and the older groups to react against them is really basic to the replacement of the old pattern.

2. On the curvilinear pattern²

— **One of the most important findings of the *Project on Language Change and Variation* (LCV) is that linguistic change does not originate in the extremes of the social scale, but rather in an intermediate group, the upper working class, or the lower middle class. Is there any more fresh evidence to confirm this "curvilinear pattern"?**

— Our Philadelphia study was designed to test this hypothesis, and it appears to be quite sound. Two of the three new and vigorous changes show this curvilinear pattern. Now, a challenge was raised to this by Anthony Kroch, who said that he feels that difference in men and women is crucial here, because perhaps the reason that the lower class is not leading in the change, and that lower class men react against the change which is dominated by the women, and thus, the fact that the upper working class and the lower working middle class coincide in this, is due to the fact that the lower working class men retreat from change. When we examined separately the behavior of men and women, we found, indeed, some evidence for Kroch's point of view in regard to one of these new and vigorous changes, which is the raising and tensing of the [aw]. But, when we came to studying the other one, the raising of checked³ [ei], it turned out that men and women were identical across all class patterns. Both the male and the female showed this great advantage of the skilled working class over the unskilled working class. So, the generalization is still maintained that it is the central groups of this local system that lead the

sound change. Now, in the United States what this means is that the upper working class and the lower middle class jointly do this, they are the central group, and they are closely associated. And this is very upsetting to people with a Marxist orientation frequently, because they like to feel that the difference between working class and lower middle class is important. But, of course, that does not make sense for a Marxist view either, because they are all workers. Ideologically people tend to think that the lower middle class aligns itself culturally with the upper middle class. That is not true, in regard to most linguistic mechanisms, until the point is reached when it becomes conscious, when the change is almost finished, and then there is stigmatization. At that time, the lower middle class reacts against their own pattern more sharply than the upper working class. The work in Panama City by Cedergren, the work in Norwich by Trudgill⁴, and our work in Philadelphia, all show this pattern. Now, Marco Oliveira⁵ has challenged this in his dissertation, where he feels that he has proved the existence of sound change in the realization of —r in Portuguese, which is led by the lower class, and he said that we have to revise this view, because in Brazil the dominant social class is the lower class, and so we might say that it is not so much the intermediate groups, as whatever the dominant group is in society, and that there might be another way of looking at it. What is clear, however, is that the upper middle class, the College educated group, which adopts a national reference Standard in its culture, is never the leader in linguistic change, and it is the truly lower class which is marginalized and they are also not involved.

— **The paper that Nuria Alturo and I have presented at NWAVE XVII, on an ongoing linguistic change which is taking place in a Catalan speech community, *El Pont de Suert*, —where the local variant [tʃ] is being substituted by the Catalan standard [ʒ]— also proves this curvilinear pattern.**

— Well, it just finds it again, in an area, but this does not have anything to prove, because it is a question of how general this phenomenon is, and it goes back to the question as to the people who defend the local neighborhoods are the people who are the stronger investment on local rights and privileges. The reason this has never been understood by University people is that, at large national universities, linguists have not been able to understand why anyone would prefer to use a local dialect. And it turns out that if your own reference group is the university and nation, the you have no investment in being local. Now, for most people, including people at local universities it is the other way round.

3. On Black English

3.1.

— **A year-long debate on the language barrier between blacks and whites took place around the country in 1985. The headlines were:**

* *Black and Standard English Held Divergence more*, The New York Times, March 14, 1985.

* *Shed A Tear As A New Era Of Separatism Overtakes Us*, Buffalo News, March 30, 1985.

* *Black Vernacular Is Going Its Own Way, Linguists' Study Says*, The Detroit Free Press, April 4, 1985.

* *Study Finds a Growing Black-White Language Barrier*, The Philadelphia Enquirer, April 8, 1985.

* *The World Is Out. Students Told Dialect Can Be Inappropriate*, The News and Observer Raleigh, April 23, 1985.

* *What They Say, Home? English Dialects Are Adding To Racial Misunderstanding*, The Washington Post, May 6, 1985.

* *Divided We Speak, It Seems*, The Boston Globe, November 21, 1985.

Can you reproduce the arguments and the evidence that you used to write the report on BEV (Black English Vernacular) and SE (Standard English) divergence, and which caused so many reactions in the press and among the community members involved?

— Well, our fundamental argument on the divergence of BEV and SE, No, let me correct that. No, we said nothing about the divergence of BE and SE, nothing at all, at no reason. We found something very different. What we found is that BE vernacular is becoming increasingly divergent from other vernaculars in the United States, not from SE. Now, SE is one of the many other white vernaculars, white dialects, but we are really concerned with the white vernacular of Buffalo, Chicago, and so on. And there we find that all the sound changes that we are describing are being driven in the white community, and the Blacks do not participate at all. On the grammatical front, there is some evidence of the continuing convergence, but the evidence for divergence is massive and continuous. When we first produced our results, they were based upon studies in Philadelphia, but also there was a large view. That Large view is one that has escaped most people. What characterizes the BEV? Primarily, there are massive differences in the tense and aspect system. Not only there is a great range of particles with different meanings, but there is reason to believe that the BEV does not have an obligatory tense marker, and this separates it from most of the dialects. Those tense and aspect markers: *BE*, stressed *BIN*, *BE DONE*, *STEADY*, *COME*, are extremely interesting because their semantics are complex, and people like Arthur Spears, John Baugh, and others have been investigating their semantic properties, which turn out to be quite complex and interesting. Not a single one of them corresponds to a semantic particle described in the Caribbean, not one. And we have to accept the fact that maybe the features of *BE* reflect an Afro-Caribbean origin. So, how is it possible that the semantics of all these particles are so radically different in the United States? For example, *BE DONE* is used in ways that look like the English Future Perfect, but if you look more closely you find cases where it is a resultative, where it implies that the second action of the future series follows

inevitably to the first in ways that cannot be translated into other dialects. No one has even hinted at the existence of such a feature in the Caribbean. So, all of that made me argue that this elaborate tense and aspect system must have developed in the United States and, of course, it is typologically Creole because it refers to particles that precede the verb, which do not have a strong tense orientation. The conditions, however, for divergence are there in terms of increasing racial segregation. And then, it turned out that Guy, Bailey and others had extraordinarily strong evidence which multiplies everyday, for the fact that the most important of these particles, invariant *BE* did not exist in the XIX Century, does not exist among older rural Blacks, but is found among the young urban Blacks in the South. I just heard⁷ a paper of John Rickford on this group at Stanford, which showed in even more categorical fashion that that invariant *BE* does not exist among anyone except the young people, so that in these small communities around the country we see the evidence of this massive migration and segregation of Black people, and its resulting creation of new and divergent patterns. People who examine all the arguments are frequently not oriented linguistically. We showed that Philadelphian young Blacks have developed a pattern whereby the third singular -s is used in the past, but not in the present. So what created this is a new rule which has nothing to do with its use in the past alone, since people do this all the time in the historic present. The structural fact is that they do not use it in non narrative present, but they do use it in the narrative past.

3.2.

— In 1974, R. Wright argued in his review of *Language in the Inner City*⁸ that there is a false dichotomy of BEV vs. SE, and that you were viewing the Black community as a dialect community and not as a speech community. Do you nowadays think that this dichotomy is really false? Would you admit that you are still dealing with the black community as a dialect community and not as a speech community?

— As far as Richard Wright's argument, that was part of an older reaction of Blacks who were not prepared to accept the fact that Blacks are different from Whites. And this characteristic has been well described. It was the result of a long emphasis on egalitarian arguments, that any difference between Blacks and Whites must be due to the inferiority of Blacks. And that has since disappeared, so that Black people today recognize that BEV is quite different, and Wright's objections, along those of others, are a part of the history of the subject, they are no longer held by Black scholars. This question about the distinction between dialect community and speech community, I do not understand.

— **This is what Wright, I think, also wrote.**

— Well, I do not understand that distinction.

4. On Linguistic/Variable Rules

4.1.

— In the Seventies, after *Sociolinguistic patterns*⁹ was published, you were accused (Darnell, R., *Language*, vol. 51, No. 4, 1975), of proceeding to analyze linguistic and social facts separately, in spite of your argument that these facts are inseparable, and of not considering social facts as part of linguistic rules, that would incorporate social information. What can you say about this?

— This article by Darnell, that you mention, I am not familiar with, but there was a tendency in the early sociolinguistic development in the Sixties and early Seventies for people to exaggerate the relationship between language and society, since they felt that sociolinguistics involved the relationships between language and society, they could then say that every linguistic fact has a social side, and every social fact has a linguistic side. Nothing could be further from the truth. The great majority of linguistic facts, an enormous body of linguistic facts have no effect and social correlation and are quite immune from social affect; people are not aware to them, they cannot talk about them, and they do not show a difference in social distribution; they are extraordinarily uniform in the population. This is certain for the vast majority of linguistic facts. So, for us the question is quite different. It says we want to see whether we can define the relationship between language and society, and look at the interface between them. Now, this interface I used to think was quite small, it is primarily on the surface. But, social affect is on the words inside. There are some counterexamples; for example, in English, the double negative is highly stigmatized, and that is a structural fact; there is no way of getting away from it, it has nothing to do with words, or to how these words are organized; it is a counterexample. But the generalization is true, and it shows out most clearly in the study of the social effects of sound change. Mergers everywhere have no social effect; no group ever reacts to it, no one is ever stigmatized for the absence of a distinction and the presence of a new one. All of the social affect is directed to the presence of a certain sound, and in particular, the sound in certain words. For example, the merger of *cot/caught*, throughout the United States, is quite invisible and inaudible to those people. Teachers do not care about it, no one has made fun of it for the absence of the distinction between *Don/Dawn*, *cot/caught*, even though it is a sweeping and massive reduction in the inventory of sounds. However, New Yorkers in California are stigmatized in their pronunciation of *law*, and they learn to say [oh], but there is nothing that tells them to merge *cot/caught*, because people do not care about such things.

4.2.

— Can you relocate the nature of linguistic rules, and in particular, of variable rules?

— Now, the question as to whether social grammars can be attached to linguistic rules is an interesting one, and it has been discussed by a lot of people. And the status of linguistic rules, they have to be seen as heuristic devices strictly, but the really, substantive question to what actually, what rules govern people's behaviour, and when they follow them, and when they go separate ways. That is still an open question. The status of variable rules in the grammar has been raised again by the work of Anthony Kroch who has been studying the development of *DO* support in the History of English, and finds that across the centuries there are some very regular and steady changes in the frequency of *DO* support, which are clearly learned by people as they grow up. But this issue was originally raised by people who could not imagine that quantitative differences could be a part of linguistic behaviour. This is very strange because Linguistics is the only field where people think while also learning another. And in fact, recently, in dealing with certain questions of functionalism, I have had occasion to learn more about probability matching, which is a formal behaviour which directly reflects people's capacity to repeat generation after generation the quantitative variation found in the speech on their parents and grand-parents and great-grand-parents. Now, probability matching is the following sort of behaviour: when an animal, including a human being, is faced with a reward which occurs three quarters of the time on the left, and one quarter of the time on the right, we might think that they would maximize a situation by always choosing the left hand channel, but they do not. probability matching, given a variation of this sort, returns to the left three quarters of the time and one quarter to the right. This behaviour may seem irrational, but studies of foraging have shown that, indeed, it is a way, the only way, in which animals can maximize their participation in the food chain. Human beings follow this kind of behaviour quite sharply, and we find more and more that this kind of variable behaviour and the replication of frequencies is built into people's behaviour. I have recently written a chapter called, "The child as a linguistic historian", which examines children's acquisition of variable constraints at an early age, and I am able to show that it is at the age of four, five, six, that children acquire these, and more surprisingly, they seem to acquire the specific socially motivated rules of their community quicker than the universal patterns that are dictated by the articulatory apparatus.

4.3.

— **Do you still believe that variable rules can be incorporated into a Universal Grammar?**

— Universal Grammar is a topic that you have raised. I have very little sympathy with it. I think that the search for generalizations, if that is what you mean by universals, is quite important, —cross linguistic universals—, but Universal Grammar does not mean that. For most people a Universal Grammar has been associated with the search for some innate mechanism which dictates

uniquely the output of the grammar, and it corresponds to the idea that there are some functions of the grammar that are so isolated from all other functions that they cannot be over-ridden, and uniquely determine the way in which people speak, or form their rules. I am certain that the search for such a mechanism will prove illusory, though I welcome my colleagues' search for it. I am sure they will learn a great deal in the course of this activity. But, all the evidence we have indicates that the forces that determine linguistic behaviour combine in a fashion in which one can influence the other, and that they will be no unique innate override of the result; instead, we will find that higher-level generalizations will be isolated by a multivariant analysis, and that is the business we are engaged in.

5. On functions of Language

— In his review (1979) of Labov's and Fanshel's *Therapeutic Discourse* — a study of the relation between language and conversation, Smalley pointed out that there was no reference in that book to Halliday's work, while he affirms that the kind of framework that you and Fanshel postulate is very near Halliday's functions of language. Would you accept this, and if so, in what way do you think that these two frameworks are compatible?

— I feel that Labov and Fanshel's *Therapeutic Discourse* omitted reference to many, many important works in discourse, and in particular, I feel that we did not make good use of Grice's insights at that time. We instead developed our own point of view which was built to a large extent upon the insights of Hockett, and other scholars. I admire Halliday's work, in particular his division of the grammar, the areas that concern the determination of information, of the participation of people, agents into the actions, and that otherwise, shows a poly-systemic character. But, I feel that our work did not make close contact with sentence grammar. What we did succeed in showing was that there is, of course, no single speech act that is being performed at any one time, but which are interactional, like challenges, defences, retreats, and so forth; and in order to understand the coherence of discourse we have to raise to higher and higher levels of abstractions. The problem of relating these speech actions to the syntax of the language is a formidable one, and perhaps more progress is made by people who begin with the grammar itself, and try to connect it with discourse. I think that many of the principles that we have developed have been useful to other people and have been built upon. I myself, however, have retreated in recent years from many considerations of functions of language. I used to write and talk about Halliday's functions and Jakobson's functions, Hyme's functions, and I feel that that was an illusion, that we will get nowhere by talking about the functions of language in this sense, because we do not know what these functions are; in other words, we do not know why we do what we do, in the sense that we do not know the purposes of our behaviour. We think we do, but in many cases we are wrong. Now, Jakobson introduced into Linguistics the means/ends model, and he was quite frank that he

wanted to introduce a teleological explanation, and many of the people doing functional analysis continue in that vein, like Talmy Givon¹⁰ and others, who do very interesting work, but they begin with the notion that they know what the functions of language are. All the research that we have done in the speech community shows that this is not very likely. We may know, or may not know, but we have good evidence to indicate that people do not choose variant A vs. variant B in a purposeful way, and they certainly do not choose it in regard to maximizing functions of language. Now, you may ask why do they behave in this way. The functional view that I would prefer is that which is common in Biology and Sociology. When we say we know the functions of language, we do not mean the purposes for which people do things: we rather mean what are the consequences of the action for the system. We have made considerable progress recently with the help of Anthony Kroch, Gregory Guy, and others in examining language behaviour from this point of view. If you look at the consequences of understanding or misunderstanding, or the consequences of choosing one linguistic form or another, you then begin to understand how the system is affected, and how the system in turn reacts to determine what people do.

6. On quantitative analysis

— In her review of *Locating Language in Time and Space* (*Lingua*, 60, pp. 87-102), S. Romaine writes that the danger in using quantitative analysis is that it can involve “the risk of dealing with trivial issues or only certain aspects of linguistic structure, simply because they are quantifiable, or of discounting as irrelevant those which are not”; in other words, she is telling us to be on guard against our own ignorance of the limits of quantitative methods, and not to use quantitative analysis as a “fishing operation”. In Spain, the studies on variation using quantitative methods are beginning to proliferate. Now, what would be your advice, so as not to fall into the above-mentioned trap about misuse of quantitative frameworks?

— I would agree that in every field there is a danger of doing bad work. I think that we should avoid doing bad work, and we should connect what we are doing with the most important issues that we can address. I do not think that Suzanne Romaine should apologize for the quantitative work that she has done in the past. I think that some of it was quite good.

7

— **What is your present research activity?**

— The research group on cross-dialectal comprehension at the University of Pennsylvania has been doing research over the past three years on the cognitive consequences of dialect diversity, examining the perceptual aspects of the changes

in speech production which have been traced over the past two decades. And I have just realized that for a hundred year, two hundred years people have been studying language change and changing sounds. But, this is the first time, to my knowledge, that people have studied the perceptual aspect and its significance for the major knowledge of language. Perhaps the major finding of our work in the speech community is that sound changes continuously and rapidly in all the major cities in the Unites States. Buffalo, Boston, Philadelphia, Chicago and Los Angeles are more different from each other than were fifty or hundred years ago. This finding violates the common sense expectation that the overwhelming effect of exposure to the uniform language in mass media would produce an extra linguistic convergence, and that all dialects will give way to a general Standard. Instead we find an increasingly divergence of the sound system led by the higher status members of the local community. In 1983, I participated in a seminar session in Montreal where our research group reported work on the relation of working minorities to linguistic change. Four papers outlined our findings that the BEV showed evidence of continued divergence from all the dialects, as well as convergence. This group of papers appeared in 1986 in the volum on *Diachrony and Diversity*, edited by David Sankoff. The directions of change in Black English continue to be a subject of research and disagreement. One of the major bodies of evidence concerns the fact that Black English seems to be aloof for the accelerated sound changes that are taking place in our local Black communities. As we turn to examine the perceptual effect of this linguistic diversity, and examine this comprehension across speech communities we are led to look even more closely to the communication between Black and White sections of the community. I want to refer primarily to comprehension and miscomprehension across White dialects, but the Black-White differences relate to the picture as well. Now, the dialects that are found by American Linguistic Geography do not reflect any major pattern of linguistic organization. They are defined by a miscellaneous set of regional words and an inventory of boundaries, that is, the phonological inventory. But, they also reflect overall directions of phonetic organization that are remarkably uniform across the vast areas. Sound change in English dialects follows two major patterns of change shifting: the Northern cities shift and the Southern cities shift. The Northern cities shift involves the change shift in six vowels. This shift is characteristic of the major cities of the Northern dialect area: Buffalo, Rochester, Detroit, Cleveland, Chicago, and Porthwest. It is found in a less advanced form throughout the area depending on the size of the speech community. Now, for our research we selected Chicago as the target city to represente the shift. I should say that Chicago, the second larger city in the United States has a dialect which is totally unknown to anybody else in the United States. Whenever we play the material that we have collected, people say California, Hollywood, New York, anywhere. We have many, many speakers who show the Chicago pattern. We base our conclusions on the work on vowel identification done by Sherry Ash. The Southern shift has even a more spread one in English dialects. It is found throughout Southern and Eastern England: New Zealand, the south of the United

States, the gold States, Texas, and the South Atlantic cities, like Baltimore and Philadelphia. Now, Philadelphia was originally a Southern city which participated fully in the Southern shift, but recently the shift shows a reversal, and the city participates in some patterns of the Northern shift. In addition, Philadelphia, the bigger target city in our research shows an interesting pattern of sound shift. Now, most of the work presented at N.W.A.V.E. in previous years has concerned change and variation in speech production. The significance of these processes for the speech community can only be determined by studying the perception and the interpretation of this variation and, of course, this requires experimental techniques. The challenge will be to see whether we can carry on such experiments without losing contact with the production and interpretation of spontaneous speech. The full development of this sound shift is to be found. The study of comprehension will, of course, relate to the consequences of change, but it will also bear upon the causes of change. It has often been argued that the motivation and direction of sound changes can be understood as a response to other changes that reduce the communicative function of language. On the other hand, it has been argued that radical changes in the phonological system do not necessarily reduce the communicative function of language. So, in 1966 Weinreich, Herzog and myself¹¹ pointed out that, after all, if a language has to be structured and accomplish a function efficiently, how is it that people continue to talk while the language changes, that is, while it passes through periods of less systematicity. Alternatively, if there are pressures which do force a language to change, and if communication is less efficient in the interval, why have such inefficiencies not been observed in practice. The first thing we want to do in studying cross-dialectal comprehension is to interpret misunderstandings as they occur in daily life. We have been able to record almost five the past two years. We find that a 27% of these are motivated by dialect differences. And many of them illustrate the effects of the changes that we have been discovering. Many of these misunderstandings occur within the community while the sound change is taking place, reflecting the fact that communicative problems produced by a change are not confined to cross-dialectal comprehension. Now, the shift difficulty with such an observation is that there is a strong bias towards accumulating the less important misunderstandings, those that are detected. It is the misunderstandings that nobody notices that can cause the most profound maladjustments in social life. Our current research began with a pilot project conducted by Robin Sabino and myself where the basic technique of extended decoding in context was used to estimate the amount of misunderstanding that occurred across dialects with full contextual information. Our main finding reversed our original questions, which was, how do people in Philadelphia understand the rapidly rotated phonology of Chicago. We found that this was a wrong question, because in many cases Philadelphians do not understand Chicago speakers at all. So the question changed to what is the full extent of cross-dialectal understanding under natural condition and how much the sound change interfered communication, and secondly, how does phonetic, phonological,

syntactic, semantic and pragmatic information combine in the process of interpretation.

Notes

1. *King of Prussia* is a neighborhood in Philadelphia. The project refers to the project on *Language Change and Variation* (LCV) directed by Labov at the University of Pennsylvania.
2. One of the major findings of the LCV is that linguistic change never originates in the extremes of the social scale, but that it rather shows a *curvilinear pattern*, according to which there is a distribution of frequency of an innovative variant, such that a greater frequency is found in an intermediate group (the upperworking class or the lower-middle class) and the frequency falls on each side of the peak.
3. Sounds occurring in closed syllables.
4. H. Cedergren, The interplay of social and linguistic factors in Panama, Unpublished Cornell U. dissertation, 1973. P. Trudgill, *The Social Differentiation of English in Norwich*, Cambridge University Press, 1974.
5. M. Oliveira, Phonological variation in Brazilian Portuguese, Unpublished U. of Pennsylvania dissertation, 1983.
6. N. Alturo & M. T. Turell, Linguistic change in *El Pont de Suert*. The study of variation of /ʒ/, Paper presented at NWAVE XVII, Montreal, October 1988.
7. At NWAVE XVII (Montreal, October 1988), where I interviewed Labov. NWAVE stands for *New Ways of Analyzing Variation in English and Other Languages*.
8. W. Labov, *Language in the Inner City: Studies in the Black English Vernacular*, University of Pennsylvania Press, 1972.
9. W. Labov, *Sociolinguistic Patterns*, University of Pennsylvania Press, 1972.
10. T. Givon, (ed.) *Topic continuity in Discourse: A quantitative Cross-Language study*, John Benjamins, 1983.
11. Weinreich, U. W. Labov & M. Herzog, Empirical foundations for a theory of language change, in *Directions for historical linguistics*, University of Texas, 1968.