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ARE BLACK AND WHITE VERNACULARS DIVERGING?

PAPERS FROM THE NWAIVE XIV PANEL DISCUSSION

EDITOR'S NOTE: The contributions which follow were presented in earlier forms at a special symposium of the Fourteenth Annual Colloquium on New Ways of Analyzing Variation in English (and Other Languages), 24 October 1985, at Georgetown University.

I. INTRODUCTION—RALPH W. FASOLD (*Georgetown University*)

WHEN STUDIES OF VERNACULAR BLACK ENGLISH became common during the 1960s, a major controversy involved the degree to which vernacular black English was different from the dialects of American English spoken by whites. Linguists unanimously rejected (and many of them published studies that refuted) the notion that VBE was a distorted and cognitively crippling version of English. But two alternative interpretations were actively disputed. According to one view, VBE is an ordinary development from the dialects of the British colonists—the same as, or only slightly different from, the dialects of disadvantaged southern whites, the observed differences between black and white speech in northern cities being due largely to the southern regional dialect features that were brought north by black in-migrants about the time of World War II. The other view involved the creole origin hypothesis, according to which VBE developed from a plantation creole with a strikingly different structure from English. The creole origin hypothesis implied that black and white dialects differed to a much greater degree than would be indicated by the dialect development hypothesis. Proponents held that the original slave creole was in the process of decreolization under contact with American English, but there were and continue to be differences of opinion among linguists who accept the creole origin hypothesis about how much decreolization has taken place—that is, how similar VBE and other American English dialects have become.

Recently, another interpretation of the relationship of black vernacular and white dialects has been proposed by William Labov and has drawn considerable attention from the news media. From this perspective, regardless of how different the origins of the black vernacular and white dialects may have been, there is evidence that black vernacular dialects are becoming increasingly DIFFERENT from surrounding white dialects. In Labov's view, certain grammatical features of VBE have de-

veloped in such a way that it is now more unlike white dialects than it previously had been. Labov sees this as both a symptom and a contributing factor to increasing schisms between some black Americans and the majority society. Isolation from white speakers, in this view, has led to dialect divergence, and this very divergence has made it even more difficult for black vernacular speakers to benefit from public education (provided in a version of English which is now even more alien than it once was) and otherwise deal with the mainstream of society. In consequence, the issue is not just an academic issue about a sociolinguistic phenomenon but a matter of serious concern about present and future social justice and stability in this country.

Other linguists have found Labov's arguments unconvincing and the representation of his views in the news media to be potentially damaging to the black community. Not satisfied in the first place that dialect divergence has taken place, these scholars are concerned that the general public will interpret the divergence as indicating that the grammar of the language of blacks is getting "worse." In the context of this controversy, the conveners of NWAVE XIV, an annual linguistics conference that focuses on sociolinguistic variation, organized a panel discussion of this issue. The panel was composed of Labov and a group of scholars with expertise in VBE linguistics who support or opposed the divergence concept to varying degrees. I served as moderator. Subsequently, *American Speech* editor Ronald Butters agreed to compile the papers presented and publish them, with an edited transcript of the discussion which followed, in the present issue of *American Speech*. The panelists have all submitted edited versions of the papers they presented at NWAVE XIV.

It would not be appropriate for me to attempt a summary of the articles in this introduction. The positions of the contributors are best represented by themselves. I consider it important, however, to make readers of this collection aware of a potentially hidden issue in this controversy. Labov's approach to the study of language takes observed data very seriously. The grammatical structure of any speech variety is to be discovered in the use of that variety in its community. A contrasting approach to the study of grammar entails maintaining a careful distinction between grammar and use. This difference in approaches to grammar has implications for readers of these papers. A linguist who assumes that grammatical structure is to be discovered in language use is more likely to find a change in grammar, say between present-day and earlier records of VBE speech, if a difference in use of a particular feature can be demonstrated. Other linguists, assuming that grammatical structure

often stays constant while community conventions for its use change, will probably assert that the grammar is still the same, even if they agree with the first linguist that the feature is now used in somewhat different ways than it formerly was. I am convinced that the issues addressed by the contributors to this issue cannot be reduced to underlying differences in their concepts of grammar. There are certainly substantive disagreements among them, and this will become fully apparent as the articles are read. Nonetheless, I believe that implicit assumptions about the degree of separation of grammar and use have a bearing on the discussion here, as well as in other work on linguistic variation.

II. WILLIAM LABOV (*University of Pennsylvania*)

The material I'm going to put before you tonight was first presented at the Montreal NWave meeting in 1983, as a series of four papers (Labov and Harris 1983; Ash and Myhill 1983; Myhill and Harris 1983; and Graff, Labov, and Harris 1983). These papers are going to appear in the conference proceedings (D. Sankoff, forthcoming). You will also be able to read the full report of Bailey and Maynor's study, which Guy Bailey will report on later (32–40 below), in a forthcoming issue of *Language and Society* (Bailey and Maynor 1985a). Bailey and Maynor's work was quite independent of mine and arrives at similar conclusions.

These findings—that there is continued divergence of black and white vernaculars—have been seen as newsworthy, as Ralph Fasold just indicated. That itself is an interesting fact. A reporter from the *New York Times*, William Stevens, wrote a story that became the center of a much wider series of reactions in the newspapers, magazines, radio, and television, raising many issues that were not the focus of our original article. I've been present at several media events with some of my colleagues here tonight—Fay Vaughn-Cooke and Arthur Spears—and they have contributed quite a bit to my thinking. So I would be giving you a false impression if I said that I had thought through all of the issues by myself: what I will say tonight has already benefited by their views.

The findings that we presented were based on two lines of research. First of all, there are studies of sound change in progress sponsored by NSF through the 1970s. These showed continued evolution of local accents in all the cities examined: New York, Boston, Buffalo, Detroit, Chicago, and Philadelphia, with radical change—shifts, mergers, and splits—so that the white vernaculars of these cities are probably more different from each other than they were fifty years ago. (For the study of

sound change in progress in American cities see Labov, Yaeger, and Steiner 1972; Callary 1975; Laferriere 1979; and Labov 1980). That in itself is startling to many people who think that the mass media would have such a powerful effect that we would have convergent dialects throughout the United States. This phonetic divergence is an important part of our findings, though it is difficult for those who are concerned only with the black situation to focus on it. Our reports are not only of blacks moving off in their own direction, but also of whites moving off in their own direction.

In all of the cities concerned, we find the cities splitting into black and white components: blacks do not participate in the sound changes of the white vernacular. Instead, we find a generalized northern black phonology that has distinctive features of its own, with only small local differences. I'm not going to restate the evidence for these findings on sound change here. Studies of sound change in progress in New York, Philadelphia, Chicago, and Boston have been well established by data from both apparent time and real time. Recent controlled experiments on subjective reactions to particular Philadelphia vowels have isolated the phonetic features that identify blacks and whites. They show that the white/black division in Philadelphia is well established in perception as well as production (Graff, Labov, and Harris 1983).

There is considerable evidence to support the view that the black vernacular is moving closer to other dialects. I think immediately of Vaughn-Cooke's work on the restoration of initial syllables (1976), but also of research on slave narratives by Fasold (1976), Dayton (1984), and Schneider (1981, 1983). These match the results that John Rickford had when he traced decreolization in the Gullah areas (1974) and later on in Guyana (1979). But there is also evidence for continued divergence of black and white grammars. When we began our study on the influence of urban minorities on language change, sponsored by the National Science Foundation, we suspected that divergence was taking place because all of the social and economic conditions for divergence were there. In each of the northern cities we find a growing black middle class whose use of English grammar is not very different from that of other dialects. But besides this upwardly mobile group who are taking advantage of the opportunities opened in the 1960s, there is an even larger group of blacks who are isolated by increasing residential and economic segregation from the rest of the community. And here we find evidence of new grammatical features, reinterpretations of features of other dialects, and continued divergence of the tense, mood, and aspect system. I'd like to look briefly at some of that evidence with you.

First, it will be helpful to take a big view of developments of the tense, aspect, and mood systems from the Caribbean to the present data. There can be no doubt that the black English vernacular (BEV) system has its roots in the speech community formed by the plantation system based on the African slave trade, a context very similar to that which generated the English-based Caribbean creoles. But as we get more detailed descriptions of the tense/aspect/mood system of BEV, it becomes increasingly evident that it has diverged and developed radically from its Caribbean relatives. The BEV auxiliary has at least three members with semantic features that are quite distinct from any grammatical functors that have been reported in the Caribbean: habitual *be*, the stressed *been* that has been labelled 'remote present perfect', and *be done*. An early example of *be done* use from a group session with the Jets in South Harlem—a sentence that was poorly understood at the time:

1. 'Cause I'll be done put—stuck so many holes in him he'll wish he wouldn'a *said* it.

In this example, *be done* is equivalent to the English future perfect, and could be translated that way:

- 1a. 'Cause I'll have put—stuck so many holes in him he'll wish he wouldn'a *said* it.

Here the *be* can be taken as a future-oriented particle, with optional 'll reflecting an alignment with the future of other dialects, and *done* shows the perfective semantics of relevance up to another future time (see figure 1). But with increasing frequency, students of BEV found sentences that did not fit into this semantic pattern. Baugh observed from his work in Los Angeles:

2. I'll be done killed that motherfucker if he tries to lay a hand on my kid again.

Here *be done* is attached to the second of the two events (as in figure 2) and it is nonsensical to translate it as a future perfect:

- 2a. *I will have killed that motherfucker if he tries to lay a hand on my kid again.

In her participant-observation studies in West Philadelphia, Dayton

FIGURE 1

| | | |
|---------|-------------------------------|-------------|
| Present | Future time A → Future time B | |
| | be done | |
| | stuck holes | wish that S |

FIGURE 2

| | |
|------------|-------------------------------|
| Present | Future time A → Future time B |
| | be done |
| lay a hand | killed |

(1984) has found a great many of these 'resultative' uses of *be done* and traced the semantic split that has led to the current situation.

The complex semantics of *be*, *been*, and *be done* show an elaboration of the auxiliary that goes beyond anything that has been reported in the Caribbean. That does not mean that the Caribbean doesn't have the complexities that we find with these particles; perhaps they do exist but have not been reported. But until we know otherwise, we have to consider that these elements have been elaborated within the context of American society, at a time when other features of BEV were converging with other dialects of American English.

Another aspect of this divergence, perhaps the most startling to me, is a recent development in the third singular *-s*, which is reported in full in Myhill and Harris (1983). The evidence is well known that the fundamental BEV grammar does not have subject-verb agreement, except in the forms of finite *be*, and that there is no basis for marking the third singular in the basilectal grammar. Like most linguistic evidence, this is not based on FREQUENCIES but on DISTRIBUTIONS—not the number of /s/'s that occur in speech, but the absence of phonological conditioning of the variation, the presence of /s/ in other persons and numbers, the idiosyncratic distribution in the population, and the heavy stylistic effects on the /s/'s that are used. In Philadelphia, we do find a very low frequency of third singular /s/ among the core speakers of our sample, but we sometimes find a relatively high frequency in passages like the following. The relevant forms are in italics.

3. 'Cuz, like my li'l cousin right? he's what, six years old, and my brother, they was playin', next thing you know he comed—the li'l boy, he *comes and hit* me right? I *hits* him back now. All the time, my brother and him was hittin' each other an' everything, an' he start cryin' and run an' my grandmother never said nothin', but then, when he hit me and I went to hit him back, an' he told my grandmother, my grandmother *comes* snappin' out on me, *picks* up the cane an' gettin' ready to hit me with it, an' that's when my mother snapped out on her.

The core speakers of BEV in Philadelphia were more extreme than anyone studied in New York. The speaker quoted here is a sixteen-year-old girl who has only six percent /s/-marking in her speech as a whole. But John Myhill observed that if we divide the text into narrative and non-narrative, we get the distribution in table 1. With sixty-seven full noun

TABLE 1

| | Third Singular NP | | All Third Singular | |
|---------------|----------------------|------|-----------------------|------|
| | /s/ | zero | /s/ | zero |
| Narrative | 6 | 6 | 27 | 32 |
| Non-narrative | 0 | 55 | 8 | 463 |

phrases, the speaker used six /s/'s: but all six were found in narrative contexts. For all third singulars, there were only 1.7% /s/'s used in non-narrative contexts, but 46% in narrative. (Bear in mind that the actual percentage in narrative is considerably higher, since many of the zeroes are deleted regular pasts.)

This specialization of third singular /s/ represents a semantic shift—not to a new semantic feature, as Myhill pointed out, but rather to an identification of this /s/ with the narrative past, in the same contexts that other dialects use the historical present. Though the shift may have been triggered off by analogy with the historical present, the new BEV use is not to be confused with it. First, because of its basic distribution: /s/ is NOT used in the general present, but almost entirely confined to past narrative. Secondly, because of the special rule that Myhill found with conjuncts: the BEV /s/ is used with the first member of the conjunct but not with the second, as in *comes and hit*. This pattern has not been observed with any other dialect.

Is the Philadelphia pattern new? In Philadelphia, it is found uniformly among younger core speakers of BEV, but not for anyone over forty, though they may show extreme vernacular patterns in other respects. Furthermore, there is no trace of it among the blacks studied in New York City in the 1960s, at any age. Over the past two decades, there have been many articles that deal with the use of verbal /s/ by blacks outside of the third singular pattern. It has been argued that this marker takes on a durative value, as in Brewer's recent discussion (1986). But the narrative use of /s/ has not been attested before.

An even more extreme divergence of blacks and whites appears in the paper of Ash and Myhill, who showed the relationship between interracial contact and linguistic divergence for different types of variables: lexical, phonological, and grammatical. Ash developed, along with other members of the research group, an index of contact based on a second survey of our sample that probed into a wide range of contacts across the black/white line. This continuous index was used to divide the population into four groups: blacks with little contact with whites, blacks with considerable white contacts, whites with considerable black contacts, and whites in the normal situation: very little contact with blacks. The upper

part of figure 3 shows data for these four groups for three phonological variables: *a apple* vs. *an apple*, monophthongization of /ay/, and nasalization of final /n/. The fourth variable is knowledge of lexicon particular to the black community. These four variables show a continuous distribution. The more contact blacks have with whites, the more they move away from the black vernacular side, and the more contact whites have with blacks, the more we observe borrowing of black forms. This seems to be a normal situation, until we examine the lower half of the diagram, which is radically different.

This is perhaps the most important display that comes out of our work in Philadelphia. The four groups were studied for grammatical features—the use of third singular /s/, the copula, possessive /s/, and the use of *ain't* in the preterite, where other dialects use *didn't*. Whites with little black contact, whites with considerable black contact, and blacks with considerable white contact are all grouped together, at the bottom of the diagram. The fourth group is isolated from all the others: blacks who have minimal white contacts. These are the speakers who are diverging.

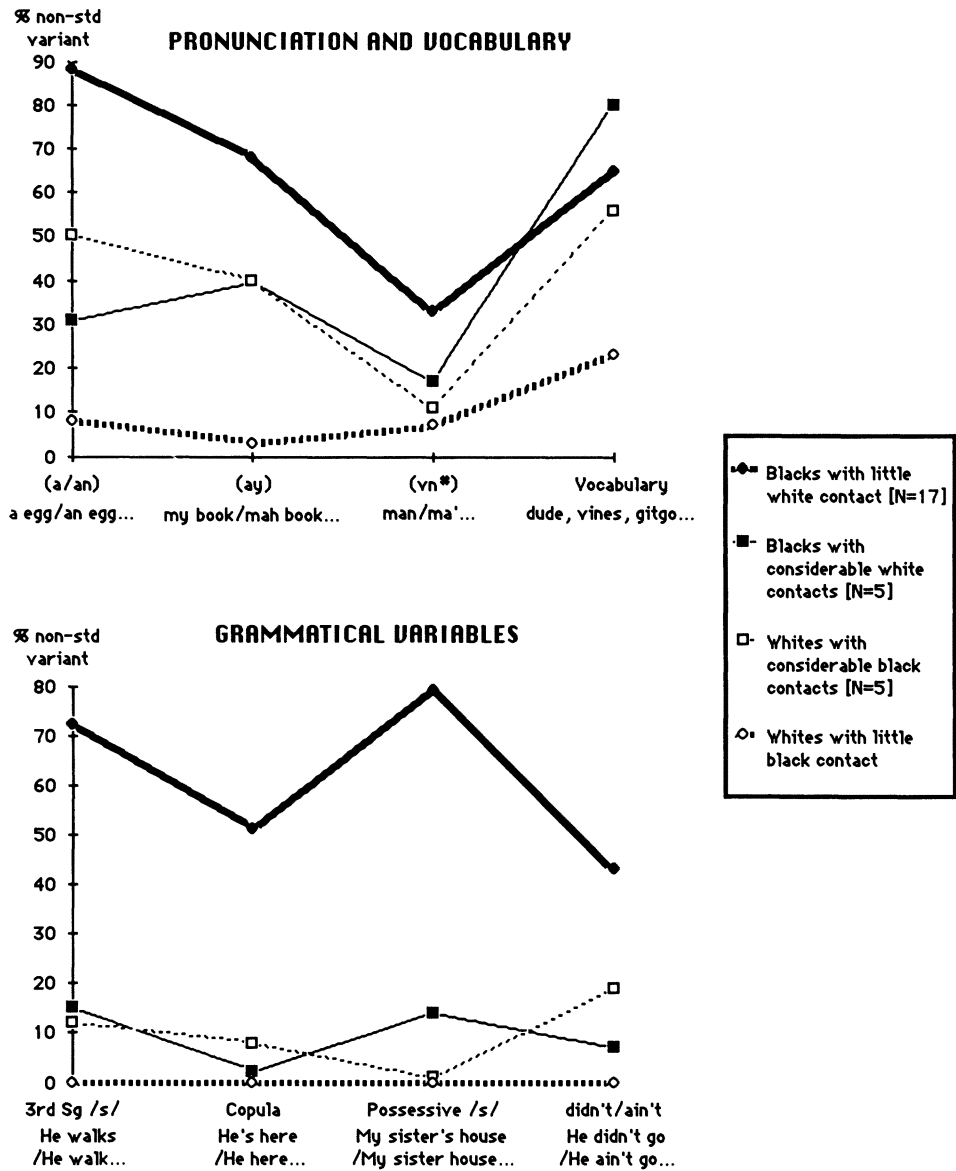
It has been pointed out to me by some of my colleagues on this panel that our data focuses on the black/white situation, but that the divergence should be expressed more generally. These speakers have little contact with speakers of other dialects in general. The absence of the black middle class from the inner cities is as much a cause of the present situation as the absence of whites. I will let others elaborate on this point, since our research paradigm focused upon black/white contact.

There is no doubt that the divergence that we have witnessed on the linguistic front is symptomatic of a split between the black and white portions of our society. It may also be a further cause of divergence in widening the distance between the English of the classroom and the vernacular that the child brings to the classroom. But like many of my colleagues, I see that the primary cause of educational failure is not language differences, but institutional racism. I can quote Arthur Spears directly, from our last press conference on the topic:

Language differences as we see them are symbolic of cultural distance. They become instruments of educational failure when they are interpreted in a way to predict and insure this failure.

One of the most remarkable facts connected with these findings is the constructive character of most public reactions. It is radically different from the response to research programs of the 1960s. At that time, there was a strong tendency to reverse the facts of any situation to fit in with a preconception that any use of the language of the street was a serious danger to standard English in the classroom. A SEEK project in Brook-

FIGURE 3
Use of Black English Variables by Four Groups of Philadelphia Speakers by Index of Cross-Racial Contact
(adapted from Ash and Myhill 1983)



lyn was using the facts of black English to teach standard English writing through contrastive analysis; it produced a storm of protest from black leaders like Roy Wilkins and Bayard Rustin that it was a plot of white researchers and their "handkerchief-headed Negroes" to impose bad English on poor black children. As recently as 1979, reactions to the well-known Ann Arbor lawsuit (*Martin Luther King Junior Elementary School Children v. Ann Arbor School District Board*) were so hysterical that the facts of the matter were reversed in many national accounts: it was widely reported that a judge in Ann Arbor had instructed teachers to learn black English so that they could communicate with the children (an account of the public reaction to work on black English is given in Labov 1982b).

Today, there has been reasonably accurate reporting of the basic findings in the press (except for the linguistic details). Newspaper editorials and columnists have noted that this is a problem for all Americans, not just for blacks. They have interpreted the linguistic findings as evidence that the situation in the inner city will not improve if present policies continue, and that if Americans do not want to live in a permanently divided society, different social policies must be brought to bear. There has also been a negative side to the publicity: as my black colleagues pointed out, any time you discuss the black/white situation there are going to be people who point to the blacks as the problem. But most white columnists have seen that this is a problem for whites to deal with if they want to improve the quality of the society they live in.

I don't want to sound at all optimistic about the present racial situation in American society. These are grim, pessimistic findings. We don't see any immediate possibility of improvement. There has been very little progress made in reversing the pattern of educational failure since the 1960s, as far as I know. And we as linguists have yet to make a significant contribution to the school curriculum that will put our linguistic knowledge to use. However, in reaction to these findings, a large number of people in our society have said that linguistic information is important—and they have given us further encouragement in our efforts to put linguistic knowledge to work in improving the situation of our fellow citizens.

III. FAY BOYD VAUGHN-COOKE (*University of the District of Columbia and Center for Applied Linguistics*)

A major goal of almost all research scholars in linguistics, and other disciplines as well, is to disseminate their findings to as large an audience

as possible. While this goal is generally achieved by presenting papers at professional meetings and publishing books and articles in academic journals, a few fortunate scholars have had the exceptional opportunity to present the results of their research to millions of people through the national press. One such scholar is William Labov.

In the spring of 1985, the results of a three-year research project, funded by the National Science Foundation and directed by Labov, on "The Influence of Urban Minorities on Linguistic Change," were reported in over 157 domestic and foreign newspapers and on a number of television programs, including the CBS Evening News (Labov 1986). According to Labov, a major and important finding of this research is that black English, contrary to expectations, is becoming more different from standard English and other white dialects. William Stevens, in his widely circulated (1985) *New York Times* article entitled "Black and Standard English Held Diverging More," summarized Labov's position with the following direct quotes:

The results of our analyses show a Black English vernacular that is more remote from other dialects than has been reported before. . . . We also believe that Philadelphia reflects a national trend in the Black community toward continued linguistic divergence. The differences appear to us to be increasing. . . . There is evidence that, far from getting more similar, the Black vernacular is going its own way.

Labov's divergence claims were highlighted in other major newspapers. The *Baltimore Sun* (18 March 1985) reported: "The widening gap between 'black' and 'white' English is a bad symptom for the future." A reporter for the *San Juan Star* (19 May 1985) wrote: "A new study by a University of Pennsylvania linguist concludes that English spoken by blacks and whites in this country is diverging so rapidly that it may lead to 'a permanent division' between the races." The headline for the *Washington Post* article (2 April 1985) about Labov's research maintained: "Black-White Schism in Speech Seen Widening: Linguist Reports Dangerous Drift."

As these quotes reveal, Labov has made some very powerful and provocative claims about the direction of change in black English. His claims provoke serious and—depending on who's doing the asking—alarming questions about the language-learning capabilities of black people in this country. One question is this: Why are black people, after 400 years, moving backward as opposed to forward with respect to the acquisition of standard English? The question was also addressed by Taylor (1986) during a panel discussion which focused on Labov's research and the media at the Fifteenth Annual Howard University Communications Conference. Taylor suggested that persons who affect the

allocation of funds for the education of black children might be influenced negatively by Labov's claims. He said, "I would suggest that the recent media accounts provide all the evidence that one would need to say, 'Let's cut programs that will support education for black and poor children.'" More specifically, Taylor speculated further that a person involved in the funding process might say

look at the millions of dollars we spent in this country in the last fifteen or twenty years to improve the quality of education for Blacks, particularly in the language arts, and look, not only can they still not talk, Professor Labov in Pennsylvania shows they talk worse.

The point of Taylor's speculation was summed up as follows: "We should not lose sight of the implications of how the media can use data, whether correct or incorrect, to play in the hands of other, larger agendas."

Given the important social implications of Labov's claims, the enormously powerful medium used to disseminate them, and the alarming questions they provoke, Labov should have recognized that he had a special responsibility to present impenetrable and irrefutable evidence to support them.

The purpose of this paper is to challenge Labov's claims about linguistic change in black English. I will argue that he has not presented adequate evidence to support his claims and that, through extensive press coverage, he has disseminated erroneous information about a group of speakers to millions of people in this country and abroad. First, Labov's evidence will be evaluated, a critical task the press failed to undertake; then counterevidence to his claims will be presented. A discussion of alternative claims about the direction of language change in black English will follow the counterevidence, and the final section of the paper will focus on the implications of Labov's claims for education.

LABOV'S EVIDENCE. The evidence presented to support the claim that black English is diverging from standard English consists of the results of four studies (Labov and Harris 1983; Ash and Myhill 1983; Myhill and Harris, 1983; and Graff, Labov, and Harris, 1983) from Labov's research project, "The Influence of Urban Minorities on Linguistic Change." The title of Labov's introduction to these papers, "The Increasing Divergence of Black and White Vernaculars," highlights the major finding regarding black speakers, which was summarized as follows: the speech pattern of black residents of Philadelphia is "developing in its own direction, and becoming more different from the speech of whites in the same communities" (Labov 1985, 1). In the paper which

he has just presented at this conference, Labov also cites Bailey and Maynor (1985a) as supporting evidence for the divergence hypothesis.

Before examining the data and results presented in the studies to support Labov's findings, it is instructive to consider the kind of data required to study language change and, specifically, to establish the fact that one language variety is becoming less like another. For this requirement, I will turn to some of Labov's earlier and more programmatic investigations of linguistic change. Labov has published some of the most illuminating and penetrating works in this area in the past two decades (see Weinreich, Labov, and Herzog 1968; Labov, Yeager, and Steiner 1972; Labov 1972a, 1980; Sankoff and Labov 1985). Labov (1972a, 163) describes explicitly the data required for a study of language change:

The simplest data that will establish the existence of a linguistic change is a set of observations of two successive generations of speakers—generations of comparable social characteristics which represent stages in the evolution of the same speech community.

The most crucial characteristic of the kind of data Labov is referring to here is *TIME DEPTH*, an essential feature of any study of linguistic change (see Chen and Hsieh 1971, and Chen 1972, for detailed discussions of the time dimension in studies of language change). The following simple example illustrates the necessity and power of time depth in studies of linguistic change. If an analyst wants to determine whether the use of possessive /s/ has changed over time in a language variety spoken by a mother and her daughter, the analyst could calculate the frequency of occurrence of this feature for both speakers, who represent different points in time. If the mother's frequency shows that possessive /s/ is present forty percent of the time and the daughter's frequency shows that this feature is present ninety percent of the time, the analyst could draw a rather obvious conclusion: the relative frequency of the daughter's expression of possession is becoming less like her mother's. Only data from two points in time could reveal such specific facts about change.

Although Labov played a fundamental role in specifying the type of data required for studying language change, the data presented to support his claims about change in Philadelphia black English fail to meet his own most basic methodological requirement. As the following summaries of the evidence will show, Labov and his colleagues' data violate the critical time depth principle: their data represent only one point in time. Thus no conclusive claims about change in black English could be made.

The Labov and Harris study. The major goal of this study, "De Facto Segregation of Black and White Vernaculars," was "to plot . . . linguistic data against . . . social networks, to see if . . . close associations account for similarities and differences in linguistic behavior" (Labov and Harris 1983, 9). This was done by examining the distribution of two grammatical features, third singular and possessive /s/, in the language of thirty-four black, Puerto Rican, and white speakers, subgrouped, not according to age, but according to their social networks. The subgroups include the Old Heads, Young Bloods, Activists, Musicians, Puerto Ricans, and Senior Citizens. According to Labov and Harris, these groups reflect "primary peer relations—people who see each other almost every day; family relations—kin and people who live in the same household; and secondary relations—people who know each other, but whose connection is maintained primarily through a third person" (7).

Results showed little correlation between the distribution of speakers within social networks and their use of the two grammatical markers. Labov and Harris concluded that while social networks are useful organizing units in the field work for locating speakers and obtaining recordings of normal social interactions, such networks have little explanatory value for individual differences in linguistic systems. In order to account for such differences, Labov and Harris proposed (10–12) that the social histories of speakers (as opposed to social networks) must be taken into account. The kinds of social experiences speakers have had in dealing with members of other groups and the way they have used language in their lives must be considered. When the speakers were rearranged according to their social histories, a correlation between the new subgroupings and linguistic behavior (the use of the two grammatical features) was observed.

The Ash and Myhill study. The goal of this paper, "Linguistic Correlates of Inter-Ethnic Contact," was to investigate "the correlation between selected linguistic features and the ethnic group affiliation of a speaker" (2). Questionnaire and spontaneous data on lexical, grammatical and phonological variables were analyzed from five white, nineteen black, and three Puerto Rican speakers. A significant finding, according to these investigators, is that "blacks who move in white circles show a major shift in their grammar in the direction of the white norm and a lesser shift in their phonology and lexicon" (16). With respect to their white speakers, the researchers found that (14)

for the phonological and lexical variables, whites who move in black circles show considerable variation in the extent to which they match blacks, but for each of

them there is good agreement with characteristics of the Black Vernacular on some points.

The Myhill and Harris study. The goal of this study was to examine the function of the verbal -s inflection in the speech of five vernacular black English (VBE) speakers. In the standard language, this inflection marks agreement between a third person singular subject and a present tense verb; however, for the VBE speakers in Myhill and Harris's study, results revealed that verbal -s has three functions. First, it marks agreement with third person subjects. The researchers note that the use of this function will be affected by the speakers' level of mastery of the standard language. Second, the /s/ inflection serves a social function. According to Myhill and Harris, this inflection may be used as a stylistic device to make one's speech more suitable for interaction with standard speakers. Third, /s/ functions as a marker of narrative clauses, regardless of the person and number of the subject.

The Graff, Labov, and Harris study. This paper, "Testing Listeners' Reaction to Phonological Markers of Ethnic Identity: A New Method for Sociolinguistic Research," tested the hypothesis that a difference in the nucleus of two vowels, /aw/ and /ow/, was sufficient to signal a distinction between black and white speech. The goal of the paper "was simply to determine whether the positions of . . . vowel nuclei could serve as cues for listeners to judge the ethnicity of a speaker" (6). A set of stimuli was recorded on a cassette tape and played for seventy listeners (thirty-five blacks, eighteen whites, and seventeen Puerto Ricans). Results of the analysis of listeners' responses to /aw/ were reported as follows (14):

The responses of both blacks and whites demonstrate that a difference in the position of the nucleus of /aw/, between front (tense) and central (lax), is a prominent cue to the ethnic identity and affiliation of a speaker among the members of this [Philadelphia] community. For this sample of listeners, not only does a fronted /aw/ yield speech that sounds significantly more white, but it is significantly less likely that a black would ever produce it.

Response to /ow/ produced different results (14):

The status of /ow/ is a rather different matter. There was a general, though not significant, tendency for the fronted version to be judged as sounding more white: among both white and black listeners, a simple majority shifted their scaling judgments in this direction.

Critique. Examination of the goals, data, and results of the four studies presented as evidence for Labov's claims reveals a deeply disturbing paradox: Labov has attempted to support his claims about the direction of

change in black English with studies which have not investigated change. Not one of the four studies summarized above was guided by a goal which focused on language change. The focus of each of the studies was stated clearly and concisely. No one (and certainly not Labov) should be confused about the goals of these investigations; they are totally unrelated to language change.

Given the goals of the four studies, one would not expect the data to be appropriate for a study of language change, and they are not. Specifically, the data in none of the studies exhibit time depth. They violate the basic methodological principle Labov taught us about studying language change and that he adhered to in his earlier works on change.

Consider the primary subject base for each of the studies. The Ash and Myhill study indicates the ages of the five whites studied (22–33); specific ages for the nineteen blacks and three Puerto Ricans were not reported. The Graff, Labov, and Harris study does not report the ages of any of the seventy listeners. The Myhill and Harris study analyzed data from five speakers; again, none of their ages were reported. Labov and Harris reported age levels for twelve of their speakers: seven were between 20 and 45 and five between 16 and 20. The remaining twenty-two speakers included senior citizens, but no specifics regarding their ages are provided. While it is possible to determine that some speakers in the Labov and Harris study span a number of age levels, the data were not organized to reveal change in language over time or across two successive generations. In short, the four studies shared one common characteristic: none was concerned with language change.

But do the conclusions of any of the four studies support the divergence hypothesis in any way? Labov and Harris showed that there is little correlation between the distribution of the speakers within social networks and their use of grammatical markers. Ash and Myhill's results indicated that blacks who move in white circles show shifts in features of their language toward the white norm, while whites who move in black circles show considerable variation in the extent to which they shift toward the black norm. Myhill and Harris's findings indicate that the verbal *-s* inflection has three different functions for VBE speakers, and Graff, Labov, and Harris revealed that differences in the position of vowel nuclei provide cues to listeners when judging the ethnic identity and affiliation of a speaker. These results are clear, straightforward, and interesting and noteworthy in their own right. However, they cannot be used to support claims about change in language. Instead, they expose Labov's seriously flawed and misguided approach to the study of linguistic change in black English, a subject to which I will return shortly

(20–21 below). First, however, I would like to examine an additional study, Bailey and Maynor (1985a), which was not part of Labov's research project, but which Labov has asserted (1986; above 5–12) provides independent corroborative evidence for the divergence hypothesis. For while Bailey and Maynor did at least perform a time-based study, their results are far from conclusive.

The Bailey and Maynor study. The central claim of this paper ("De-creolization?") is precisely the same as that made by Labov. In the authors' words, "BEV is becoming less like, not more like, white varieties of English" (2). This conclusion was drawn from an analysis of invariant *be* in the speech of seven adults and twenty children, all natives of the Brazos Valley in east-central Texas. The adults were all over 70 and the children were all 12 or 13 years old. The results of Bailey and Maynor's analysis revealed a major quantitative difference between the speech of children and the older adults. Invariant *be* occurred about three times as often in the speech of the children. Additionally, analysis of the syntactic constraints for this feature revealed another striking difference between older speakers and children. Invariant *be* occurred far more frequently before verbs than before other syntactic constraints (e.g., *gonna*, predicate adjectives, predicate locatives, and noun phrases). For example, in the first person singular environment, invariant *be* accounts for over a third of the data before verbs in the speech of children, but only one-tenth of the tokens in the speech of the adults. Bailey and Maynor provided the following explanation (36):

Although both [older] speakers and children use [invariant *be*] as an occasional variant of the copula, the children also use it as a systematic variant of auxiliary *be*, contrasting with \emptyset , *is*, and *are* to mark a habitual/durative aspect. This distinction . . . suggests that black speech is diverging from, not converging toward, white speech.

In spite of the differences Bailey and Maynor observed between adolescent and adult speech, their data set cannot be used to make claims about the direction of change in VBE, for it exhibits a crucial gap that precludes the study of the propagation of linguistic features across successive generations. Specifically, Bailey and Maynor's data include very old and very young speakers, but no middle-aged speakers; therefore, their data set cannot reveal whether the structural differences observed in the speech of children represent a recent innovation that will spread to subsequent generations, as Bailey and Maynor suggest, or an example of age-grading. Previous analysis of invariant *be* in three independent investigations (Labov, Cohen, Robins, and Lewis 1968; Wolfram 1969;

and Fasold 1972) provide no evidence for linguistic innovation, but they provide compelling evidence for age-grading. Consider Labov et al.'s conclusions, which were based on an analysis of invariant *be* in the speech of New York City children (235):

It . . . appears that [invariant *be*] is heavily age-graded. We encounter [invariant *be*] in frequent use among pre-adolescents and adolescents in every ghetto area, including Los Angeles, Chicago and Cleveland. But adults rarely use [invariant *be*]. We have only a few examples in our adult material . . . and in those few, the speaker is talking about an adolescent experience. This is not the case with many other NNE features, and a number of our adults from the South represent the furthest departure from the dialects with which we are familiar.

It is not clear why Labov cites Bailey and Maynor's study on invariant *be* as evidence for language change in black English when his own study of this same feature provided such convincing evidence for age-grading as opposed to linguistic divergence.

Wolfram's (1969) investigation of lower-working-class speakers in Detroit also revealed frequent use of invariant *be* by children and infrequent use by adults: a mean number of occurrences of 16.3 for children age 10–12, dropping to 12.8 for the 14–17 age group and 3.8 for adults (201). Fasold (1972, 214) observed a similar pattern in Washington, D.C. Although his informants showed a lower mean number of occurrences than did Wolfram's (4.7 for ages 10–12, 3.1 for ages 13–19, and 2.7 for adults), Fasold found that a far greater percentage of Washington children used invariant *be* than did adults (85.9%, 68.8%, and 25% respectively for the three groups).

In sum, data from three studies in different geographical areas strongly suggest that invariant *be* in the speech of adolescents is an example of age-grading, rather than a linguistic innovation which is causing black English to diverge from standard English and other varieties of white speech. Bailey and Maynor's results cannot be used as evidence for the divergence hypothesis until the age-grading issue is resolved.

Clearly, what is needed are the missing data on middle-aged speakers to fill the critical gap in the data set, and until such data are provided, Labov must search elsewhere for evidence to support the divergence hypothesis. (See Wolfram's contribution below for additional discussion of this point.)

Labov's Approach. I return now to what I have characterized above as Labov's "flawed and misguided" approach to the study of linguistic change in black English. Specifically, the four studies from his research project show that Labov formulated his divergence hypothesis but NEVER TESTED IT! Labov's untested hypothesis was then boldly disseminated as a

finding, a fact about language change. The magnitude of this flaw in Labov's approach is incomprehensible. I simply cannot understand how a rigorous methodologist like Labov could blatantly skip hypothesis-testing—one of the most elementary (but absolutely essential) steps in the scientific method. Labov's approach to the study of language change in black English represents an abrupt departure from the exemplary carefulness, cautiousness, and impressive rigor that has characterized his prior, extensive research in general, and his work on language change in particular.

It is also important to point out that other scholars, noted researchers in the field, have also criticized Labov's flawed approach. The papers of Wolfram (40–48) and Rickford (55–62) presented at this panel are critical on this score. Frederick Spahr, executive director of the American Speech-Language-Hearing Association (a fifty-thousand-member organization for speech-language pathologists and audiologists), in response to Labov's discussion of the divergence hypothesis on the CBS Evening News, wrote a letter (12 December 1985) to CBS which expressed his outrage and requested an opportunity to challenge Labov's claims. In part, this correspondence read:

The interview featured the research findings of William Labov, who claims that there is a diverging communication gap between Blacks and Whites. I am concerned that the [news story] . . . did not expose the methodological flaws in Labov's research and . . . allowed erroneous conclusions to go unchallenged. . . . I strongly urge that you not permit such a one-sided presentation of this issue to go unchallenged. There are many respected linguists and speech-language pathologists among our membership and our National Office Staff who would be happy to describe in detail Labov's research limitations and the potential damage that can result from his erroneous claims.

Professor Orlando Taylor, Acting Dean of the Howard University School of Communications, sent a similar outraged letter to CBS.

COUNTEREVIDENCE TO LABOV'S CLAIMS. This section will focus on another basic flaw in Labov's approach to the study of linguistic change in black English, namely his failure to review the literature relevant to his topic. Specifically, before constructing his hypothesis Labov neglected to examine critical results from cross-generational studies which have investigated linguistic change in black English. I will discuss here Anshen (1969), Vaughn-Cooke (1976) and Nichols (1983, 1986); further counterevidence may be found in Butters and Nix (1986), and Butters (1986).

The Vaughn-Cooke Study. This study (Vaughn-Cooke 1976; see also 1986) investigated variation that represents a phonological change in progress which is affecting the pronunciation of words containing initial

unstressed syllables (e.g., *afraid*, *across*, *electric*, and *America*) in the speech of three generations of forty working-class black speakers in Franklin County, Mississippi. Examination of the data revealed that all of the speakers pronounced words like *afraid* and *electric*, which exhibit initial unstressed syllables, WITH and WITHOUT the initial syllable. It was observed, for example, that some speakers said 'fraid as well as *afraid* and 'lectric as well as *electric*. Vaughn-Cooke hypothesized that the variation in the pronunciation of these words represented a phonological change in progress, and, following Labov's requirement for studying language change, she utilized data from three generations of speakers to test this hypothesis.

Her data support her hypothesis. Elderly speakers (ages 60–92) produced unstressed initial syllables only fifteen percent of the time (43/286). Middle-aged speakers (40–59), however, doubled this percentage and produced unstressed variants of syllables more than thirty percent of the time (165/542). The young speakers (8–20) exhibit another major increase; they produced unstressed variants almost forty-eight percent of the time (276/581). This pattern was observed not only for classes of words but also for individual lexical items within word classes (see Vaughn-Cooke 1986, 120). The direction of change in the data is unmistakable: IT IS TOWARD STANDARD ENGLISH, RATHER THAN AWAY FROM IT. The frequency of the standard variant is increasing over time. Based on these data, one would have to argue that black English is CONVERGING upon the standard variety rather than diverging from it. Labov fails to consider this powerful counterevidence to his divergence hypothesis.

The Anshen Study. Anshen (1969) examined variation which provides evidence for a phonological change in progress that is affecting the pronunciation of words containing postvocalic /r/ in the speech of three generations of black speakers in Hillsborough, North Carolina. Like Vaughn-Cooke's data, Anshen's data on postvocalic /r/ revealed that this feature is converging toward standard English rather than diverging from it. While all of Anshen's eighty-seven speakers used a high percentage of nonstandard variants (\emptyset and [ə]) for /r/ (80.5% for the youngest speakers in word lists, up to 96.1% for the oldest speakers in interview style), the pronunciation of /r/ by the youngest speakers (19–39) contained a higher percentage of the standard variant [r] than either the middle-aged (40–54) or oldest speakers (55+). The differences between the percentages of occurrence of [r] for the young and old speakers were statistically significant for all three styles; for the middle-aged and old speakers, differences were statistically significant for two styles, interview and word list. Though differences between the young and middle-aged

speakers' use of the standard variant were not statistically significant, overall, their percentages conform to the pattern of convergence toward standard English. Anshen concluded the following regarding his finding: "Among Negro speakers in Hillsborough, a sound change with respect to /r/ has taken place within the lifetime of the oldest speakers" (1969, 101). The evidence doesn't show that this change is complete, but the direction of change is again unmistakable: it is toward standard English.

The Nichols Studies. Nichols (1983) reported evidence for a syntactic change in progress in the pronominal systems of thirteen Georgetown County, South Carolina black speakers representing three age groups: 15–25, 25–65, and 65–90. Examination of their pronominal systems revealed variable or categorical usage of *it*, the third person neuter pronouns in subject and object positions in a sentence. The majority of Nichols's speakers used the older, nonstandard, creole Gullah forms *ee* [i] and *um* [ʌm], as opposed to the innovative standard variant *it* in subject and object positions, respectively. For example, in subject position Nichols reports (210), *And ee was foggy, and they couldn't see* [Speaker 1B] 'and it was foggy, and they couldn't see'. In object position, one finds examples such as "'cause I put um [the clock] on the time" [Speaker 2B] 'because I put it on the time'.

Based on their use of the five different variants (*hit*, *ee*, *he*, and *it* in subject position and *hit*, *ee*, *um*, and *it* in object position) of the third person neuter pronoun in subject and object positions, Nichols divided the speakers into two groups, or lects—"groups of speakers who show similar linguistic variants" (Nichols 1986, 79). Speakers in Lect 1 use both standard *it* and nonstandard *ee* in the subject position; in the object position, three variants were used by these speakers: standard *it* and nonstandard *ee* and *um*. Note that the variants for speakers in Lect 2 contrast sharply with those in Lect 1—the standard variant *it* is used categorically in both subject and object positions. Another feature of the data is noteworthy: seven of the ten Lect 1 speakers are above age 65, while all of the speakers in Lect 2 are middle-aged or young. According to Nichols, the variable use of standard and nonstandard pronominal forms observed for the older speakers and the categorical use of standard forms observed for young and middle-aged speakers in Lect 2 provide evidence that the language spoken by blacks in Georgetown County, South Carolina is converging toward the regional standard. As she puts it (211):

The standard form *it* is used categorically by three . . . black speakers. . . . They represent the wave of the future as long as [they] have the expanded educational

and job opportunities now becoming available to the younger generation. Their speech I would characterize as regional standard English. [Their] lect . . . reflects the direction of change for . . . black . . . speech in this region.

Nichols' pronoun study represents a third piece of counterevidence to the divergence hypothesis.

Nichols' (1986) investigation of the language of her Georgetown County, South Carolina speakers provided evidence for a second syntactic change in progress. Her examination of an aspect of the prepositional systems of sixteen young, old, and middle-aged speakers revealed three variants—standard *at* and nonstandard *to* and \emptyset (the null form)—for the preposition *at*, as in the following examples (78–80):

Some of um stay to the schoolhouse 'Some of them stay at the schoolhouse'.

I just work for one place; I had work \emptyset Conway Lumber Company 'I just work for one place; I had worked at Conway Lumber Company'.

John is at his office.

Nichols again divided the speakers into lects, based on their use of the three variants *to*, \emptyset , and *at*. As was the case in her pronominal study, the results show that a majority of the twelve speakers (two-thirds) who used the nonstandard forms *to* and \emptyset were between 65 and 90. One of the elderly speakers used the nonstandard variant *to* categorically—no alternation with standard *at* was observed in her speech. The results repeat an important finding of the pronoun study: categorical use of the standard variant was observed for middle-aged and young speakers only. Nichols concluded, "Older speakers in [the community] tend to show the variation between *to* and *at*, while the young and middle-aged are moving toward the categorical use of standard *at*" (83). Finally, Nichols pointed out that her preposition data provide "further evidence that [the speakers] are moving toward a regional standard English" (78). Nichols (1986) contains yet another piece of counterevidence for the divergence hypothesis.

The Bailey and Bassett Study. Bailey and Bassett's (1986) data on invariant *be* from three areas in the South—East Louisiana, Gulf Mississippi, and Lower Mississippi—point to another change which is resulting in the use of a nonstandard feature by fewer young speakers. Summarizing their findings, Bailey and Bassett noted (162), "While blacks of all ages . . . use unconjugated *be*, the form is most common in the speech of older people, with twenty-three of twenty-nine informants over age 65 (79%) using it. Only four of ten (40%) of those under 65 use the form." (Their data on invariant *be* do not reveal the frequency of occurrence of

this feature in the speech of their informants; as a result, we cannot determine whether the adolescent[s] in this study exhibit the high occurrence of invariant *be* reported in Labov, Cohen, Robbins, and Lewis 1968, Wolfram 1969, and Fasold 1972.) Data on this feature, like those on the preceding ones, provide evidence for convergence toward standard English.

The evidence against the divergence hypothesis is convincing and compelling. Such evidence would force almost any scientist to reject this hypothesis as an explanation of change in black English and formulate a new one, but first, one would have to know about the counterevidence and take it into account. The fact that this evidence comes from the southern part of the United States, as opposed to a large northern city, does not exempt the divergence hypothesis from accounting for it, for this hypothesis was advanced as an explanation of linguistic change in the English spoken by blacks all over the United States. For example, reporter Juan Williams of the *Washington Post* began his article about Labov's divergence claims with the following sentence: "English spoken by blacks and whites in the United States is becoming increasingly different. . . ." The lead sentence in the *New York Times* article by William Stevens read, in part, "Contrary to expectations, the form of English spoken by many black Americans is becoming more different from standard English rather than more like it. . . ." And Ash and Myhill (1983), one of the four project reports circulated by Labov, contains this inclusive statement (1-2): "Findings of the present research project indicate that the Black English Vernacular (BEV) is becoming progressively more different from the white standard language." As these examples show, the references to English spoken by working-class blacks in this country by the press and Labov's research project are very inclusive: they refer to blacks in the North, South, East, and West.

In sum, the divergence hypothesis provides a good illustration of the necessity of reviewing relevant literature prior to and during hypothesis formulation. This basic step in the research process must not be skipped by anyone, inexperienced beginners or seasoned authorities.

THE FORMULATION OF A MORE TENABLE HYPOTHESIS. The studies examined in the previous section show that the divergence hypothesis cannot provide a comprehensive account of linguistic change in black English. Indeed, the available evidence suggests precisely the opposite hypothesis, that is, that black English is converging toward standard English.

This **FIRST FORMULATION** of an alternative hypothesis must be examined in the light of three other important considerations, the linguistic

history of black English, the history of the social setting in which black English is spoken, and the directionality of change during language contact.

The Linguistic History. Given the history of black English, should one expect this language variety to be converging toward standard English or diverging from it? Black English has a creole history, and, while there has been some debate about this fact, the extensive evidence to support it is now fairly widely accepted (Stewart 1968, Dalby 1971, Dillard 1972, Rickford 1977).

The evidence shows that black English originated as a pidgin, was later creolized, and subsequently began the process of decreolization; that is, structurally it began to shift toward standard English. Weinreich, Labov, and Herzog (1968) and Labov (1972a) have noted in their discussions of the actuation of linguistic changes that sometimes it is difficult to isolate the forces which cause a language to undergo change or to shift toward another. However, this is not the case for black English, because the disruption of the international social order which set the changes in motion observed in this language is rather well known today. Here I am referring to the fact that four centuries ago, thousands of African people were abruptly removed from their native linguistic and cultural settings and placed in foreign settings. Along with their removal came the responsibility of social and linguistic adjustment. The linguistic adjustment involved acquiring by informal means the local varieties of English, the native language of the more socially, politically, and economically powerful white inhabitants of the foreign setting. The initial social pressure created by the need to communicate not only with the socially powerful white speakers but also with African speakers from other linguistic communities led to the processes of pidginization, creolization, and finally decreolization. Nichols' data on prepositions and pronouns cited above provide examples of this later process. Recall that her young and middle-aged speakers are replacing creole forms with regional standard variants. Such is the result of decreolization, a powerful mechanism of linguistic change.

Given these facts about the linguistic history of black English, the prevailing expectation would appear to be that black English should be converging toward standard varieties rather than diverging from them. This is why reports of Labov's findings have noted that the claims of the divergence hypothesis are "contrary to expectations" (Stevens 1985) about the direction of change in black English.

Given the evidence from the linguistic history of black English, the first formulation of the convergence hypothesis seems reasonable.

The History of the Social Setting in Which Black English Is Spoken.

Linguistic and social factors are closely interrelated in the development of language change. Explanations which are confined to one or the other aspect, no matter how well constructed, will fail to account for the rich body of regularities that can be observed in empirical studies of language behavior. [Weinreich, Labov, and Herzog 1968, 188]

This was the final principle and closing statement of the penetrating and programmatic article "Foundations for a Theory of Language Change." This principle, though ignored by Labov in his approach to the study of language change in black English, is particularly relevant to the formulation of a hypothesis about change in this dialect, because of its very turbulent social history. One aspect of that history which has had a profound effect on language is the initial absence of formal education for blacks, and later (1872), the institution of free educational systems (Cook and Doll 1973). The data which provide evidence for convergence show consistently that the better-educated, younger speakers use more standard forms than the less-educated, older speakers. We can return to Anshen's (1969) data on postvocalic *r* to illustrate this point.

Anshen found (64) that *r*-retention scores increased as level of education increased for the young and middle-aged speakers. This pattern was observed across three educational levels (grades 0–6, 7–11, and high school graduates) and three speech styles (interview, word list, and sentence list). Note, however, that this pattern was not observed for older speakers (age 55+) who exhibit the lowest *r*-retention scores, the lowest percentage (15%) of high school graduates, and the highest percentage (55%) of speakers at the lowest grade level. The overall low level of education of the oldest speakers had a dramatic effect on their speech, as exemplified by their low *r*-scores, and conversely, the effect of the relatively high level of education of the younger speakers is exemplified by their higher *r*-scores. Anshen (1970, 1) offered the following explanation:

Something happened that caused the schools to instill /r/ as a "correct" pronunciation for postvocalic *r*. . . . It is reasonable to blame the schools for this change, since . . . those with little schooling have not had their speech affected by this change.

Anshen also explained, "The teachers I spoke with and the principal of the Negro high school all agreed that the schools attempted to correct their pupils' speech" (Anshen 1969, 38).

The effect of education on language has been demonstrated in a number of other studies, specifically those which have subgrouped speakers according to social class (Wolfram 1969, Labov 1972a, Fasold

1972, Vaughn-Cooke 1976, Baugh 1979, and Rickford 1979). These works show that higher-educated, middle-class speakers, unlike their less-educated, working-class counterparts, exhibit a preference for standard speech forms.

When the effects of education upon the speech of blacks is considered, together with the increased educational opportunity over time, one would expect the English spoken by blacks in the United States to be converging toward standard English. Our first formulation of the convergence hypothesis has been further sustained.

The Directionality of Change During Language Contact. It is important to recognize that data discussed above from cross-generational studies encompass only five linguistic features, two phonological and three syntactic. This is a very restricted set of features; ultimately, we must account for ALL of the changes in progress in black English, not just the handful on which data are currently available. Such an ambitious goal increases the challenge of constructing a tenable hypothesis. In order to raise the probability of achieving this goal, the following must be considered: a complex interaction of factors, social and linguistic, can affect the direction of changes, such that a feature undergoing change might reverse its course or exhibit a course that is totally unexpected. Janson's (1977) Swedish data from Stockholm speakers provide an example of the former possibility, while Anshen's Hillsborough data illustrate the latter.

Janson's investigation of the deletion of final *-d* in Stockholm Swedish documents a sound change that reversed its direction. By analyzing data from word lists and spontaneous speech, Janson found that Stockholmers usually delete the final *-d* of words like *ved* 'wood', *hund* 'dog', *blad* 'leaf', and *rod* 'red' in ordinary speech. A relevant characteristic of this deletion process is that the class of words that can undergo optional *-d* deletion is now much smaller than it was a half-century ago. It appears that final *-d* was disappearing in some dialects of Swedish as early as the fourteenth century. For Stockholm speakers, the deletion of final *-d* occurred for many words and across several grammatical categories. This change gained ground until the seventeenth century, when, according to Janson, *-d* began reappearing in the spoken language. The investigator provided this interpretation of the data: "The diffusion [of the change] is obviously operating in reverse: instead of gaining new words with the passage of time, the rule of *-d* deletion appears to cover a smaller sector of the lexicon now than two centuries ago" (260). Such data cannot be ignored when constructing a hypothesis about the directionality of change in black English or any other language.

I turn now to Anshen's study (1969). Utilizing the same data employed for the analysis of postvocalic *r*, Anshen investigated the effect of age on the frequency of occurrence of the standard form, voiceless *-th* (/θ/), in words like *both* and *month*, where *-th* appears word-finally. In addition to producing *-th* at the end of words, Anshen's informants also produced the nonstandard variants /t/ (as in /mʌnt/ for *month*) and /f/ (as in /mawf/ for *mouth*). Surprisingly, Anshen found a statistically significant increase in the percentage of the nonstandard variants for young speakers (age 19–39) as compared to older speakers (55+ years) for all three speech styles, and an increased use of nonstandard variants for young compared to middle-aged (40–54) speakers in interview style. In other words, for this particular linguistic feature, Anshen appears to find divergence, NOT convergence.

Why is this happening? Anshen offered this implausible explanation: "older people have had a greater amount of exposure to standard forms and this greater exposure has enabled them to master standard forms to a greater degree than have younger people" (36–37).

This explanation ignores the fact that mere exposure to standard forms, without active teaching (like that received in school) and critical linguistic reinforcement (in the form of community models), will not necessarily lead to the production of such forms, even when they can be comprehended fully. For quite some time now, studies have shown that the relationship between a speaker's comprehension and production of linguistic form is often asymmetrical (e.g., Ervin-Tripp 1964).

In spite of the absence of a plausible explanation for the unexpected directionality of the *-th* variable, these data require a reformulation of the convergence hypothesis. While the majority of changes examined support the convergence hypothesis, it is now evident that a change can assume the opposite course—it can diverge from standard English. Thus the following revised, two-part hypothesis is put forth: the majority of features undergoing change in black English are converging toward standard English; powerful social and linguistic counterforces can reverse the expected direction of a change. This more fruitful hypothesis does not limit the complex process of linguistic change to a unidirectional course; thus it is likely to yield a more coherent account of this phenomenon in black English. (For further support for this point, see Wolfram's paraphrase of Rickford's comment on convergence and divergence as a function of changes in sociohistorical conditions, p. 41 below.)

LABOV'S COROLLARY CLAIM. The preceding sections exposed the major problems of Labov's approach to the study of linguistic change in black

English, namely the formulation of an untenable hypothesis, the failure to test this hypothesis, and the injudicious dissemination of its unsupported claims to the general public. This section will focus on an additional problem: the dissemination of an unsupported corollary claim (inferred from the divergence hypothesis) about the education of black children. Labov asserted that the supposed widening gap between black English and standard varieties is certain to cause educational problems, in particular reading problems. The closing statement of Labov and Harris (1983) expresses this view:

The main result of our present research is clear. Young black children from the inner city, who must deal with the language of the classroom, are faced with the task of understanding a form of language that is increasingly different from their own. [26]

Labov speculates that such an increasing difference might lead to reading problems: "We're not ruling out the possibility that it is contributing to failure of black children to learn to read. How much a little child has to do to translate!" (quoted in Stephens 1985).

This alarmist tone is echoed in a number of other major newspapers. The *Baltimore Sun* reported: "Dr. Labov believes that the language division is creating ENORMOUS DIFFICULTIES [emphasis added] for black children who enter school without much previous contact with whites." In the *Washington Post*, Williams (1985) stated, "Labov suggested that the key impact of the differences in language is that black children increasingly are at a disadvantage when they begin school. Their teachers speak a different dialect and their books are written in a different dialect." A final quote comes from the *San Juan Star*, which reported: "Inner city Blacks begin school with a SERIOUS DISADVANTAGE [emphasis added] because they are faced with the task of understanding a form of language that is increasingly different from their own, says William Labov."

Undoubtedly, Labov's reputation as an authority and his professional affiliation provided sufficient evidence for reporters, naive about language research, to disseminate, confidently, his unsupported claims. None of the research reports which Labov disseminated in the academic community contain the evidence needed to support such claims. In fact, there is reasonable counterevidence.

Such evidence comes from a two-year study on language diversity and classroom discourse conducted by Lucas and Borders (in press). The goal of their research was to reexamine "dialect interference through a description and analysis of language functions in elementary school classrooms in which children are dialect speakers" (3). This was done by

videotaping, audiotaping, and observing a wide variety of events in kindergarten, fourth, and sixth grade classrooms in a public school in Washington, D.C. Events included whole-group lessons, small groups with and without the teacher (both of an academic and nonacademic nature), and one-on-one interaction. Lucas and Borders reported the following major finding:

Our study demonstrated that while the issue of interference [resulting from dialect differences] may be fundamentally a language issue, it is clearly not an issue of the production and comprehension of language forms. The production of dialect forms did not impede interaction in the classrooms that we studied. To the contrary, our examination of dialect diversity revealed that the children had a fairly sophisticated sociolinguistic competence. Specifically, they clearly demonstrated awareness of and capacity for situationally appropriate language use. Furthermore, there was no conversational repair work relating to comprehension of language forms.

Lucas and Borders' results indicate that researchers must look beyond language for the cause of black children's educational problems. Fasold (1981) made the same claim. And Labov (1982a) adopted this position in "Competing Values in the Inner-City Schools." In that insightful paper, he wrote (150):

The linguistic analysis that we carried out in South Harlem in the late 1960's convinced us that the Black English Vernacular was more different from school-room English than any other dialect; but no matter how great those differences might be, it did not seem possible that they accounted for the massive reading failure that we were witnessing. THE ETHNOGRAPHIC SIDE OF OUR WORK POINTED TO OTHER CAUSES [emphasis added].

Labov offers no explanation for his abandonment of the above position and his adoption of the alarmist, unsupported claims reported by the media.

LABOV'S SOLUTION TO THE DIVERGENCE PROBLEM. Labov and Harris (1983) offer a solution to the divergence problem; however, its underlying premise is also not supported by available evidence. The solution is this: black children should have more contact with whites in order to learn the local variety of English. Labov and Harris predicted, "If the contact is a friendly one, and we achieve true integration in the schools, the two groups may actually exchange socially significant symbols, and black children will begin to use the local vernacular of the white community" (25).

I assume the vernacular that Labov and Harris are referring to is a standard one. If this is the case, their proposal implies that standard English must be learned from white speakers. Taylor's comment (quoted in Banks 1986) is relevant here: "[Labov] implies that the only way to

learn standard English is from white speakers, that standard English is 'white English.' That is not so. One can learn it in an all-black environment, from black speakers. It is the language of education, not the language of whites."

The development and successful implementation of a method for teaching standard English to nonstandard black speakers is an enormously complex task, which must involve consideration of not only linguistic issues, but social and political ones as well. A method which relies on black and white friendly contact seems unrealistic, given the history of race relations in this country.

CONCLUSION. I've argued that the divergence hypothesis advanced by Labov and his colleagues is uninformed and simplistic, and that it cannot provide a coherent account of language change in black English. Moreover, the data presented as evidence for this hypothesis are inappropriate. They lack time depth and are thus incapable of revealing facts about the details of linguistic change. A problem, however, more serious than the simplicity of the divergence hypothesis and the absence of adequate evidence to support it is that this hypothesis was disseminated as a FINDING to millions of people through the national press. The public was misinformed, and this error should be corrected. Labov and his colleagues should either present adequate evidence to support their claims or they should retract them.

IV. GUY BAILEY (*Texas A&M University*)

My work on the present tense of *be* in the black English vernacular actually began in 1981 when I read a paper on invariant *be* in the lower South at a conference organized by Michael Montgomery. Using data from the Linguistic Atlas of the Gulf States (LAGS), Marvin Bassett and I looked at the occurrence of invariant *be* in the speech of blacks and whites of comparable age and social class in East Louisiana and Lower Mississippi, with the latter area including those communities where Walt Wolfram (1974) had studied the feature in the speech of children (Bailey and Bassett 1986). Our findings, however, were quite different from Wolfram's. He found that while invariant *be* resulting from *will/would* deletion occurred in the speech of Southern whites, "distributive *be*" (what I will call *be*₂) did not. The LAGS data, on the other hand, provided clear examples of *be*₂ in the speech of some Southern whites, as in *My biscuits is hard. They don't be spongy like hers;* and *Like when people be together, they understand one another.* Although the form is limited to the

most insular white varieties, our research showed that it is unquestionably a part of Southern white folk speech. My work in Texas provides further confirmation (see the appendix, 39–40 below). As I began to reexamine our work and Wolfram's in order to account for the discrepancies in the results, I could find only one striking difference—the age of the informants. Wolfram's study concentrated on the speech of children, ours on the speech of adults, most of whom were over sixty-five. When I went to Texas A&M in 1982, I decided to explore the possibility that age differences might be important in black-white speech relations and to look at the historical development of those relationships. In order to gather data to answer these questions, Natalie Maynor and I did a series of studies of black and white vernaculars in Texas and Mississippi (Bailey and Maynor 1985a, 1985b, 1985c); what I am presenting here are the results of those studies.

Because the questions of age-related differences in black-white speech relationships and of their historical development are embedded in the distinctive social history of blacks, we tried to collect data which would reflect that history. In the United States the social history of blacks comprises three broad phases. The period of slavery, the first and longest of these, involved the importation of blacks, primarily into the South, and their implantation into a rural agrarian society. This period was characterized by forced illiteracy and the limited social and geographic mobility which generally accompanies bondage. The second phase began with emancipation and lasted only about fifty years. While blacks were freed politically, they remained tied economically to the same rural southern agrarian society through the tenantry system. Thus in 1890 almost ninety percent of the black population still lived in the South, with over three-quarters of it in rural areas. The third period, which began with the advent of the First World War, has seen a massive migration of the black population out of the rural South and into the urban North. As a response to labor shortages created by the draft in northern cities, the decimation of the cotton crop in the South, and the oppression of Jim Crow laws, this Great Migration represents the largest demographic shift in U.S. history. In 1970, almost half of the black population lived outside of the South, with thirty-four percent of all blacks living in seven urban areas. In fact, by 1976 sixty percent of the black population was concentrated in inner cities.

Our data come from informants who represent each of these phases. These informants include a group of lower-class children, ages eleven through thirteen, who live in Bryan, Texas, a city of about 50,000 which serves as the major urban area of the Brazos Valley. They represent the

phase of urbanization. A group of tenant farmers and the wives of tenants represent the second phase. All of these informants, who range in age from sixty-five to ninety-six, are natives of the Brazos Valley area of Texas, have a grade-school education or less, and would be Type IA's in Linguistic Atlas terminology. I refer to them here as folk speakers. By a rather fortunate accident, we also have informants who represent the first period. While we were examining the WPA slave narratives as a possible source of linguistic evidence, we discovered that the Library of Congress had a set of recordings done during the 1930s and 1940s with former slaves born between 1844 and 1864 and was willing to make those recordings available to us. With these, we now have data from informants who represent a span of 130 years and who represent all three phases of black social history. We also have data from comparable white folk speakers and children. I am here presenting evidence from all of these groups except for the white children. The data from that group is much like what Wolfram found. His conclusions about black-white speech relations among children are largely right; they just don't apply to adults, nor do they account for striking changes in those relationships.

Table 2 summarizes the Texas data (excluding *ain't*, which we treat as a single negating morpheme). A quick look at the table might suggest that we have four similar varieties here that differ only quantitatively. The same forms are used in all four varieties with a similar pattern of person/number distribution. Standard forms predominate in the first singular and third singular, but in the plural and second singular, where

TABLE 2
Person/Number Distribution of Forms of the Present Tense
of *Be* in Four Varieties

| <i>Environment</i> | <i>Form</i> | <i>Black Children</i> | <i>Black Folk Speech</i> | <i>Ex-Slaves</i> | <i>White Folk Speech</i> |
|-------------------------------|-----------------|-----------------------|--------------------------|------------------|--------------------------|
| First Singular | am | 120(83%) | 369(96%) | 85(95%) | 160(98%) |
| | be ₂ | 23(16%) | 12(3%) | 2(2%) | 1(.6%) |
| | Ø | 1(1%) | 3(.7%) | 3(3%) | 2(1%) |
| | is | 0 | 2(.5%) | 0 | 0 |
| Third Singular | is | 734(82%) | 2000(90%) | 159(88%) | 1282(98%) |
| | be ₂ | 39(4%) | 16(.7%) | 0 | 3(.2%) |
| | Ø | 121(14%) | 194(9%) | 22(12%) | 26(2%) |
| Plural and Second Singular | are | 36(14%) | 137(19%) | 17(18%) | 192(47%) |
| | be ₂ | 73(28%) | 44(6%) | 4(6%) | 7(2%) |
| | Ø | 130(49%) | 407(56%) | 55(58%) | 148(36%) |
| | is | 24(9%) | 139(19%) | 18(19%) | 64(16%) |
| Total | | 1301 | 3323 | 365 | 1885 |

standard English requires *are*, all four varieties show a great deal of variation, with \emptyset , *be*₂, and *is* all competing with *are*. The frequencies of \emptyset and *be*₂, the forms most often discussed in connection with BEV, differ among the varieties, but the patterning of these forms is similar in all four of them. The zero forms are most common in the plural and second singular, less common in the third singular, and rare in the first singular. Likewise, in all four varieties *be*₂ is most common in the plural and second singular, but it is more likely in the first singular than in the third singular.

However striking these similarities, they actually mask significant structural differences. John Holm (1984) has pointed out that, in Caribbean creoles, what determines the form of the copula (or more precisely, what determines which copula occurs) is the following predicate and has suggested that BEV preserves vestiges of this pattern. A re-analysis of Labov's New York City data (Baugh 1980) confirms Holm's position. As a result, we decided to analyze the co-occurrence of the various forms of *be* with the following predicate type. What emerges from this analysis is quite surprising. As table 3 shows, the speech of the

TABLE 3
Syntactic Constraints on Present Tense Forms in the Plural and Second and Third Person Singular (each form as a percentage of the total number of tokens in a given environment)

| <i>Black Children</i> | | | | | |
|--------------------------|-------|-------|------|------|----|
| | V+ing | gonna | adj. | loc. | NP |
| is/are | 14 | 11 | 73 | 67 | 86 |
| be ₂ | 44 | 0 | 2 | 13 | 2 |
| \emptyset | 41 | 89 | 25 | 19 | 12 |
| <i>Black Folk Speech</i> | | | | | |
| | V+ing | gonna | adj. | loc. | NP |
| is/are | 34 | 27 | 82 | 73 | 91 |
| be ₂ | 1 | 0 | 3 | 8 | 1 |
| \emptyset | 65 | 73 | 15 | 20 | 8 |
| <i>Ex-Slaves</i> | | | | | |
| | V+ing | gonna | adj. | loc. | NP |
| is/are | 29 | 0 | 69 | 77 | 87 |
| be ₂ | 0 | 0 | 2 | 8 | 1 |
| \emptyset | 71 | 100 | 29 | 15 | 12 |
| <i>White Folk Speech</i> | | | | | |
| | V+ing | gonna | adj. | loc. | NP |
| is/are | 65 | 46 | 89 | 91 | 97 |
| be ₂ | .6 | 0 | .8 | 1 | .2 |
| \emptyset | 34 | 51 | 10 | 8 | 2 |

former slaves, black folk speech, and white folk speech seem to share a set of syntactic constraints. Before progressives and the intentional future *gonna*, \emptyset is generally the most common form, with *is* and *are* predominant elsewhere, although \emptyset is also fairly common before adjectives and locatives. In each of these varieties, be_2 is simply an infrequent variant form in all environments except before *gonna*, where it never occurs. The syntactic constraints on the speech of the children, however, are remarkably different in at least one respect. While \emptyset is still the dominant form before *gonna* (with *is* and *are* occurring most often where they do in folk speech), before *V + ing*, be_2 accounts for nearly half of the tokens and is the single most common variant. Moreover, over two-thirds of the instances of be_2 in the children's speech occur in this environment. What we have is not simply an increase in the frequency of be_2 ; in fact, if we eliminate the *V + ing* environment, both the percentage of be_2 as a part of the total corpus and the percentage of be_2 before the other predicates is virtually identical for the former slaves, the folk speakers, and the children. Rather, what we have is the increasing use of the form in a single environment, with its use elsewhere remaining constant. While most published studies have not looked at the syntactic constraints on be_2 , passing comments suggest that what we have found is no anomaly. Two studies which rely heavily on the speech of children, for example, note that be_2 is especially common in progressive constructions (Fasold 1972; Sommer 1986); Sommer notes that half of the tokens of be_2 in her corpus of lower-class speech occur in this environment. Comparable data on the use of the form by elderly blacks is harder to come by, but Brewer's citations of instances of be_2 from the WPA slave narratives (1979) suggests that our data from former slaves and folk speakers is not anomalous either. In fact, only one of her twenty-four citations is in a progressive construction. The best available evidence, then, suggests that the speech of black children differs not only from the speech of white children, but also from the speech of blacks of earlier generations in at least one important respect. While be_2 was just a variant form in all environments except before *gonna* in earlier black speech, in the speech of young blacks it has come to be the most common variant in one environment—in progressive constructions. (See also Myhill 1985.)

This conclusion, of course, simply leads to other questions. Why should be_2 be increasing in only one grammatical environment? What about the progressive would lead to an increase in that environment only? And couldn't all of this simply be the result of age-grading? The nature of the English progressive provides a clue to what is actually happening. Scholars who have written on that aspect have noted its "un-

usually wide range" (Comrie 1976) and have pointed out that it can signal a number of meanings that are not progressive at all. In English the progressive can refer to events that are habitual, continuous, or even in the future, as in

I'm running on Tuesdays and Thursdays
 I'm growing older every day
 I'm leaving on Thursday

An analysis of all of the progressives in our corpus suggests that the more frequent use of *be*₂ in that environment by black children may be a response to the wide range of the English progressive. As table 4 indicates, whenever the progressive signaled limited duration (i.e., was a true progressive) or referred to some future event, the children generally used \emptyset (sixty-five percent of the time) or, less often, *is* or *are*. *Be*₂ rarely occurred in these environments. Whenever *V + ing* signaled habitual or continuous actions, on the other hand, the children used *be*₂ more than three-quarters of the time. In other words, in the speech of these black children a systematic contrast between *be*₂ and \emptyset seems to exist before *V + ing*, although not elsewhere, with the former used for habitual and continuous meanings, the latter for "true" progressives and futures.

In adult black folk speech, however, no such contrast exists. The meaning of the progressive constructions makes little difference, with \emptyset accounting for about three-quarters of the tokens in both environments. The former slaves obviously don't make such a contrast either since none of the instances of *be*₂ in their speech occur in progressive constructions. In fact, if we examine literary citations in an attempt to extend the analysis further into the past, we find a similar situation with regard to *be*₂. One of

TABLE 4
 Meaning of Present Tense Forms before *V-ing* (each form as a percentage of the total number of tokens in a given environment)

| | <i>Children</i> | |
|------------------------|-------------------------|----------------------------|
| | Limited Duration/Future | Extended Duration/Habitual |
| <i>is/are</i> | 29 | 3 |
| <i>be</i> ₂ | 5 | 77 |
| \emptyset | 65 | 20 |
| | <i>Folk Speech</i> | |
| | Limited Duration/Future | Extended Duration/Habitual |
| <i>is/are</i> | 21 | 20 |
| <i>be</i> ₂ | 0 | 6 |
| \emptyset | 79 | 73 |

our graduate students at Texas A&M, Patricia Repka, has been looking at the present tense of *be* in representations of black speech in American plays and novels written during the eighteenth century. While there isn't a great deal of data in this literature, what we do have confirms my conclusions. Except for the fact that \emptyset occurs much more often in that data (occurring often even where standard English requires *am*), the patterning of forms is much like what we found for the speech of former slaves. Of the thirteen instances of *be*₂ that Repka has found, only one occurs in a progressive construction.

In the speech of black children, then, a contrast seems to have developed which did not exist in earlier varieties. It might be possible, of course, to argue that this contrast is actually just an age-graded feature and that as the children grow older, the extensive use of *be*₂ will diminish as their paradigm becomes more like that of folk speech (or perhaps that of standard English). Such an argument runs into several problems, however. First, as I've pointed out already, the difference between the speech of children and the earlier varieties is not simply a matter of the absence or presence of a feature or even of the frequency of a feature. Rather, the difference is one of grammatical function. *Be*₂ is more frequent in the speech of children because it has a different role. In fact, except before *V + ing*, *be*₂ occurs as frequently in black folk speech and in the speech of slaves as it does in the speech of children. To argue that this difference in the use of *be*₂ is age-graded is to argue that as children grow older, they actually lose a grammatical distinction while preserving the form which signals that distinction. Second, the age-grading argument fails to explain why only this form (or actually, grammatical distinction) is age-graded. Finally, the argument doesn't explain the source of the contrast between *be*₂ and \emptyset . If the contrast is a vestige of some earlier grammatical distinction, it seems odd that we don't find any sign of it in these earlier, quite insular varieties.

If age-grading cannot explain this difference between the speech of black children and earlier varieties of black English, then we are left with one explanation: the children's contrast between *be*₂ and \emptyset before *V + ing* must be the result of recent grammatical developments. Even as the southern white vernacular has moved somewhat closer to standard English in losing invariant *be*, BEV has begun to diverge not only from white vernaculars but also from its ancestors in reanalyzing invariant *be* as an auxiliary marking habitual actions or actions of extended duration. Wolfram was right in asserting that the speech of black children differs qualitatively in at least some respects from that of white children. What our research has shown, however, is that the black and white vernaculars were at one time much more alike than they are now and that some of

the current differences are the results of recent developments as BEV has begun to diverge not only from white speech but also from its own ancestors.

Such a conclusion, of course, demands an explanation. Why at this point in our history should black and white vernaculars be diverging? There is, I think, both a social and a linguistic explanation. The social explanation involves the increasing spatial segregation that has been the most apparent consequence of the Great Migration. As blacks have become increasingly isolated in inner cities, the day-to-day social contact that characterized life in the rural South has disappeared. Even as blacks have won new political rights, many of them have become more isolated than ever from the dominant culture. In spite of the abhorrent social and political relations that characterized earlier periods, blacks and whites had to communicate with each other; the consequence was a mutual influence that reshaped both vernaculars. Without the opportunity for frequent communication, the black and white vernaculars have begun to develop in different ways. In BEV part of this development has been the reanalysis of invariant *be*, a reanalysis that requires a linguistic explanation. In the earlier black varieties that I have described here, *be*₂ was just a spare form, a form that had no distinct meaning or function. As such, it was responsible for a lack of transparency in the present tense paradigm. Further, the wide range of the English progressive made that structure less than optimal perceptually. The reanalysis of *be*₂ as an auxiliary marking habitual events and those of extended duration, as opposed to \emptyset , which marks progressives and futures, is a clear movement toward transparency and perceptual optimality, with the spare form developing a distinct function and the complex English progressive becoming somewhat simplified.

I have tried to show here how the black English vernacular is developing new grammatical distinctions not present in earlier varieties. If, as I believe, my conclusions represent the best explanation of some rather striking data, then the relationships between black and white speech are clearly changing. When taken in conjunction with the findings of Labov and his associates, they provide a powerful argument that the black and white vernaculars are in fact diverging.

APPENDIX
Instances of *be*₂ in White Folk Speech
Unambiguous Instances

1. In September we can plant potatoes again, and if the weather *be* right, they come up.

2. Sometimes you, right up on the top [where corn has black spots], you *be* done cut that part off.
3. In the wintertime when I get there, tha's where they *be* at.
4. In the wintertime, they [horses] know [when you are coming] and they *be* out there waiting for you. In the summertime they get plenty of grass and they don't come up then.
5. He sells his pigs when they *be* six to eight weeks old.
6. These pigs, when they, when they, they *be* castarated when they small.
7. These rural churches, they [bells in those churches] still all, they all *be* pulled by a rope.
8. It [her voice] *be* old and worn out.
9. I had a good life with the young people here, and lots of activities around that little town—a whole lot better than they *be* now, it seems like to me.
10. I just don't *be* worried about finding help, you know.
11. Uh, we always mention this, and we claim it *be*, to be the smallest incorporated town in the state of Texas.

Ambiguous Instances (Not Counted in Any Tallies)

1. Chitlins, yea, that *be* out of the hogs; they make chitlins out of hog stomachs.
2. I could have a German prisoner wounded and an American, American wounded in the ambulance together, and if they *be* lying say at the bottom, and one or the other could understand the other one, they would, they would be talking to each other.
3. Just *be* out there and have your fun [what children used to do].

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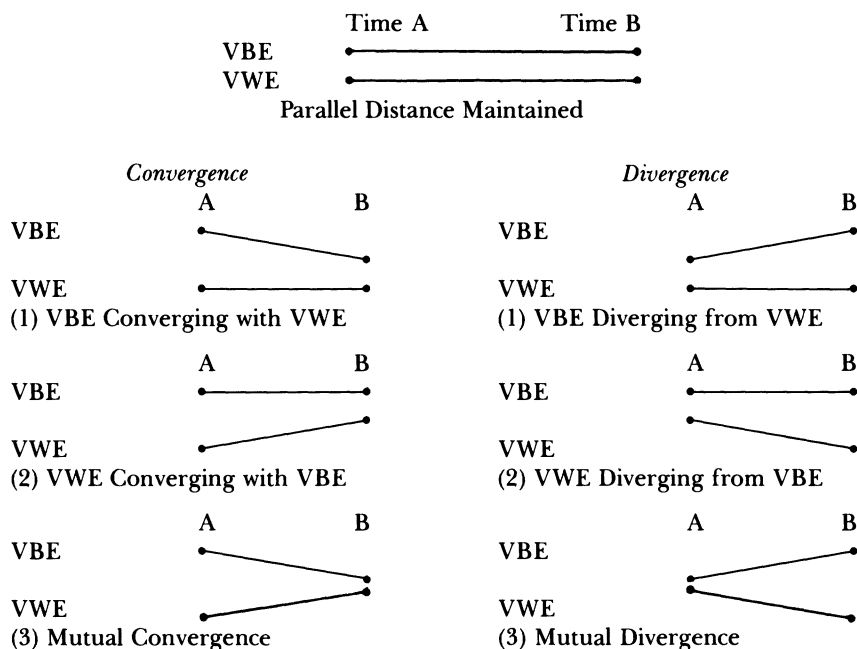
For one reason or another, the relationship of Vernacular Black English (VBE) to other varieties of English is an issue which typically carries strong emotional overtones. The issues are debated by language scholars and lay people alike, and the media seem to be particularly prone to seize upon this topic as "good story." The recent flap over the divergence of VBE is another paradigm case of the emotional context in which VBE exists, as both language scholars and the public are confronted with the position set forth in the packet of research papers distributed by Labov and his colleagues (Labov 1985; Labov and Harris 1983; Myhill and Harris 1983; Ash and Myhill 1983; Graff, Labov, and Harris 1983). In the public context of such a debate, there is considerable potential for misinterpretation and distortion of positions and facts, and a great deal of energy can be expended simply trying to explain what the positions are and what the empirical evidence is. As Fasold stated (1981, 163), "In investigating a topic with emotional overtones, like race and speech, it is

perhaps more necessary than usual to specify exactly what the issues are, what are to count as facts, and what methodology is to be used in addressing the problem." In this discussion, I would like to clarify the issues surrounding the developing controversy over VBE divergence by specifying the evidence upon which the acceptance or rejection of the recent hypothesis must ultimately be based.

POSITIONS ON VBE DEVELOPMENT. There are a number of different claims that can be made about the developing relationship of VBE and vernacular white varieties. These include a position that VBE is being maintained at approximately parallel distance from vernacular white varieties over time, and three possible claims each about increasing convergence and divergence. These positions are set forth in figure 4.

As Rickford (personal communication) points out, relationships of convergence and divergence may change over time as sociohistorical conditions change, so that convergence at one period of historical development may be replaced by divergence at another period of time, or vice versa. The position that Labov (1985) takes, based upon recent research

FIGURE 4
Positions on the Developing Relationship of Vernacular
Black English and White English



on vernacular white and black speech in Philadelphia, is represented in Divergence, Position Three, where the white Philadelphia vernacular, particularly the phonology, is changing in a direction away from other varieties, including the black vernacular. At the same time VBE, in its own right, is moving away from corresponding white varieties. Thus, VBE would become more different, even if the white vernacular were not becoming more different, as in Divergence, Position One. In Bailey and Maynor's important paper (1985a), Divergence Position One is taken, where VBE is becoming more different while corresponding white varieties apparently are not diverging in their own right. For this discussion, Divergence Position One and Position Three may be discussed together, since the issue of diverging white vernaculars is not central to this debate. Thus, no challenge to the position on white English divergence will be offered; instead, I shall focus on the claim that VBE is diverging from other varieties in its own right. For the most part, the remarks will be focused on the evidence presented in the packet of materials offered by Labov (1985) as evidence for this position. In addition, the paper by Bailey and Maynor (1985a) is included here, since the research reported there is now considered important independent confirmation of the Labov (1985) position.

ADMISSIBLE EVIDENCE. At the outset, I would like to insist that the hypothesis about the increasing divergence of VBE should be subjected to scrutiny on the same basis that we might subject any other claim about language change. In this light, reasonable empirical justification for the position should follow what I offer as the principle of admissible evidence for language change among vernacular varieties: **EVIDENCE FOR THE INCREASING DIVERGENCE OF A VERNACULAR VARIETY SHOULD CONSIST OF COMPARABLE DATA WHICH REPRESENTS AN AUTHENTIC TIME DEPTH DIMENSION SHOWING THAT STRUCTURES ARE SYSTEMATICALLY BECOMING MORE DIFFERENT FROM THOSE STRUCTURES IN CORRESPONDING VARIETIES.**

There are three primary issues that need to be raised with respect to the data we presently have been offered as evidence for the divergence of VBE: (1) the comparable data issue, (2) the authentic time depth dimension issue, and (3) the linguistic similarity issue. Questions in all of these areas must be addressed if we are to accept the current evidence for the position.

The Comparable Data Issue. One of the important claims made in the current reports about VBE divergence is that the kind of fieldwork done here "represents the most advanced fieldwork done so far in sociolinguistics, and brings us closer to the language of every-day life than any

other systematic study has done" (Labov 1985, 2). Taking this observation at face value, we must consider the implications that such advanced field techniques might have for the occurrence of particular vernacular structures presented as evidence for divergence. As Labov himself (1972b, 1975) originally pointed out, data elicitation techniques cannot be ignored, as they affect the outcome of linguistic analysis; furthermore, they may be of critical significance when attempting to account for structures that occur in interviews with vernacular speakers. The particular paradox posed in the Labov packet of evidence is this: IT IS DIFFICULT TO MAINTAIN THAT STRUCTURES ARE DIVERGING IN VBE AT THE SAME TIME IT IS MAINTAINED THAT MORE ADVANCED METHODS MAY LEAD TO SOME PREVIOUSLY UNCOVERED FEATURES IN VBE.

If it seems like this paradox is methodological nitpicking, consider the case of narrative *-s* as it is presented in the Myhill and Harris (1983) study. On the one hand, Myhill and Harris maintain that there is a VBE narrative *-s* form which is likely to occur only in the more lively narrative styles that more advanced field methods are able to elicit. In fact, Myhill and Harris note (1983, 9–10):

it is possible that this usage [i.e., narrative *-s*] is quite widespread, having gone unobserved by linguists because in the social situation created by most linguistic interviews, the /s/ inflection is likely to be used by the interviewee as a consequence of the interviewer clearly being of a different social class, thereby producing many linguistically unsystematic tokens of this inflection. The social function of /s/ in this situation obscures its linguistic function. This was not the case for the interviews we gathered our data from, which contain a deeper variety of BEV than interviews done for other linguistic studies.

At the same time it is claimed that the "deeper variety" of VBE in which narrative *-s* occurs can be captured only by using more advanced field procedures, Labov maintains that this form is "the strongest single piece of evidence that speakers of the Black English Vernacular in Philadelphia are developing their own theories of grammar" (Labov 1985, 3). But how can we be certain that the structure presented as evidence for divergence was not omitted from earlier fieldwork because the earlier fieldwork techniques simply did not produce the kind of lively, everyday speech in which it might have been used? Studies of narrative styles akin to that of narrative *-s* in other varieties (Shiffrin 1981; Wolfson 1979) seem to confirm that this narrative discourse form is particularly resistant to some of the standardized procedures for collecting naturalistic data. Unfortunately, the choice of a structure found only in the most advanced fieldwork conditions as a paradigm case of divergence does not meet the condition of comparable data. We need to ask realistically

what is authentic in terms of language divergence and what might be a function of advanced data collection procedures which lead us to previously undescribed forms.

Not all matters of data collection are as broadly based as those relating to overall fieldwork strategies in earlier studies of VBE vis-à-vis the current set of studies. There are also methodological issues that arise with studies that overtly appear more comparable in terms of data collection strategies.

In some respects, Bailey and Maynor's (1985a) study of habitual *be* is more comparable with previous studies in terms of data collection strategies; nonetheless, there remain methodological questions that have to be raised in terms of data comparability. In examining the examples of habitual *be* + *-ing* from the children in Bailey and Maynor's study, helpfully given in the appendix, I was struck by how many cases seemed to involve childhood games and play activities. In fact, according to my conservative tabulation, at least fifty-one of the seventy-eight examples of the habitual *be* + *-ing* examples occurred in this context. Children would, of course, talk about such activities as present-time, habitual activities, but adults over 70 (the two groups examined are children and adults over 70) obviously would talk about these as past-time events. That constitutes an important difference in terms of linguistic structures representing present-time, habitual activity, the appropriate semantic context for the use of the *be* + *-ing* form discussed by Bailey and Maynor as an innovative form within VBE. What if all the current games or childhood activities were eliminated for both groups in order to ensure comparability of potential cases for *be* + *-ing* usage? Would the children still be different from the adults? And if not, what are the consequences for the analysis of *be* + *-ing* as an innovative form within VBE? Experience in collecting vernacular forms (e.g., Labov 1975, Wolfram 1986) has taught us that the kinds of forms that occur in an interview may be influenced greatly by the structure of the interview, and that this issue cannot be trivialized into cavalier dismissal in our analysis.

The Time Dimension. The second issue that needs to be faced is the time-depth dimension. To show that current-day VBE is systematically diverging from what it was at some prior stage in its development, it is necessary to set a baseline for the dialect at some previous stage in time.

Most curiously, none of the research studies cited in support of Labov's hypothesis (e.g., Myhill and Harris 1983; Labov and Harris 1983) offers any real or apparent time-depth dimension as a basis for establishing increasing divergence. The closest we come to such a dimension is a

group of senior citizens included in the Labov and Harris (1983) study, but this group is not compared in any formal way with other groups in terms of the time dimension necessary to establish change; in fact, for the features discussed in Labov and Harris (1983), no substantive differences are presented. For the claim that narrative *-s* is developing a special function in present-day VBE, five different speakers are presented (Myhill and Harris 1983). We presume that these five speakers represent a younger generation, but specific ages are not given, and my independent attempt to identify the specific ages of the speakers by matching them with speakers in the Labov and Harris appendix of speakers by social networks did not turn up all the speakers. The important point to be made here is that, if there are important generational differences to substantiate the divergence hypothesis, the evidence from generational differences has not been presented. For such a critical claim about increasing divergence, the only option we have is to assume that there is some baseline from previous studies or state of knowledge. Whatever it is, it is not given. We thus have no recourse but to ask to see such critical data analogous to that presented by Labov in careful studies documenting language change over time in other situations (Labov 1966; Labov, Yaeger, and Steiner 1972).

Bailey and Maynor's (1985a) study presents a much more rigorous case for change than Labov by showing generational differences for VBE speakers, as they systematically compare speakers over 70 and adolescent speakers to justify the conclusion that habitual *be* + *-ing* is an innovative feature of later VBE (i.e., post-Great Migration). Assuming that the data-comparability issue we mentioned earlier can be dealt with in Bailey and Maynor, the evidence seems to be adequate. There is, however, one proviso that needs to be openly addressed. It is a familiar quibble that has long plagued the assumption of apparent-time studies (e.g., Labov 1966), where different generations are seen to represent different time periods in the absence of longitudinal studies. The assumption is that different generations within a community will represent genuine changes of the language over time rather than changes within the life cycle of speakers within the community as a function of age-grading.

In fairness to Bailey and Maynor, this option is considered and rejected. But I have a nagging reservation. I recently re-examined the different studies of VBE based on data collected in the 1960s (Labov, Cohen, Robins, and Lewis 1968; Wolfram 1969; Fasold 1972) and, to the extent that data is available, it appears that habitual *be* is much more common among children than adults. So twenty years ago, in the Northern urban areas, we have a pattern which is also characteristic of the

1980s in the South as presented in Bailey and Maynor (1985a). There are certainly no studies that show adults to be more extensive users of habitual *be* than adolescents. Perhaps it is historical coincidence, but the predominance of habitual *be* among children at different times and difference places suggests that the possibility of age-grading is more than a red herring. I think this is an issue that can be resolved, however, with a little further study. I propose that we examine middle-aged VBE speakers in communities where twenty years ago we found habitual *be* to be more frequent among children than adults (e.g., Detroit, Washington, D.C.). If current speakers between 30 and 40 show persistent high levels of habitual *be* + *-ing*, then Bailey and Maynor are probably correct in proposing habitual *be* + *-ing* as a relatively recent change; if not, *be* + *-ing* is probably genuinely age-graded. I hope to collect data of this type in the Washington, D.C. area and will happily report the results of this study in print. I am certainly willing to be proven wrong in my reservation, but I would like to see the missing evidence.

The Linguistic Issue. Finally, there is the linguistic issue. Are structures in VBE, independent of vernacular white varieties, becoming more different from corresponding white vernacular varieties? I shall focus only on one example here, but one which is offered as "the strongest single piece of evidence that speakers of the Black English Vernacular in Philadelphia are developing their own theories of grammar . . . the use of verbal *-s* inflection" (Labov 1985, 3).

It is appropriate to begin this section with a confession: my own analysis of *-s* present-tense inflection (Wolfram 1969) by VBE speakers over fifteen years ago was simplistic and inadequate: there appear to be several functions for *-s* on third person and non-third person forms, not all perfectly understood by me or others despite a number of different recent proposals (Brewer 1986; Pitts 1981). The recent proposal concerning narrative *-s* by Myhill and Harris (1983), enthusiastically endorsed by Labov (1985), suggests that the use of *-s* may mark a lively narrative in a way somewhat akin to the historical present found in other varieties of English (Myhill and Harris 1983, 9). Its usage is perhaps best seen through an illustrative passage, as presented in Myhill and Harris (1983, 3–4):

Ooh, jus' like that day Jackie wen' in the hospital an' Andre came over, so I said "c'mo', Andre, Le's go to Gino's or le's go to McDonal's." So, Verne was gonna go wif us. So I said, "Shit, she don' gotta go, we go." So we went to Gino's, COMES back. So the secon' night, LaV—we said "Cmo, le's—" I SAY "Le's go to McDonal's again." So, we GOES to McDonal' again. So, we ain' go to McDonal',

wen' to McDonal' then we wen' to the bar so Andre set up the bar an's said "Pam." I said "What?", he SAY "You know, I think this is a setup" I SAY "I 'on know," he said "'cause it's two night straight LaVerne sposta wen' wit us an' ditn' go." I said "Well I 'on' know." Jackie COMES out the hospital, here COMES LaVerne with the mou' "bla bla bla bla bla bla bla." Jackie GOES back and TELL Andre that, yeah, the whole time that she was in the hospital me and him wen' out.

In this function, the present tense verb forms are analogous to the so-called historical present as found in other varieties of English (e.g., Wolfson 1979; Shiffrin 1981), as a lively narration of a past-time event systematically shifts between present tense forms, with and without *-s*, and past tense forms. For example, consider the following illustration of the traditional use of an historical present, from Shiffrin (1981, 46):

Then all of a sudden everybody GETS involved, and they made a mess. So, uh . . . this lady SAYS . . . this uh Bert, "Oh, my son'll make them. He's an electrician." So he MAKES them, and he CHARGES all the neighbors twenty dollars a set, and there I paid three dollars. So I called her a crook. And I called her son a crook. So they were really mad at me.

To my knowledge, no one had previously maintained that VBE used a type of present tense narrative form resembling the historical present documented for other vernacular varieties of English. If this is the case, then the use of the general discourse type would appear to present a possible case of convergence (with, of course, the same types of data-comparability problems cited above) rather than divergence. However, as presented by Myhill and Harris (1983), there are several morphosyntactic differences in this use of the narrative *-s* in VBE as compared to its use in other varieties. One difference is the relative nonuse of *-s* on *say* as *she says* or *I says* in VBE, which is a favorite context for its occurrence in white vernacular speech. The other difference cited is the use of *-s* only on the first member of conjoined clauses (e.g., *This white guy RUNS behin' me and BEND down*), which certainly contrasts with the constraints on such marking for white vernacular speech as presented by Wolfson (1979) and Shiffrin (1981). There are a number of other, deeper aspects of historical present organization that might have been considered, following Shiffrin (1981), but none of these is considered by Myhill and Harris (1983) or Labov and Harris (1983). (See also Butters 1987.)

I won't argue with the conclusion about the morphosyntactic differences between historical present in white vernacular forms and narrative *-s* in VBE, based on the five VBE speakers who constitute the evidence in Myhill and Harris (1983). But it appears that, given these differences, this discourse organization is still rather similar in underly-

ing organization to the historical present, a fact admitted by Myhill and Harris (1983, 9). So the question that we must confront is this: Which is more different, a system which uses historical present with some *-s* marking realignment or one which shows no evidence of the historical present usage? Remember here that this is the first attestation in VBE that we have for a discourse structure similar to the historical present in other vernacular varieties. Given the narrative similarities for the historical present, I personally find it hard to accept the surface, morphosyntactic dimensions of narrative *-s* as the strongest single piece of evidence for increasing divergence in VBE. This is not to say that the analysis is not insightful; I think it is, and it constitutes an important step in our understanding of the uses of *-s* forms in VBE. However, as evidence for the growing divergence of VBE and vernacular white varieties, it must be re-evaluated as *prima facie* evidence.

CONCLUSION. So where are we in terms of the presented evidence for the divergence of VBE? In all intellectual honesty, I cannot say that the available evidence for the divergence of VBE is convincing. This is not to say that I could not be convinced. It is certainly possible that data and analyses which meet the conditions for admissible evidence might be met. And, if these conditions are met, I will be happy to accept the hypothesis offered by Labov and his colleagues and Bailey and Maynor (1985a). But we must be offered data which meets reasonable standards for any claim about linguistic change over time. On this score, VBE certainly should be treated no differently from any other variety, and we do no service to the variety or its speakers by holding it to a different set of standards for admissible evidence.

VI. ARTHUR K. SPEARS (*City University of New York*)

When we talk about the study of language in its social-cultural context, we also have to talk about the dissemination of research results in their socio-cultural context. There are some issues that need to be sorted out that haven't been so far. One has to do with the difference between presenting research results for linguists and other language scholars, on the one hand, and presenting them for popular audiences, on the other. This is one type of issue that clearly has to be confronted directly because there are obviously some misunderstandings. Let's consider specifically some alternative approaches we can take in evaluating Labov's research.

When we consider his statements about divergence between the ver-

vernaculars, we have to keep in mind certain aspects of the historical background of those statements. And we must also remember that most of the writing on his research has been for lay audiences. Keep in mind also that there was a general impression (outside of linguistics and to some extent in linguistics) that there had been a steady convergence, decreolization specifically, between white and black vernaculars, that is, that black vernaculars were changing steadily in the direction of white vernaculars, but not vice versa. In other words, the change was one-sided. (Here, I'm talking specifically about phonology and morphosyntax; I believe black influence on the lexicon of general American English is often recognized.) It seems that one-sided convergence—that is, decreolization—was assumed to be the result of mass media influence, language norms inculcated by public school education, and increased social interaction between blacks and whites resulting from school integration. In view of this assumption of one-sidedness in change—which had certainly permeated much of the public's thinking about black English and which resulted from information disseminated by linguists during the late 1960s and early 1970s in particular—against that backdrop, Labov's claim of divergence takes on a special importance. It says that the overall evolution of black culture—and his focus is on language specifically—is about more than assimilation. It affirms what most black social scientists have been saying all along, in unison with some of their nonblack colleagues, that black culture is creative and transformative, taking from mainstream American culture as well as other American cultural traditions, but at the same time maintaining and evolving, if you will, its own distinctiveness. This is what we expect in all situations of contact, even those showing the particularly strong and negative aspects of the black-white contact in American history. Although Labov is not the only one to claim divergence, his claims and discussion of them and their implications are the only ones that have had an impact on the public at large. To sum up, I believe we could expect, *a priori*, at least some divergence, in language as well as other areas of black culture.

It's also worth noting that Labov has linked his claim of linguistic divergence to the social fact of increasing segregation between blacks and whites. There is a strong implication if not an outright claim that the linguistic divergence is due specifically to the INCREASING social isolation of black people. I believe this needs some qualification. For divergence to exist, and I would prefer to use the term INDEPENDENT CULTURAL (including linguistic) DEVELOPMENT, the only requirement is that there be vital black communities, and by "vital" I simply mean ones large enough to have a thriving cultural life of their own. This is essentially the situa-

tion we have in large urban areas of the northeast in particular as well as other areas of the country. In other words, the sociological data pointing to increasing segregation in our northeastern cities are a red herring. The conditions for linguistic phenomena such as narrative past *-s* to develop in the black community have been present throughout the better part of this century, if we limit ourselves to speaking of large urban black communities, and they have been present throughout American history, if we're speaking simply of vital black communities whether they are large and urban or not.

There are several key questions for linguists regarding the material produced by Labov and his associates. But before talking about them I want to add some remarks to those Walt and Fay have already made relating to the term DIVERGENCE itself. It's clear that it can be interpreted in several ways, as Walt has noted. For one thing, are we talking about global divergence, that is, between language varieties as wholes, or simply divergence with respect to certain features of grammar? Surely, neither Labov nor anyone else is claiming that there is divergence affecting white and black grammars as wholes, that is, with respect to all of their features. After all, there is clearly some convergence, as much research has already shown, so we can safely eliminate this global interpretation of divergence from discussion. We might also ask whether, on balance, black and white dialects, in Philadelphia or any place else, are more different from one another than they were, say, twenty or thirty years ago? That is, are there more specific grammatical features which differentiate black and white dialects now than there were then? Of course, our knowledge is not yet advanced enough to begin to answer such a question. Our situation, in effect, is one in which we're forced to talk about divergence—or convergence—with regard to particular features of grammar. The real question, then, becomes one of to what extent the claim of divergence can be supported, a claim we will interpret as attaching to specific features of grammar. To state it differently: is the research conducted so far merely suggestive, incontrovertible, or somewhere between the two?

First of all, I see no reason at this point for questioning Labov's findings concerning the existence of the Philadelphia black English narrative *-s* verbal inflection and the vowel change in progress among Philadelphia whites. The diachronic situation with regard to *-s* is another matter. I will leave aside for now the question of how many blacks in Philadelphia have the grammatical feature in their speech.

Now the existence of the narrative past *-s* does not in and of itself tell us anything about divergence since DIVERGENCE is essentially a dia-

chronic term, but the existence of this feature does indeed tell us something about difference. Significantly, in this case we're talking about an "absolute difference," to use Labov's term, that is, a qualitative one as opposed to a quantitative one, between black and white vernaculars in Philadelphia—and conceivably in other areas. This difference is one of several that have been claimed over the past several years to separate black and white vernaculars. I can point out as examples the disapproval forms *come* and *go*, which I have discussed (1982); others such as certain uses of *be done*, which Baugh (1983) and I (1985) have discussed; and the invariant auxiliary *be*, discussed by Myhill (1985; as a disapproval form specifically). In other words, the constructs that we set up, and which change continually as we learn more, which we label respectively as black and white vernacular English—these constructs are diverging. This is divergence on the level of abstraction, we might say; but, of course, the question of concrete divergence, in actual speech over time, is still with us. In spite of this, it is still worth stressing that we are still discovering significant differences between black and white vernaculars. The primary reason this is possible is that structural-functional differences in black speech can be camouflaged, in utterances which ostensibly show no uniquely black morphosyntactic features.

Narrative past *-s* is often camouflaged. We can take some examples from Myhill and Harris (1983, 3): *Jackie COMES out the hospital, here COMES LaVerne with the mou'[th]*. The *-s* in these examples could be mistaken as the third singular *-s* in other English dialects. The true grammatical nature of *-s*, in other words, is camouflaged. It is only through the quantitative and functional analysis of this form that a real understanding of its grammatical status is reached. Such an analysis reveals, among other things, that this *-s* occurs with persons other than third singular, it occurs about fifty percent of the time in narrative clauses, hardly ever in nonnarrative clauses, and all black speakers studied have it in their speech. The really important consideration in all of this is that, given the camouflaged character of narrative *-s*, there is the possibility that it has been in Philadelphia black English vernacular—and indeed that of other locales—for some time without ever having been noticed.

In discussing another recently discovered grammatical feature of black English, the semi-auxiliary *come* (e.g., *She come callin' me, come yellin' in my phone* 'She had the nerve to call me and [had the nerve] to yell in my phone'), I noted (1982) that it would be well if we could detail the history of such forms. There are at least three possibilities—and two are worth entertaining for narrative *-s* because we don't yet know whether it is indeed a new feature in Philadelphia black English. This feature could

be a creolism, that is, a holdover from the creole past of black English. It could be a relic once found in nonblack dialects, that is, a feature once found in American and possibly British dialects which have lost it while black English retained it. Or, it could be an independent development, in other words, a feature that has emerged during the evolution of black English with no counterpart in nonblack dialects, neither at present nor historically. This last possibility is, of course, what is implied by the research of Labov and his associates.

It is important to remember that the last two are real possibilities. The first is not: as long as nothing like this feature has been reported for English-based creoles, we have to assume that narrative *-s* is not a creolism. It is not inconceivable that narrative *-s* is a relic once found in white dialects, but we have nothing to support such a view. It could even have been imported from the South, only recently taking a significant hold in black speech. Some possibilities are certainly more probable than others, but all are to be considered.

To return directly to the question of divergence, it is clear that narrative *-s* could be part of a stable sociolinguistic variable and, consequently, one that has been in the language for some time. The sociolinguistic literature has already identified *-ing* and /θ/, among others, as stable variables. Obviously, what is needed for narrative *-s* is some indication of its distribution across age groups, but, as others have already noted, we have none. To reiterate the point, we need at least data not only from young speakers, but from older ones also. Ideally, we would want data from three age groups, in order to eliminate any suspicions of the age-grading Wolfram has already mentioned. In sum, additional data are needed to draw the diachronic implications essential for any discussion of divergence. I should add, in passing, that the situation is different with the vowel shift among Philadelphia whites: there are apparent-time data. Furthermore, this vowel shift is absent among Philadelphia blacks. So, with regard to this particular feature of grammar, we can indeed talk about divergence. To repeat what I said before, we can talk about divergence with respect only to specific features and sets of features. Our knowledge is not yet broad or deep enough to discuss divergence with respect to language wholes.

Although we are here to talk about divergence between black and white vernaculars primarily from a sociolinguistic viewpoint—in the narrow sense—I believe it is always apposite to discuss sociolinguistic claims in a broader sociolinguistic sense. We, as linguists, have to take responsibility for shaping public discourse about the social implications of findings on the grammar of minority languages. We have a critical under-

standing that derives from our expertise in matters of grammar, and when we can short-circuit the dissemination of misbegotten ideas, we should do so.

For the sake of discussion, let's take some of Labov's claims on face value and assume that narrative past *-s* is a new feature in Philadelphia black English, and that there is indeed divergence between black and white dialects in the sense that these dialects are more different today than they were at some time in the past. What would this mean?

Let's consider specifically some comments on the implications of the research findings. These comments appeared in a news release from the University of Pennsylvania, dated 15 March 1985. Of course, we have to recognize that a person's ideas can change over time, and a news release can't be expected to convey the full breadth and subtlety of someone's thinking on issues.

These language differences have contributed to widespread educational failure among blacks in the inner-city schools. . . . And the problem seems to be getting worse over time rather than better. Labov believes that language division has been caused by decreasing personal contact between blacks and whites. The most effective way for black children to learn other dialects of English in addition to their own dialects is through greater interaction with whites. . . . The mass media, including television and radio, have had little influence on the speech patterns of blacks or any other group.

First of all, it should be pointed out—as it already has been by others and me—that the role of language in the widespread failure of black students in the inner-city schools is problematic. Joshua Fishman (1972) was the first, to my knowledge, to point out that differences between standard and nonstandard dialects in a number of countries—Switzerland, Sweden, Finland, and Japan, to name a few—are much more radical than those existing between black English vernacular and the standard used in American education. Yet, in these countries, one finds significantly less educational failure. It appears, therefore, that while language differences can cause educational problems—it only stands to reason—there is no reason for such problems to stand in the way of educational achievement. Greater language differences are overcome elsewhere. Why can't they be overcome in American schools? The answer that comes through in a number of studies of the issue is that the real problems are attitudinal and social. All these problems can be related to the general problem of institutional racism, that is, racism which permeates the very structure and daily workings of all societal institutions, so much so that individuals do not have to be racist for racism to affect the lives of minorities. Frantz Fanon (1952, 1970) first articulated

the theory of institutional racism, and it has been elaborated by many others, so I need not go into detail now. Institutional racism is implicated in several problems, among them low teacher expectations and disrespect for the home language and culture of inner-city pupils.

Now let's consider the idea that language division has been caused by decreasing personal contact between blacks and whites. We must be careful to maintain the distinction between white vernaculars, such as that studied by Labov in Philadelphia, and the standard English of the schools. It is uncontroversial that separation between groups leads to the drifting apart of their speech. The problems come when it is assumed that "the most effective way for black children to learn other dialects of English . . . is through greater interaction with whites." This is simply not true, particularly when we are talking about standard English. It is often implicit in such discussions that blacks can learn not only white vernaculars, but also standard English, solely through contact with whites. This view totally ignores the fact that an important factor, if not the most important factor, in the acquisition of standard English by black English vernacular speakers is contact with middle-class blacks who speak standard English. Consequently, it is imperative that the separation of the typically working- and underclass blacks who speak the vernacular, from middle-class blacks, following the widespread self-removal of the latter from inner cities, be considered alongside black-white residential segregation in attempts to explain any claimed divergence.

The last comment I will make concerning Labov's research in light of larger social issues has to do with standard English acquisition as a goal for black English vernacular speakers. Obviously, standard English is an important tool in our society. There is a consensus that it is the schools' job to teach it, and it is incumbent upon teachers in the nation's schools to teach it well. Some linguists, as well as other scholars and lay-people, however, go on to claim that standard English is essential for economic success and that the economic handicap that many blacks face is a result of their not being able to produce standard English. This type of statement needs major qualification.

Standard English is indeed important for some types of jobs, but not all. There are many well-paying jobs for which the ability to speak standard English is irrelevant—for example, the overwhelming majority of skilled labor jobs. While it is true that the service sector in the United States accounts for a steadily increasing percentage of all jobs, and that service jobs typically require the ability to communicate in standard English, there remain many jobs for which standard English is just not a requirement. Also, we must ask, if black English vernacular has eco-

nomically handicapped blacks, why haven't varieties of nonstandard white English economically handicapped the majority of white Americans who speak them? No attempt is ever made to blame the economic woes of white communities on their language. Why is this continually done in the case of blacks? I believe this is simply another way of sidestepping the fundamental issue of institutional racism. This is not to say that it is not true that more stigma is often attached to nonstandard black speech than to nonstandard white speech. The real question is why nonstandard black speech is more stigmatized. As students of language attitudes know, it is because its speakers are more stigmatized—for reasons discussed in various theories of racism and oppression.

I am making these remarks for the record simply because remarks along these lines are too few and far between. After all, what would happen if all black people in the United States suddenly woke up tomorrow morning speaking English like President Reagan's, or Queen Elizabeth II's, for that matter? Would the economic problems of black people be set on a steady path toward solution?

One last comment: even though not all of the evidence is in, my suspicion is that Labov is correct on at least some points. Specifically, my guess would be that narrative past *-s* is indeed a new, independent development in black English vernacular grammar, and that the white and black Philadelphia vernaculars are in some ways moving away from standard English, each in its own direction. There's a good possibility that they are also moving toward standard English in some ways.

VII. JOHN RICKFORD (*Stanford University*)

I think a number of issues are raised by the work of Bill Labov and his associates, Ash and Myhill and others, and by the work of Guy Bailey and his associates, and the implications arise on many levels. Obviously, there are purely linguistic issues that have to do with the description of black and white dialects; these give rise in turn to much larger, particularly intriguing, theoretical and methodological issues about how we do variation theory, and how we do sociolinguistics. There are also, of course, a great many applied sociolinguistics issues. Professors Spears and Vaughn-Cooke have both spoken eloquently about the educational and political issues that are involved, and I want to talk instead about some of the theoretical and methodological issues that are raised, and maybe introduce a little additional data at the end.

The points that arise from studies of this nature fall into two groups,

the linguistic and the social, even though (as always) it's very hard to keep the two apart.

Within the linguistic group, the first point is the importance of a distinction between real time and apparent time. Other speakers have said so much about this issue that I hardly need to say anything about it, except to stress, as Vaughn-Cooke has already done, that it was Labov himself who pointed out the critical importance of having comparison points in real time.

Data in apparent time can be misleading, particularly with some of these variables, because of a second point which sociolinguists in general fail to take into account: the old competence vs. performance distinction (see Rickford, *in press*, for more discussion). For years we've been running around feeling that because we use elaborate techniques for getting at the vernacular, the evidence that we have of what speakers *DO* is perfect evidence of what speakers *CAN* do; even when we make qualifications to the contrary, deep down in our hearts we believe that we have come close to their real systems. But these matters are usually more complicated. Take the matter of age-grading. When I first entered the field of linguistics and read the early work, I was led to believe that age-grading was a reality, that black English was spoken mainly if not exclusively by black children and adolescents. But after moving to Philadelphia, and living right in the middle of the black community, I heard all these people—twenty, thirty, forty, fifty, sixty years old—who were using all the forms that they were supposed to have been age-graded out of many years before. One has to ask: what comes with increasing age? Is it a loss of forms that one knew before, or is it an extension of repertoire that allows you then to do different things with the way you present yourself in public life, a development of sociolinguistic competence that allows you greater freedom to represent what you can and cannot do? It's very clear that adults in the black community have a wider range of styles in which they can present themselves to the outside world than do children aged six, seven, eight, nine, or ten. Now we *KNOW* these things, and yet we tend to *FORGET* them. There are parallel examples in macrosociolinguistics; Fishman reported recently (1981) that a 1970 census shows a much higher proportion of mother-tongue speakers, people who claimed competence in a non-English mother tongue, than he found to be the case in 1960. Well, is it true that in ten years there was a massive rise in the number of people who in fact had a non-English mother tongue? If anything, our experience with language would lead us to expect the opposite. I think what was happening in that case is that we were witnessing the tail end of a whole series of political movements in the 1960s which

made ethnicity something that it was much more acceptable to claim and to display and to talk about. And Fishman's own work has explored some of the ways in which factors of this type operate. (See also Fishman et al 1985, 107–94 and 508–13.)

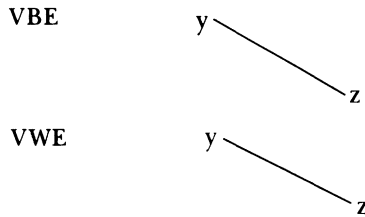
Another important linguistic point is the question of the differences between lexicon, phonology, and grammar which has arisen, I think in some very revealing ways, in the work of Labov and his associates. Given the differences that we find in the degree to which lexical, phonological, and syntactic features are diffused, it really becomes difficult to sum these all together and talk about Variety A and Variety B becoming on the whole more different or more similar. As some people have noted, you may have convergence at one level and divergence at another. So in the end we really have to restrict our research and our conclusions to one feature at a time.

Now the fourth point I want to talk about is the possible types of relationships between black speech and white speech which we might expect to find over time. This is an important matter, since it is easy to jump to larger (or smaller) inferences than the data warrant. I should say at the outset that I came up quite independently with diagrams which are very similar to the ones which Walt Wolfram has just presented (figure 4, 41), so I'll make reference to his where possible and introduce new diagrams of my own only where they differ from his in one or more respects.

Between any two points in time (A and B in Wolfram's diagrams, v, w, x, y, and z in mine) there are basically only three possible relationships which VBE and VWE (or any other pair of varieties) might exhibit with respect to a particular feature: equidistance (Walt's "parallel distance"), convergence, and divergence. Walt's first diagram is a good characterization of equidistance over time without change in either variety. To this I would add only that equidistance could equally well be maintained through parallel change in both varieties, as depicted in figure 5. That is, we have to be careful not to equate equidistance over time with absence of change over time. Equidistance with parallel change would suggest a closer relationship between VBE and VWE than equidistance without change, since (barring coincidence) members of each community would have to know about and follow developments in each other's communities throughout the time period to retain the same degree of similarity or difference as change occurred, whereas equidistance could be maintained without further contact if no change occurred in either community over time.

Wolfram's diagrams provide an excellent characterization of the different types of convergence and divergence which are possible. There is

FIGURE 5
Equidistance Over Time with Parallel Change in VBE and VWE



an additional wrinkle to these relationships which we tend to forget, however, which is that convergence between varieties over one time period might be followed in the next time period by divergence. Figure 6 is one possible model of this relationship, and it is not entirely hypothetical. The adoption by whites of erstwhile black slang terms like *hip* during the late 1960s was a common talking point among blacks, and may have been in part responsible for the subsequent replacement of *hip* by *live* (layv) and *fresh* among some black adolescents and young adults. Whether or not this was actually the case, figure 6 models a fully plausible possibility, parallel to the pursuit-followed-by-flight metaphor which Joos (1952) introduced, but without the references to elites and masses present in his discussion.

Figure 7 demonstrates that if the time period is long enough, and we have evidence about the shorter time periods which comprise it, we might find that the two varieties have gone through virtually all the different possible relationships (equidistance, convergence, and divergence). The reason it is important to keep figure 7 in mind is that if it is in fact true that VBE and VWE are currently diverging in a number of respects—and I have still not seen the real-time evidence to clinch the case—some researchers will leap to the larger, unjustifiable conclusion that it is evidence of a long-term trend, perhaps of the antirealist position that black-white speech was more similar in slavery days but has

FIGURE 6
Initial Convergence Over Brief Time Period through Approximating Change in VWE, Subsequent Divergence Over Brief Time Period Through Distancing Change in VBE

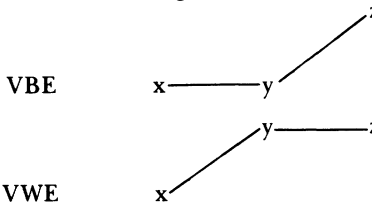
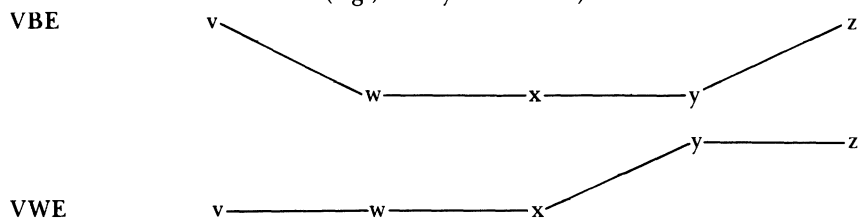


FIGURE 7
Convergence, Equidistance, Convergence and Divergence Over Brief Time Periods (e.g., Thirty Years Each)



become more different because of increased segregation since then (the third theoretical combination of positions on the extent-of-differences and creole-origin position presented by Wolfram 1971, as well as by Fasold 1981, 163–64). In figure 4, Wolfram represents this trend as “Mutual Divergence,” and his “Mutual Convergence” illustrates the opposing decreolization hypothesis—that black and white varieties were more different during the days of slavery but have been becoming more similar since then because of increased social mobility and acculturating pressures from the white majority. Proponents of either position (and the other theoretical possibilities outlined by Wolfram 1971) will undoubtedly continue to argue the merits of their respective views, but figure 7 should remind them that it is treacherous to base arguments for long-term hypotheses on current developments or short-term trends.

I’ve talked a bit about the linguistic factors. I just want to say something briefly now about the social factors, and I think these fall under two main categories. First, there are the *ECOLOGICAL* factors (Whinnom 1971, 92), the factors having to do with the nature of the contact: how many blacks, how many whites, where are the neighborhoods located, what is the nature of their interrelations? It is not a priori clear to me (and I would certainly like to see more data on this) that the contact situation that exists, in the different places that have been talked about, is in fact representative of an *INCREASE* in segregation. The second factor that’s relevant here is what Whinnom (1971, 93) calls *ETHOLOGICAL* factors, including emotional and attitudinal factors. There’s a general assumption (which, of course, deep in our hearts we don’t believe) that contact alone will lead to input, so that—if you get enough exposure—input from the outside will become intake, which in turn will become output. However, this is not always the case. Labov (1984, 14–15) and his colleagues have shown very clearly that this isn’t the case with data from TV or the mass media. But it isn’t necessarily the case with interactional data either. The recent study I’ve done (Rickford 1985) of one black and

one white speaker in the South who've lived together on an island nine miles by five miles, an area of maybe forty-five miles in all, for seventy years, who showed very strong convergence and similarity in phonology, with marked divergence in grammar, led me to question the normal assumptions about the effects of contact. If you live on that island, you get a strong sense that there are very strong social notions of the way whites should speak and the way blacks should speak. And this is reflected both in the forms that each group uses and the kinds of speech events in which they participate.

Now, the other thing is that, although we have very nice work going on in modern-day speech communities, we have a real challenge to go back in time, both to the historical records and as far as possible to all of the other available evidence to see what was going on. In this respect I particularly like Guy Bailey's attempt to get at those early periods. Recently, I've been looking in detail (Rickford 1986) at the relations of the black and the Irish, northern Scotch-Irish vs. southern Catholic Irish, in the Caribbean and in the United States. The findings are extremely interesting. One can ask, even without looking at the linguistic data, what kinds of diffusion one might have expected from the sociohistorical record of Irish/African contact.

It turns out that in the seventeenth century, throughout the Caribbean and in different parts of the United States, blacks and southern Catholic Irish worked together and were commonly reviled. Initially, both were servant groups; the Africans weren't brought in initially as slaves. There's a lot of evidence that these two groups rebelled together; in some cases they conspired together; they were hanged together as public examples. And while there are limitations from one place to another that require caution about saying they were identical kinds of populations, there's a great deal of evidence that there was a good deal of cohesion. And I think one would expect, although there have been disclaimers to the contrary as a general principle, that the high level of sociopolitical solidarity would have led to greater linguistic diffusion between blacks and whites.

In the eighteenth century America started to get the northern Scotch-Irish group, which differs from the southern group in a number of ways. Their relations with local black populations in some places are very interesting. For instance, early eighteenth century Pennsylvania, in particular Philadelphia, shows situations and conditions which were more favorable to convergence in social terms, between blacks and whites, than those in other parts of the country at the same time, or in the same place at later periods. For instance, Pennsylvania as a whole in 1700 had only 400 to 500 slaves. The number of slaves in Philadelphia in 1767 was

only 1400, or nine percent of the city's population. Slave holders in Pennsylvania typically had only one or two slaves each. Furthermore, most of these slaves were household servants, with young ones explicitly preferred "so they could be trained at an early age and devote the largest portion of their lives to serving their master and his family" (Foner 1975, 227). Those who were not household servants were skilled artisans. And one visitor commented on the fact that, in Pennsylvania's iron factories in the 1750s, there were a great many blacks and whites (servants from Ireland and Germany) working together. Now whether they communicated also outside of the work environment is another matter. But there is evidence that there were fairly close relations in that century.

The nineteenth century presents an interesting schism between the contact or ecological factors and the ethological factors. On the one hand, there are the huge numbers of nineteenth-century Irish immigrants, mainly southern and Catholic, coming to America in the wake of the various Irish potato famines and failures. There were hundreds of thousands of them, mainly concentrated in northern cities. By the middle of the nineteenth century, forty-six percent of all foreign-born Irish were in four northern cities: Boston, Brooklyn, New York, and Philadelphia. Now, to the extent that there were blacks in those cities, both they and the Irish were commonly reviled, as in the seventeenth century. One finds ads in the *New York Courier and Enquirer* which said, "No Blacks or Irish need apply." But whereas common oppression in the closed plantation environment of the seventeenth century had led to black/Irish solidarity and joint rebellion, the blacks and the Irish in the nineteenth century were scrambling for open employment opportunities—it was a very different sociocultural climate—and in fact what the record reveals is tremendous hostility between the two groups. In 1834, Irish shipwrights set fire to the Shelter for Colored Orphans and attacked the black Bethel Church in Philadelphia (Foner 1983a, 432). In the 1850s, the luminary Frederick Douglas lamented the fact that every day brought hundreds of new immigrants "whose hunger and color" seemed to favor them most (Foner 1983a, 214–15). By the 1860s, black/Irish job riots were commonplace in northern cities (Foner 1983b, 392–95). Because of the hostile climate in which blacks and Irish were interacting during this period, it's very unlikely that there existed the intimate contact that would have provided opportunities for linguistic diffusion between the two groups. Even if there were opportunities for linguistic input, putting it out as output and talking like an Irish white if you were black or like a black if you were Irish white would have been highly unlikely.

Now the ideal thing would be to find linguistic features which allow us

to test these hypotheses about the effects of social relations on linguistic diffusion, and in fact there are some. One of them involves variation between *be* and *do(es) be* as habitual aspect markers. The eighteenth-century Irish immigrants who came in smaller numbers than their nineteenth-century counterparts but had closer relations with blacks were largely from northern Ireland, probably using a form of Irish English in which habituality was expressed by *be* without a preceding *do* as in *He be happy* (Harris 1985, 77). The nineteenth-century Irish immigrants were primarily southern Irish, probably using an Irish English variety in which habituality was expressed by *do be*, as in *He does be happy*. Now there were far more nineteenth-century Irish immigrants than eighteenth-century ones, and if numbers were more important than solidarity, we would expect *do be* to have been fairly strong in nineteenth-century northern cities and to have diffused quite readily to northern blacks. But in fact the evidence for *be*, both now and in the past, is much stronger, suggesting that the nineteenth-century southern Irish users of *does be* had little linguistic influence on the black community in northern cities of the United States.

The issue is actually more complicated than the picture I've been able to paint here might suggest, because we have to take into account the effect of massive black migration from the South and remember that the evidence for creole-based *does be* habituais and decreolizing 's *be* and *be* forms is much stronger in parts of the South (see Rickford 1986 for more discussion). My overall point, however, is that the kinds of questions raised by the work of Labov, Bailey, and their colleagues should force us not only to re-examine synchronically and in real time what is happening in communities today but also to attempt to reconstruct the social relations and linguistic processes of earlier times—to make specific hypotheses and then try as hard as possible to test them with the data that's available. I think if we take both approaches and bear in mind all the points that have been raised in this panel discussion, the result will not only be to shed light on the sociolinguistic relations of blacks and whites in Philadelphia, but also to increase the scope of sociolinguistic analysis as a whole.

VIII. DISCUSSION

WILLIAM LABOV. The overall impression that the panel made on me was a constructive one, promising for future research. We had some very thoughtful comments and some very pointed questions. As you heard

from my opening statements, I was able to repair some of the limitations of our earlier presentations from some of the comments and criticisms that Fay Vaughn-Cooke and Arthur Spears had made at earlier meetings. I was also encouraged to find new input—a large body of data from Guy Bailey, who gave independent confirmation to our findings. Though we used different methods and dealt with completely different communities, there is convergence of results.

In the rest of my comments, I'd like to synthesize the remarks of Fay Vaughn-Cooke and Walt Wolfram, and deal with them as a single set of topics, weaving together the questions that they raised.

First of all, I want to make it clear to everyone that the press has been constructive, accurate, and helpful. No one in the field of linguistics went to the press and asked them to interview us. What actually happened here is that the title of a paper on "De Facto Segregation of Black and White Vernaculars" at Montclair State was noted by a reporter from the *New York Times*. The *Times* editors took the initiative and referred it to their Philadelphia reporter, William Stevens. They decided that this was news.

How accurate is the original news story? I received eight telephone calls from the editors of the *Times*, who wanted to be absolutely sure that the facts were as accurate as they could get them. Every quote that Fay Vaughn-Cooke has given you from Bill Stevens' article was an accurate representation of what I had told him. But as Fay talked, she translated his language into her own language—and lost that accuracy. For example, he accurately quoted me as saying that black English vernacular is diverging from other dialects, while Fay spoke about divergence from "standard English." We have not phrased the matter in terms of divergence from standard English; when black people use standard English, it is very close to everyone else's use of standard English. A large part of our findings, a good half, deals with the divergence of white vernaculars from black vernaculars, as the speakers of the white vernaculars move off in their own direction. It is only natural that the *Times* would be more accurate here. Fay is not interested in the divergence of white dialects—this is not her area of work—and so she quickly translated what Bill Stevens reported into her own preconception of the situation. Only those who are interested in the situation in the United States as a whole, and not in the particular situation of blacks, see that we're talking about a separation of two groups, and not only the development of BEV.

A second aspect of the reporting concerns the relation between convergence and divergence. In everything we have said, in every report to the press, we have stressed the importance of Fay Vaughn-Cooke's find-

ings, and John Rickford's findings, on evidence of decreolization and divergence. This has never been in question. The news reports were accurate in showing that linguistic evidence had demonstrated both divergence and convergence. The new story, as Arthur Spears pointed out, is that something has occurred that is different from expectation, and from a social point of view, that divergence is crucial. It is worth noting that all the accounts of decreolization are based on work in areas of the South that are not highly industrialized. In most of the rural areas where the population is diminishing, we find that young people use fewer vernacular forms than their parents, while in the large northern cities, we find the reverse.

A number of people on the panel (particularly Fay) refer to "claims." I will be very embarrassed if you find the word *claim* in what we have written. It is not the spirit of our work to CLAIM anything. This is a term taken from formal and philosophical grammar. What we try to do is to report as accurately as we can what we found and see what the implications are for further work. So it isn't necessary to say that every aspect of the black English vernacular is changing in order to say that there is divergence.

Now let's get down to the substantive question that Fay and Walt raised about comparability of data bases. Almost everyone has difficulty in dealing with the literature on this question, because the study of change in progress is a relatively new undertaking. At the same time, we have to remember that Gauchat (1905) did the first study of sound change in progress in 1899, and his work was followed up thirty years later by Hermann in 1929. Hermann did confirm that four of the six sound changes studied by Gauchat in apparent time had continued in real time, but two had not: these were cases of age-grading. Since that time, every scholar alert to the issues has searched for comparable points in real time to confirm any indications of ongoing change. (It is not impossible to make inferences about change in progress from synchronic data, particularly where sound change is involved. Labov [1981] presents a number of possibilities. The situation is particularly favorable for phonology since there is evidence that most people preserve the phonological system of their youth throughout life.)

The difficulties of dealing with real-time data are considerable. We would like to have studies that are absolutely comparable, but there are many reasons why this is rare. First, there are technical advances, like the tape recorder or linear predictive coding, and it would be foolish to throw away the new techniques just to get comparability with older data. Secondly, we find that earlier linguists were not usually interested in the

questions that concern us today and did not design their studies to suit us any more than future generations will design their work around our interests. As a result, we cannot make QUANTITATIVE comparisons with earlier work; it would be unwise to draw inferences about change in progress from the fact that phoneticians noted lower variants of a vowel thirty years ago or reported that consonant cluster simplification was common. Unless the investigators and the methods are the same, we have to look for QUALITATIVE differences between earlier and later work.

Let us examine the case of Martha's Vineyard, since that has been cited as a model. I observed the centralization of /ay/ and /aw/, with striking distributions across three generations that suggested change in progress. The Linguistic Atlas of New England data had been gathered by Guy Loman in 1933, twenty-eight years before. When I tried to interpret the Atlas data on the centralization of /ay/ by matching Loman's use of IPA symbols with mine, it appeared that his four informants, age 56 to 82, had a little more centralization than my oldest informants, but less than my middle-aged informants. The situation was quite confused. But the qualitative data on /aw/ was quite conclusive. The Atlas informants showed no centralization of /aw/ at all; the only alternations had to do with fronting of the nucleus. Similar situations appeared in New York City, where the oldest observations dated back to 1896: only qualitative observations of same and different allowed us to draw conclusions about the state of the system. In Philadelphia, we are fortunate to find that R. Whitney Tucker observed in 1944 that Philadelphians made no difference at all in the vowel quality of *sight* and *side*, though he did himself. From such evidence we can conclude that the strong centralization of *sight* in the city today is a part of an ongoing sound change.

In the case of BEV /s/, we have no earlier records in Philadelphia. Our best comparison will be with the work done in New York in 1965–1968. Most of our Philadelphia sample is older, among the males, though the female members of the core group are about the same age as the Jets, the Cobras, and the Oscar Brothers in New York City. We did not design our Philadelphia sample to be an exact match to the New York City sample. Given the powerful effect of observation on BEV, our primary concern had to be a solution to the Observer's Paradox. We will be comparing the work of two gifted field workers—John Lewis and Wendell Harris, who both recorded several hundred speakers, and both bypassed the effects of observation with great effectiveness. Their major tool was participant-observation, combined with an intense development of recording techniques, particularly group sessions, that overcome the usual constraints on the emergence of the vernacular. In the final analysis, the

Philadelphia core group showed higher frequencies of basilectal variants than the New York City core group. In other respects, the two cities showed the same qualitative and quantitative features. For example, the fine-grained influence of the following grammatical environment on the contraction and deletion of the copula that Baugh found in the New York Cobra data was exactly replicated among the Philadelphia core speakers.

The new third singular /s/ pattern among the younger Philadelphia speakers had no correlate in New York City in the 1960s. In fact, the /s/ of the historical present was rarely heard among the New York City speakers of BEV, and we speculated at the time that this absence of the historical present may have been coupled with the absence of third singular /s/.

We then have three qualitative differences that point towards the existence of change in real time.

First, as Myhill and Harris reported, no Philadelphia speaker over 40 has shown any trace of the specialization of third singular /s/ in past narrative. On the other hand, all the younger members of the core group of North Philadelphians that we studied show this pattern. The apparent time contrast is not a quantitative fact but a qualitative one.

Secondly, no New York City speaker of any age showed such a pattern, including the youth of comparable age and social background to the Philadelphia speakers.

Thirdly, it has often been suggested in discussions of the role of verbal /s/ in BEV that it serves as an emphatic signal, a mark of formal English, or as a durative tense, but no one has suggested that it serves as a marker of past tense narrative.

Our chief basis for comparison in real time is not as strong as it could be, since there is a geographic difference in the two samples—Philadelphia vs. New York. Our main interest is not in how or where the new development arose, but rather in the creation of structural rules that have no existence in other dialects. The core speakers of BEV in Philadelphia are innovating in a way that's possible only for people who are working in an isolated system.

While the comments about improving the comparisons in apparent-time and real-time are interesting and valuable, it might be worth pointing out that no one on the panel except Bailey has addressed the major issues that our research has raised:

(1). What are the causes of the continued differentiation of white dialects from network English and from each other, and why do black speakers not participate in this process?

(2). Why is it that not a single description of the tense and aspect system of West Africa or the Caribbean has found particles with the semantics of habitual *be*, stressed *been*, or *be done*?

No matter when and how these differentiating processes began, it is evident that increased differentiation took place within the United States in fairly recent times.

Finally, I'd like to turn to the educational impact of this work in relation to the media. In looking ahead to future research, we stated that we were not ruling out the possibility that the divergence of black and white dialects, particularly the increasing phonetic divergence, could be a cause of misunderstanding within the schools. Some reporters took that possibility and tried to turn it into a fact. But the cognitive problems that accompany language differences are not recent discoveries. In speaking of the problem of translation between BEV and other dialects, I was citing Gary Simpkins, one of the three authors of *Bridge*. This program is one of the most powerful and effective means of helping black children learn to read; it provides the necessary linguistic and cultural transition from the vernacular to the standard English of the classroom. Simpkin's testimony at the Ann Arbor trial stressed the additional cognitive problem facing black children who have to translate a widely different dialect into their own system as they listen and reverse this process as they speak within the classroom setting.

The social situation that we're addressing here is not an academic matter: it is real and pressing. The findings of our research are the linguistic reflections of the increasing segregation in northern cities. The Philadelphia Social History Project has demonstrated how blacks are differentiated from all other groups over the past century and a half in the pattern of increasing residential segregation and unemployment (Herschberg et al 1981). This is the result of what Bailey and Maynor call the largest single dislocation of population in the United States: the movement of blacks from the southern states to the inner cities of the North. Reporters who saw the linguistic data as interesting were reacting to the knowledge that things are not going well in these inner cities: the policy of benign neglect is going to lead to a lower quality of life for everybody. The linguistic divergence that we've seen here demonstrates the results of that policy. It is not a total divergence, but insofar as it takes place at all, it reflects an increasingly grim social reality.

FAY VAUGHN-COOKE. There are a couple of things that I want to respond to. First, Bill Labov has just said that I reinterpreted his statements about black English becoming more and more different from

other dialects, saying that he had asserted that black English was becoming more different from standard English. Well, I was quoting the *New York Times* article (1985) and that's exactly what he says. As a matter of fact I have it here, and it says, "The black vernacular, a recent study has concluded, appears to be steadily diverging not only from standard English"—the whole issue here is about black English and standard English. And it's throughout the articles—that is really what the press is interested in; they're not interested in white dialects spoken by working class whites and so on, they're interested in black English versus standard English.

LABOV. On the contrary, following this there were an equal number of articles which were equally interested in our finding that white vernaculars are diverging from each other. Stevens spent twice as long writing any article on white vernaculars as he did this one.

VAUGHN-COOKE. Well, the impression that he has given here, quoting you, has to do with the divergence of black and standard English. I'm thinking about the impact when the public sees this.

LABOV. Let's get the whole quotation then. "The black vernacular, recent studies concluded, appears to be steadily diverging not only from standard English as spoken for example by radio and television announcers, but also from local and regional white dialects. These dialects are themselves moving in the direction of separation both from the black vernacular and the American standard." So it seems to me that is giving equal and accurate time to both aspects of this phenomenon: that the white and black vernaculars are moving apart.

VAUGHN-COOKE. Well, the point is "standard English" is in here. He mentions standard English, so I don't feel that I was incorrect in actually saying that the article said that black English was diverging from standard English.

The other point that I want to make is about Labov's concern with my use of the word *claim*. Let's throw it out, I don't care—we can use *statement*. My concern is not with the term, it is with the lack of evidence. And I still want Bill Labov to answer my question: why didn't he have data that span generations?

LABOV. Fay, our overall sample of two hundred and fifty speakers and the subsample of thirty-five that we've studied most carefully both span three generations. The youngest of them are fifteen, since we didn't go down into the preadolescent series, and the oldest of them is 83. We spent six months studying the senior citizens at great length—and the findings on their third singular /s/ were qualitatively different from the findings on the younger speakers. Perhaps that didn't receive as much

attention in the reports you've seen. We also have a massive data base in New York City. When we started to make comparisons with it, we found that the differences were qualitative. The major effort of our research was on the black and white boundary: within the black and white community, what are the consequences of contacts between them? That was our major focus. But certainly we span those generations, as was reported in Myhill and Harris (1983) and elsewhere, and I think you are right in calling attention to the fact that we didn't report it as clearly as we should have in the newspaper articles.

VAUGHN-COOKE. OK, I didn't think I had misinterpreted that. The other point that I want to comment on has to do with the implications for education. Now that's always been a concern of linguistics, and I think that it has been very fulfilling to me as a linguist and to a lot of my colleagues that our work is somehow related to the real world. I've been very much rewarded by that connection. The point, though, I think, is that we have a very serious responsibility. We have to be cautious if we are going to make recommendations regarding what should be done in the schools, what should be done with black children in reading, and so on. We ourselves should know where we are going and have the evidence to support the statements that we make or the positions that we take. The thrust of a number of the articles that have appeared in papers around the country was that, given this increasing divergence, we must consider the implications this has for children in schools—for reading. For example, in the Williams (1985) article in the *Washington Post*, Labov is quoted as saying, "young black children from the inner city who must deal with the language of the classroom are faced with the task of understanding a form of language that is increasingly different from their own." Now, that is so general, the way it's stated, it doesn't say "with respect to certain features" and so on. A teacher reading this could certainly interpret that as 'a language that may be unintelligible' to her, 'maybe this is the reason why black children are failing in school'. I think we'd do well to consider the comments just made by Spears and also the 1981 article by Fasold in *American Speech* on the relations between black/white speech in the South, in which Fasold urged us to look elsewhere. I felt a sense of hope when I read that. Williams' article seems to me to be taking us back in a direction where we have already gone and have never made a whole lot of progress; that is, he is going back to a hypothesis about comprehension and translation problems and so on—and the fact is the data that it's based on doesn't even prove that there is divergence, certainly not this massive divergence that would lead to the kinds of problems that are implied here from the research. On a related point, there is another article in the *Baltimore Sun*

(1985) which states that “the English of inner-city blacks hurts their progress.” This writer is going even one step further and saying that the kind of English inner-city blacks speak is not only going to cause problems in school, it’s also going to hurt their progress! Can you imagine how some employer, who might already be looking for an excuse not to hire someone who is a black English speaker, could use this against black English speakers in terms of, say, deficiencies that they might have once they attempt to enter certain kinds of job markets? The point that I’m trying to make is that we need to be very cautious, particularly with the press, because the impression that I get from many of the newspaper articles about language is that the press is looking for what is dramatic—for what’s going to generate a reaction. That isn’t always what OUR intentions are, but once the writers publish their articles, then we have no more control over what happens.

Concerning the Simpkins work that Labov mentioned on *Bridge*: I’m not aware of any evidence that they actually presented which provided convincing information about cognitive translation and cognitive differences. I think that what we are seeing with black English are differences in form. The work that I’m conducting right now with Ida Stockman and Wilhelmina Reveron at the University of District of Columbia and at the Center for Applied Linguistics shows that the underlying semantic categories that black children exhibit are basically like that of all other children, even those middle class speakers in the studies that have been done on middle class children. We compared our results with Lois Bloom’s data, and Roger Brown’s, and Peggy Miller’s working class data, and Ira Blake’s data. What we found is that there certainly shouldn’t be any problems in terms of a translation on a cognitive level. Any problem would come possibly in that subset of forms that differ from standard English. I don’t see that as being an issue that would be broad enough or large enough that we would need to make sweeping claims about the educational implications of linguistic differences for reading and writing.

LABOV. I’d like respond to the question of whether linguists can apply their knowledge of language to influence the curriculum or whether they should really be working on improving attitudes towards language: as you know, our early work said that the problem was largely a political conflict in the schools, and we all agree about that. The question is, what can we as linguists do? Many people have taken the results of published work, particularly statements of Ken Goodman, to say that when people have examined the questions of whether dialect differences could influence reading they have found that it does not. I would ask you to obtain Ken Goodman’s actual report and look at the charts and tables that com-

pare the performance of black people with Hawaiians, Indians, and others, and you will see that the percentage of errors associated with the black vernacular is much higher for that group than any other. I think that it has been premature to turn away from our knowledge of language differences and conclude erroneously that linguistic knowledge is not relevant. I don't think linguistic knowledge is the whole story, but I hope that we continue to use what we know. And I'd agree with what Fay said in that a formal difference may not be a difference in the semantic situation. However, the third singular -s, though it has no semantic content except that of subject-verb concord, would be considered by many people to present a serious cognitive problem to a reader who had to account for this signal on the page.

ARTHUR K. SPEARS. Going back to the issue of convergence and divergence and the fact that we must ultimately compare whole dialects (or whole languages) rather than specific features, I urge that we recognize that there's simply a lot we don't know, not only about black and white dialects of English, but also—and this is very important—about the creole grammatical systems that black English in particular is being compared with.

One of the things that I would hasten to point out is that some of the more recent, more detailed studies of tense, mood, and aspect of the verbal systems in creole languages show that these verbal systems involve a lot more than, say, Bickerton's classic creole tense, mood, and aspect system. What this means is that, if more is going on in these creole languages than we yet know, then, when we say that a certain grammatical feature is a new development in black English because we don't see it in creoles, or that it's evidence of divergence, we're making such statements based on insufficient evidence. My research on Haitian creole, for example, shows that the verbal system is much more complex than what we've recently believed. So we have to hesitate, to qualify our findings. There is always room for additional supporting evidence.

One reason we have gotten ourselves into this situation, questioning the *EXTENT* of convergence or divergence among vernaculars, is that sociolinguistics has typically approached these dialects through the study of variables, which interest us, of course, because they tell a lot about the connections between language and social structure. But, they don't give us a broad picture of what the language is all about. A few exceptions aside (pronominal systems, for example), they don't give us a complete view of grammatical subsystems. What we really need—and this is certainly truer of creole languages than black and white dialects of American English where there is so much overlap—is broad descriptive studies. With such studies, we would really know what we're comparing.

When we talked about elements in the black English verbal system, we would really know something fundamental about the nature of creole verbal systems, and we'd be able to make valid comparisons.

RALPH FASOLD. At this point, is there anyone in the general audience who would like to direct a question to a member of the panel?

ALBERT MOSLEY (*University of the District of Columbia*). I just want a clarification. On the one hand, I keep hearing about a divergence between black dialects and standard English. On the other hand, I thought I understood Labov to say something about the divergence being between Philadelphia and New York? Is that right? In other words, how do we get the generalization to black dialects in general?

LABOV. What seems to be happening is this. For most of the grammatical features that we've been talking about, we're getting roughly the same report—very little difference from Los Angeles, Chicago, Washington, Boston, Philadelphia, New York. The black vernacular shows extraordinary homogeneity with only minor differences, and that raises the question as to whether this finding in Philadelphia is a local development or whether it can be found elsewhere. In all of these cities we're talking about, the black English vernacular shows roughly the same phonology with small differences and the same grammar, while we have a white local accent that is developing and diverging so that Chicago is moving in one direction, New York in another, Philadelphia in a third, Pittsburgh in a fourth. The blacks do not participate in this process, so that as the white dialects are diverging from each other, they are also diverging from blacks everywhere. Let me give you a concrete example. Black people in Philadelphia as in most places will say *No doubt about it*, and the /aw/ of *doubt* and *about* is well back of center—[ɑ^o]. The older, conservative, white Philadelphians say *No doubt about it* with the /aw/ fronted—[æ^o]. This was the subject of one of our controlled experiments—we analyzed the vowels of a black speaker, altered the second formant to shift [ɑ^o] to [æ^o], and resynthesized it. In matched guise tests, this alteration of vowels changed the ethnic identification of the speaker for members of the Philadelphia community. Now these two pronunciations are not very far apart, but the younger people of Philadelphia don't say [æ^ot] they say [ɛ^o:t] and [e:ɔt], so that the distance between [e:ɔt] and [ɑ^ot] is much, much greater than the distance between [æ^ot] and [ɑ^ot]. This is only one of many such cases. Presently, we're investigating the question of whether these extraordinary shifts of vowels tend to lead to misunderstanding between children and adults in the schools.

These northern cities are like a sandwich. There's a group that's na-

tionally oriented, such as lawyers and professors, and their speech is very similar from city to city. And you have what's called the external proletariat—the Hispanics and the blacks, who are very similar nationally. And in between you get the local culture, highly differentiated. These are the people holding most of the things that are prized in the society: the houses and the jobs.

JOHN RICKFORD. I want to make one further remark. With all the qualifications and counterpossibilities, I think we have to admit, and in a sense haven't admitted from the way the data has been presented and talked about, that what we really lack is comparable reference points in real time—data from comparable cities collected by comparable methods. The current work of Labov and Bailey and their colleagues is establishing a vital reference point now for FUTURE comparisons, but I think it also should prompt us to go back to other places like New York, where we've worked before, and look to see whether we have comparable data. Walt Wolfram can defend himself very well, I'm sure, but I don't think he meant that Labov should ask his field workers to hold back by any means. What Walt was saying, I think, is, "Gosh, I wish the fieldworkers of ten or twenty years ago had *not* held back!" And he was also saying, I believe, "Well, let's look at the methods that we used ten years ago and maybe use some of these comparable methods in other communities."

The point is, we've all been well trained by Bill Labov, and by other leaders in the field, to question whether apparent-time data can itself provide incontrovertible evidence of divergence. We might suspect, from how we see the sociopolitical variables operating, that black and white vernaculars might be moving further apart, but we need to retain that intellectual caution which Labov helped us to develop, and to say, "But look, we don't have the evidence here." If I had done a dissertation for Labov on this subject six years ago without reference points in real time, Labov would have said "John, you've got a big problem here." So I think what we're trying to say is that we may have a similar problem with the data that's now emerging.

LABOV. I'll agree, John, because I think in all our methods we have to struggle between comparability and getting deeper into the local situation. We always try to get a compromise of a sort. And the compromise sometimes lets us down when we need that comparability the most. So, I agree it is an unsolved problem.

WALT WOLFRAM. I probably shouldn't open my mouth again, but that never stopped me before so I'll do it once again.

I raised the issues of comparability, evidence, and language change because I think it is important to identify the kind of data still needed to

resolve the controversy. That way, we can proceed to gather the necessary data. We need to pinpoint the problem areas and the kinds of data that will resolve the remaining issues. I mentioned this point to Guy Bailey when I told him that there are two kinds of data that I needed to see in order to accept the conclusions of his paper. One is the need for a middle-aged sample of speakers that his sample lacks. This missing sample might help us to determine whether the differences between old and young speakers are authentic changes as opposed to age-grading. The other need is comparable interview topics in which habitual *be* might occur. Children talking about games is a present-time, habitual activity, whereas adults talking about these activities involves a past-time dimension. Since so many of the examples for the children come from childhood games, we need to resolve this problem of comparability for the potential use of *be*.

We must resist the temptation to say that we have sufficient and conclusive evidence, and be open to the need for additional or different kinds of data to resolve critical dimensions of the divergence issue. Academics are often too reticent to speak of the limitations still found in their studies and what is needed to overcome the limitations. I think we need to say, "Here's what data we still need; now we're going to go out and get the necessary data." I personally would campaign for a commitment to go get the necessary data so that the present set of objections can be adequately answered.

LABOV. I want to remind everybody that at last year's NWave Henrietta Cedergren reported upon a restudy of Panama City, and she has done exactly what Walt Wolfram is asking. Peter Trudgill has gone back to Norwich and restudied his sample and will be reporting on this in the near future.

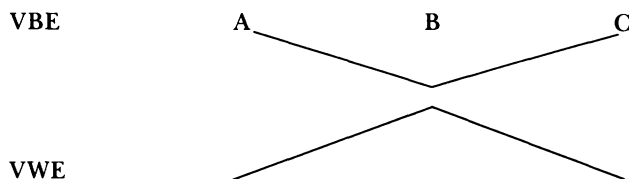
FASOLD. Are there other questions from the floor?

DENISE BORDERS (*McGraw-Hill Publishers*). Vaughn-Cooke and Labov have touched upon an important point, the application of sociolinguistic research to classrooms. We know that there are problems with black children in schools, and we need to know what those problems have to do with linguistic forms. My colleague and I, Phil Lucas, did a study in Washington, D.C., in one school looking at three different classrooms and three different age groups—kindergarten, fourth grade, and sixth grade. And we did a quantitative analysis of dialect features in regular classroom events—of language in use in classrooms. We found that the dialect features basically occur in situations where the teacher is not present. Students are using standard English when there's a formal lesson

and the teacher is present—but they're also not talking very much at all. When a lot of talk occurs a lot of dialect occurs, but teachers and students are understanding each other. So we can't say anything, I think, about communication, but when it comes to reading and writing and cognitive issues in terms of translation and making sense of talk written down that is not spoken, then I think that we need to go a step further—but in documenting dialect features in actual use in classrooms, we can't find any evidence of interference in communication. However, interference in reading and writing is another issue. The point is, applications can be made but we need to get into classrooms and look at language in use rather than in lots of other settings. And context had a tremendous effect on the use of dialect.

GUY BAILEY. Walt Wolfram has raised two questions which I'd like to address. First, he points out that most of the instances of *be*₂ in the speech of children occur in answers to questions about childhood games and activities. He notes that if the same questions were asked of elderly adults (and they were), the adults would be likely to answer with responses in the past tense, thus skewing the frequency of *be*₂. He is right to some extent: the larger number of *be*₂ tokens in the speech of children is in part the result of questioning strategies. However, my argument is based not on differences in the frequency of *be*₂ but on differences in the grammatical function of the form. Even if discussions of childhood games are eliminated, the same contrast before *V + -ing* exists in the speech of children, but not in the other varieties. A comparison with our Mississippi sample will make this point clearer. In obtaining interviews with children in Mississippi, Natalie Maynor was forced to rely on school teachers to gather the data. In interviewing the children, these teachers used an entirely different set of topics, structuring most of the conversation around what the children were expecting for Christmas. When we began to analyze the data from these interviews, we thought that we had found a situation entirely different from the one in Texas. *Be*₂ accounted for only about three percent of the total corpus (as opposed to ten percent in the speech of the Texas children) and only eleven percent of the tokens before *V + -ing* (as opposed to forty-four percent in the Texas children). Nevertheless, as we began to examine individual tokens, the same contrast between *be*₂ and \emptyset emerged. There simply weren't many instances where *be*₂ could occur. Wolfram is right, then, in suggesting that questioning strategies may affect the frequency of occurrence of *be*₂. Those strategies, however, do not account for the systematic contrast between *be*₂ and \emptyset before *V + -ing* which exists in the speech of children but not in earlier varieties.

FIGURE 8
Convergence/Divergence



Wolfram and Rickford also have presented useful diagrams which summarize possible positions on the developing relationships between the black and white vernaculars. However, they omit the position that I take, which I've schematized in figure 8. My position is that for quite a long period, the black and white vernaculars, at least in the South, were on paths of mutual convergence. Over the last seventy-five years, these varieties have been on paths of mutual divergence.

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