

SOCIAL DIALECTOLOGY: A PLEA FOR MORE DATA*

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In the *Grammar* of his home dialect of Windhill in Yorkshire, published in 1892, Joseph Wright presented a vowel system that had developed "correctly" from the vowel systems of Old and Middle English. According to Wright, someone who was born in a certain area, had lived there all his life and had been exposed only to the linguistic influences of this limited area had to mirror correctly this local speech pattern. If he deviated from this norm and used 'unexpected' forms — Wright noticed this (cf. his *English dialect grammar*, pp. IV and V); however, he suppressed it in his *Windhill-Grammar* — he was making "mistakes" — for which there was no room in the description of a "pure, homogeneous" dialect. Wright was not aware of the fact that a speaker could make use of several linguistic registers and that his way of speaking could be influenced by a number of factors. Consequently, the result was an idealized description of a dialect in which every dialectal sound could be derived correctly from a Middle English predecessor. Quite a few descriptions of English dialects that followed used Wright's analysis as a model.

We meet idealizations in linguistics also later, even if different premises underlie them. With regard to linguistic theory certain idealizations are no doubt necessary. Ideally, linguistic data obtained on a sound basis should condition linguistic theories and models. This is self-evident to anyone working in the field of linguistic variation, one should think. Linguistic theory needs a strong empirical basis. "We will concede that it is desirable to have the most elegant theoretical analysis possible. Nevertheless, data must not be buried in an elegant symmetry. If the theory will not accommodate the data, then the theory must be modified in some way, or a new theory must be

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devised", as R. I. McDavid, Jr., has put it elsewhere. Symptomatic of the method, however, employed in several linguistic quarters nowadays seems, regrettably, to be a book by Burgschmidt and Götz, published in 1974, on contrastive linguistics: the first five chapters, i.e. almost three quarters of the book, are concerned with theoretical questions. Only at the end of the book does the reader get some information on actual differences between German and English. The arrangement and the weighting of both parts of the book point in the same direction: Theories exist prior to linguistic data!

Yet also in a theory-dominated time there existed the "dull cataloguer of data", to use Robert B. Lees's 'famous' remark. Even though today *data* is the most obscene of the four-letter words for many linguists, we have witnessed in recent years a significant change insofar as the "dull cataloguer of data" no longer leads a shadowy existence now. In some linguistic areas he even begins to dominate the scene. The analysis of the "mistakes" in the sense of Joseph Wright has come of age. Even in the area of formal grammar the study of language as a social instrument does not remain without influence. Chomsky's theses of the "well-formedness" of "grammatical" "sentences" were criticized increasingly in the light of linguistic reality, i.e. also linguistic variability. There is no way of talking about grammaticality or well-formedness without getting involved in the intricate details of social interaction by means of language.

In the area of linguistic variation in English it is, perhaps, advantageous to differentiate with Aarts (1976) between two directions of present-day research — one that deals primarily with language *structure* and the other that is mainly concerned with the investigation of *usage*. As regards the direction mentioned first, we owe the most detailed synchronic description of English to the collaborators of the London *Survey of English usage*. Quirk's *Grammar of contemporary English*, published in 1972, is characteristically restricted to 'educated English'. It is a so-called 'common-core' grammar of "educated English current in the second half of the twentieth century in the world's major English-speaking communities" (p. V), i.e. it is made clear that *the English language does not exist*. It is rather a heterogeneous formation; the common core of all 'educated variants' is described here. On the American side there is nothing comparable to this *Grammar*. Instead, American scholars are concerned above all with problems of *usage* trying to cover the complete linguistic reality in concrete situations. (It was, above all, Trudgill 1974 who introduced this kind of research in Great Britain.) Most of these sociolinguistic studies also claim to be structurally oriented. However, there is an important difference between this type of study and the just mentioned *Grammar* by Quirk and his associates — which, by the way, is concerned almost exclusively with syntax — in that the sociolinguistic studies investigate the social stratification of certain elements which are not related with one another.

In other words, they are extremely taxonomic. The first survey of this school of thought in the United States we owe to Wolfram and Fasold. In their book *The study of social dialects in American English*, published in 1974, they provide in chapter 2, entitled "Social dialects as a field of inquiry", an historically oriented overview — which is rather unfair. They discuss Charles C. Fries's *American English grammar* (1940) in half a page, the activities of the American linguistic geographers in less than two pages and then describe at great length the 'new type' of dialect studies under the heading "Quantitative studies" — as if Fries's *Grammar* had not been quantitative. However, he also quantified the results of his empirical study; in fact, we owe a number of important insights to Fries. But, it is true, only a few people listened to him and hardly anybody in the schools, to whom he had mainly addressed himself.

As regards the criteria according to which Fries classified his informants — they are almost identical with those of the *Linguistic atlas*. Of course, they do not suit the taste of modern sociolinguists — for them they lack objectivity. Modern sociolinguists rather stick to objectively measurable factors, such as income and house type, which they then multiply differently without giving any reasons for this. This is the procedure followed, among others, by Shuy, Wolfram and Riley in their *Detroit Dialect Survey* (1968). Here as well as in similar studies of a more recent date the procedure is binary: two races are distinguished, two classes — a middle and a working class — and these two classes are again subdivided each into an upper and a lower class. Whether a division according to purely mechanical factors, as the ones mentioned, is better than to ask — as the *Linguistic atlas* does — for factors such as church membership, leisure-time activities and community organizations is at least an open question. The latter-named factors may have a more direct bearing on social class than a simple measurement of economic factors.

Still, there is not the slightest doubt about the importance of the dialect studies of the new type. Although quite a few of them do not come up to their self-imposed standards — one should not let oneself be deceived by the statistically ascertained results which give a very reliable and exact impression — some of these studies have affected linguistic theory a great deal — above all the work carried out by William Labov. Right from the beginning he had the consequences of his research for linguistic theory in mind. In *The social stratification of English in New York City* (1966) he says clearly: "In the past few years, there has been considerable programmatic discussion of *sociolinguistics* at various meetings and symposia. If this term refers to the use of data from the speech community to solve problems of linguistic theory, then I would agree that it applies to the research described here. But *sociolinguistics* is more frequently used to suggest a new interdisciplinary field — the comprehensive description of the relations of language and society. This seems to me an unfortunate notion, foreshadowing a long series of purely descriptive

studies with little bearing on the central theoretical problems of linguistics or of sociology" (p. V).

Thus Labov's work must also be seen as a reaction against those areas which had before been excluded from serious consideration by *many* authorities in US linguistics, such as the field of language change or the problem whether linguistic variants are really "free".

Labov and Chomsky hold completely opposite views. (We deliberately exclude the Prague school from our discussion here.) Chomsky is concerned with anti-sociolinguistic idealizations of the ideal speaker-listener and of the homogeneous speech of the speaker-listener. In his *Aspects of the theory of syntax* of 1965 Chomsky says: "A grammar of a language purports to be a description of the ideal speaker-hearer's intrinsic competence" (p. 4). According to Chomsky, the grammar of *one* member of a homogeneous and static speech community is thus identical with that of every other member of the same speech community. Thus interpersonal (and intrapersonal) linguistic variation is completely ignored. On the other hand, Labov maintains that "the construction of complete grammars for 'idiolects', even one's own, is a fruitless and unrewarding task; we now know enough about language in its social context to realize that the grammar of the speech community is more regular and systematic than the behavior of any one individual", as he puts it in his article "Contraction, deletion, and inherent variability of the English copula", published in *Language* in 1969. This, again, is an idealization, even if different from the one proposed by Chomsky, for Labov's grammars make far too great demands on the human memory. This becomes especially noticeable with Labov's variable rules. If, for example, a certain element is deleted in certain speakers in 75.2% of all cases, then a rule is postulated that deletes this element in these speakers in 75.2% of all possible applications. It has already been pointed out (e.g. by Bickerton 1971) that it is completely impossible to learn a language in this way as the numbers of different variability are far too great and unsystematic. Later Labov changed his variable rules: the percentage figures that originally appeared as index numbers were abandoned in favour of a hierarchical arrangement in which variability is taken into account in that way that some rules are more optional than others. Apart from the fact that the number of such relations is still enormously high, the question arises whether these variable rules are really capable of what they seem fit in their adherents' opinion, namely to predict precisely the linguistic behaviour of *any one* member of the group from which the speech data were drawn. Some scholars were, it is true, somewhat cautious with this far-reaching claim, while others postulated it bluntly. To the latter group belong Cedergren and Sankoff (1974), who declare that the variable rule "accurately predicts the behavior of each individual" (p. 335). It is exactly this precise predictability of linguistic behaviour which must be doubted

very much in view of the fact that two speakers belonging to the *same social class* — however defined — show great differences with regard to the use of a certain variable. Wolfram in his analysis of Detroit Negro speech, published in 1969, provides significant examples of this. One must therefore ask with Butters 1973 (1975) whether Labov did not simply set a statistically idealized speaker-listener against Chomsky's idealized speaker-listener. In any case, linguistic variability of *individual* speakers deserves thorough investigation. Another objection against Labov's approach — rightly pointed out already by Dressler (1976) — lies in the problem of the perception of the numerous, often only *fine-grained* frequencies and their immediate 'consumption' by the hearer. This asserted capability is the more surprising when Labov concludes from subjective reaction tests "that stigmatized features are not overtly perceived up to a certain frequency, and heard beyond a certain frequency, as occurring all the time" (1971: 470). As Labov himself found out, the classification of a speaker according to various sociolinguistic criteria takes place so fast — in his subjective reaction tests Labov used only very short speech samples of every classifiable person — that no representative probabilities could have possibly been attained by the hearer (cf. also Dressler 1976: 61).

Despite extensive empirical investigations carried out by Labov and his followers many questions, as we have seen, still remain unanswered. To this list also belong the following: how many styles have to be differentiated, how are they to be determined and how are we to differentiate between stylistically conditioned and inherent variability. It is true that Labov explains linguistic variation within the same situation as inherent variability; on the other hand, he also takes dialect mixture into account without, however, stating the limits of the applicability of this principle.

Fries's *American English grammar* and the 'new type' of dialect studies have in spite of all the undeniable differences revealed as a common result the fact that speakers belonging to different social classes mostly differ from each other not by the rigorous use versus non-use of certain investigated linguistic features but only by differences in frequency. Linguistic features often found in the lower social class are also heard, although more rarely, in the middle and upper social classes. Thus Detroit Negroes of *all* social classes use, e.g. *he work* and *he works* and analogous forms, although with different frequencies. In other words, it is wrong to allot certain *closed* linguistic systems, which are so readily postulated by many linguists, to certain social classes. The boundaries — linguistic as well as social — are in reality not fixed. Such 'systems', whenever postulated, are nothing but abstractions from real life and allotting them to certain social classes shows an undue simplification.

In the foregoing remarks we referred once or twice briefly to the English

spoken by Negroes in the United States. This Black English, so-called, has received increasing attention during the last ten or so years by dialectologists and sociolinguists, thus ignoring other underprivileged groups which outnumber the Afro-Americans by far. It is to be hoped that the linguistic research carried out in the area of the disadvantaged will soon give up this largely one-sided orientation in favour of the Americans of African descent. That Black English is so much discussed today by linguists and educationists alike is not so much justified numerically, but is rather politically motivated. This discussion is often led with more heat than reason and it is necessary to separate fact from fancy.

Mainly two opinions are put forward in this matter nowadays which are unreconcilable with one another. These had their advocates already in the 1930's. We refer in passing here only to the discriminations on the part of the Whites who attributed the English of the Negroes to their low intelligence, and to the 'fact', so-called, that their speech organs had not been properly developed. Consequently, Negroes spoke a corrupt English which resembled *baby talk*. Such views were expressed in the relevant literature rather early, i.e. already during the early years of the twentieth century. At that time "the assumption of the innate and inherited inferiority of non-Anglo-Saxon racial and ethnic groups permeated and dominated intellectual as well as popular thought" in the United States. "Social, scientific and historical thought both mirrored and reinforced this racism" (Yetman 1967: 538). Unfortunately, the above-mentioned views can still be heard today. Of the two opinions referred to earlier, one, perhaps the older view, is advanced by American dialectologists who maintain that the English spoken by *lower class* Negroes — the Negroes of the middle and upper middle classes speak Standard English just as the Whites of a comparable social level do — does not differ *typologically* from that of the Whites of the same social and regional background. Black and white speech exhibit the same range of variants, differing at best in frequency. (See the publications of, among others, Juanita Williamson 1972 and 1973, herself a black scholar.) Further, the origin of this kind of English can be traced back to British English and British English dialects. Since for economic reasons Negroes migrated in considerable numbers from the *rural* Southern United States to the *urban* areas in the North and West (roughly within the last sixty years) and naturally took their modes of speech with them, it is possible to speak of a *de facto* Black English in the North of the United States — which in effect is Southern (white) American English. The linguistic geographers tolerate one exception: that is Gullah or Geechee, a creolized variety of English still spoken on the South Carolina and Georgia coasts and on the nearby Sea Islands. The creolists, on the other hand, scholars like William Stewart (1966—67, 1967, 1968) and Beryl L. Bailey (1965), maintain that Black English is a language altogether different from

White English because its *deep structure* is different, having its origins in some proto-creole grammatical structure. This is asserted because of some structural similarities between Gullah and some creoles spoken in the Caribbean. Consequently, the British English/British dialect-based origin of Black English is strictly denied. According to the creolists, features of Black English can ultimately be derived from West African languages. Stewart believes that Black English in the rest of the United States, except Gullah that is, is now in a state of decreolization in which creole features are gradually lost. He tries to "prove" the different stages in the development of Black English by unearthing mainly 18th and 19th century attestations of Negro speech in works of literature — which of all the available evidence is the most unreliable source! It is, above all, in the diachronic area where intensive research is still needed before conclusions can be drawn. Apart from Negro speech attestations in works of literature, newspaper advertisements concerning runaway slaves were investigated (cf. Read 1916 and 1939). Of course, no direct conclusions on the nature of early Black English should be drawn from such studies. Furthermore, letters written by Negroes before the Civil War were analysed (cf. Eliason 1956); but these were letters written to their masters, not to fellow slaves. The best available diachronic evidence seems to have hardly been touched so far (an exception is Brewer 1973 [1975]). It consists of 2300 ex-slave narratives. In the 1930's interviews had been conducted in seventeen states with old, mainly rural Blacks who before emancipation had experienced slavery themselves. These vivid reminiscences from ex-slaves have several advantages for a study of Black English: the material "covers a time span extending back to the middle of the nineteenth century [and] it covers a wide geographical area... The narratives were written down in long-hand as they were spoken" (Brewer 1973: 7). Although the project director advised the interviewers that "all stories should be as nearly word-for-word as is possible" (quoted from Brewer 1973: 7), a certain amount of editing of both the interviewers' longhand version and the published narratives cannot altogether be ruled out. However, since several interviewers were at work in every state (among them also Negroes), the possible bias of a single interviewer can fairly easily be discovered. Most of this valuable material has recently been published/reprinted (Rawick 1971—1972) and now awaits linguistic analysis.

Although the status of Gullah is undisputed, the relationship between Gullah and the Black English in the other parts of the United States is not; in fact this relationship is crucial also with regard to the origin of the features of Black English. Whereas the dialectologists believe with Turner (1949) that Gullah is an anomaly which developed in a culturally unique situation, it is for the creolists an important piece of evidence to prove an African creole substratum in *all* of Black English. However, their arguments remain un-

convincing as long as the creolists fail to explain why certain features which they think characteristic of Black English — such as the habitative or durative *be* — have so far not been recorded in Gullah. They bring forward what serves their purpose and ignore all the other evidence. Thus, durative *be* is found in British English dialects, as is *gwine* 'going to' and related forms; they dominate in the whole south-west of England. Yet creolists trace its origin back to Akan language forms of the word *gwa*, *gwo*, which — according to them — syncretized early with the 'to go' verbs of the Germanic languages (cf. Viereck 1978). So much for this line of research.

From the purely descriptive point of view, Stewart's opinion was supported by Marvin Loffin. He notes: "Efforts to construct a grammar for Nonstandard Negro English suggest that the similarities between it and Standard English are superficial. There is every reason, at this state of research, to believe that a fuller description of Nonstandard Negro English will show a grammatical system which must be treated as a foreign language" (1967: 1312).

However, the evidence he brings forward is by no means convincing. Thus Loffin, e.g., overlooks the fact that the past tense marker of the verbs, the suffix *-ed*, is deleted in Black English for purely phonological reasons. Wolfram demonstrated this in his investigation of Detroit Negro speech, referred to earlier. This analysis no doubt has its merits, the principal one being to have shown that there exists a stratification of English also within the Black community. However, it sheds no light whatsoever on the problem of the relationship between black and white English. Only by using multiple correlations between black and white speech patterns within social status categories would it have been possible to prove with validity the existence of Black English. (The extent to which Wolfram's data give evidence of the existence of Black English specifically because his informants are black is rather doubtful.) Fragments of an adequate procedure are to be found in Wolfram's comparison of the speech behaviour of the upper-middle-class black and white informants. There he had a reference group, although it only served the purpose of representing Standard English. Further shortcomings of Wolfram's procedure are that his sample is not representative of the black community of Detroit at all and that his results, conclusions, and statements are therefore only valid when referring to each individual informant of his sample. One final point deserves mention: Wolfram's analysis of final *-Z* is noteworthy in several respects. With the possessive *s* — which we will take as an example — Wolfram discovers a so-called sharp stratification, that is, there is a distinct difference in usage between the social classes. This is a useful descriptive statement. Deep-structurally, however, Wolfram cannot do with *one* deep structure for his *few working-class black informants!* He says: "... there are several individuals (among the working-class informants) for

whom *-Z* possessive is much more frequently absent than present. For these informants, it is difficult to postulate an underlying ... *-Z* as a part of their basic N[onstandard] N[egro] E[nglish] system" (1969: 142). Apart from the fact that this so-called Black English feature — the absence of the possessive *s* — can also be found in Southern white speech (and in British English dialects), Wolfram encounters the same deep structure difficulties in his treatment of the simplification of consonant clusters (cf. 1969: 82) and the post-vocalic /r/ where he says: "It may well be that *some* working-class informants have no underlying R [word-finally and before a consonant]" (1969: 115; our italics). We can watch with close attention how many deep structures will be presented to us in the future! Recently, Luelsdorff (1975a, b, c) has added an 'interesting' tint in this respect. In his various contributions Luelsdorff postulates different deep structures for the dialects of Northern, Midland and Southern American English. He does this in using or rather misusing Black English where — in his opinion — there is no contrast between /ɛ/ and /ɪ/ before a nasal, so that e.g. *pen* and *pin* are pronounced the same, namely *pin*. Some reading would have indicated to Luelsdorff that his assertions are incorrect: the pronunciation feature mentioned is by no means characteristic of Black English!

It is, of course, possible to postulate a Black English as a hypothesis but then the results of the investigations must allow a verification or falsification of this hypothesis. As indicated above, Wolfram could do this only in a very insufficient way; it is altogether impossible for Luelsdorff to follow such a procedure, since he works with a *single* informant only: a 14-year old male adolescent! Methodological mistakes are thus perpetuated. One cannot possibly prove the existence of a Black English by comparing the speech of lower-class blacks with Standard English but only by comparing the linguistic behaviour of blacks and whites of the same social and regional background in comparable speech situations, to say nothing of the problem of the origin of the linguistic forms. This, surely, is a long-term project and nothing for those who — instead of trying to collect all relevant data first — prefer to jump to far-reaching conclusions with little or not even a shred of evidence. Research on Black English has up to now been rather data-poor and theory-rich. The sooner this state is changed, the better it will be for a sensible discussion of the many problems involved.

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