

NEW DIRECTIONS IN AMERICAN DIALECTOLOGY

RAVEN I. McDAVID, JR.

University of Chicago

To some extent, appraising the developments in American dialectology is like determining the geology of a new planet while the primordial lava flows and earthquakes are still going on. But we have the consolation of realizing that an appraisal of the dialectology of any community at any time — if it had engaged in the study of its own speech variations — would face the fact of continuing change. We may take for granted that the relationships among the varieties of any spoken language are in a continual state of flux; what is possibly new is the ease with which new coinages in one variety of the language can be picked up by another. Even the modishness of slum idioms in the most aseptic suburbs has its parallels so far back as the time when the heir of the House of Lancaster consorted with an unsavory crew of roisterers and cutpurses, or when Roman Emperors got their kicks from performing in gladiatorial combats — though there is no record that any ever lost a decision.

At one time it was easy to summarize the research in American dialects for a given period in a few pages, and even give some time to some of the data presented. Here, however, we will be more restricted, by the volume of work that has been done both under the traditional directions of research and under the new heavily funded grants from the Office of Education with instant applicability in mind. To clear the air about any possible benefits accruing to me and mine under the latter dispensation, I will admit that I was one of the principal investigators for a project involved in the study of Chicago speech. But I received no financial benefits for myself — not even a reduction of course load, for I taught my full schedule for the duration of the project. And the total amount is about half of what William Labov is now asking for a two-year definitive study of general sound change, or four percent of Roger Shuy's grant for the study of language variation in the Detroit area.

However one views it, there is much more interest in dialect study in America today than at any past time as witnessed by the recent appearance of two anthologies. A great deal of the interest is due to new energy among the

leadership of the American Dialect Society. Beginning in the 1880s with the hope of preparing an *American Dialect Dictionary* on a scale comparable to the *English Dialect Dictionary* of Joseph Wright, the Society dawdled along in gentlemanly inertia and nearly succumbed to the great depression of the 1930s, publishing only six volumes of *Dialect Notes* in the process. Reorganized in the years of World War II, it at first drew in the dilettanti, but under a series of professionally-minded secretaries it has enlarged its membership, raised the scholarly standards of its publications, and its new series of *Publications of the American Dialect Society* is not only nearly caught up but committed for nearly three years ahead. Two years ago it added a *Newsletter*, which has provided, *inter alia*, very valuable bibliographical information, especially in its lists of completed dissertations. And flexing its muscles, the Society is negotiating with the Columbia University Press to assume editorial responsibility for *American Speech*, which has been almost cryogenically anesthetized by its current editor, who despite frequent assertions that he was "just about" to catch up, has allowed the journal to fall three and a half years behind schedule. In fact, a new editor has been recommended by a search committee; but protocol forbids me to mention his name.

I

The largest-scale project in the study of American dialects has been the *Linguistic Atlas of the United States and Canada*, conceived in 1929 under the sponsorship of the American Council of Learned Societies and the directorship of Hans Kurath, after several distinguished linguists — notably C. C. Fries and Edward Sapir — had indicated that the time was ripe for a survey of Western Hemisphere English comparable to that of French under Jules Gilliéron and that of Italian under Karl Jaberg and Jakob Jud. New England was chosen for the pilot project for a survey whose completion was envisaged within a decade. However, the launching of the project coincided with the onset of the Depression. Though the field work in New England was completed in two years (1931 - 33) and the editing and publication by 1943, funds had dried up meanwhile so that only one investigator, the late Guy S. Lowman, Jr., could be kept in the field. On Lowman's death in 1941, and the involvement of the United States in World War II later that year, Kurath saw that managing a nation-wide survey was beyond his resources, and he accepted the notion of a set of autonomous regional projects west of the Appalachians, retaining jurisdiction only of the New England project, nearing completion, and the proposed regional atlases of the Middle and South Atlantic States, for which Lowman had completed some two-thirds of the projected interviews.

The war also interfered with the distribution of the *New England Atlas*, restricted to a mere two hundred copies. The continental universities, where

dialectology had developed, were cut off from communication with North America, and wartime austerity and the effects of the depression restricted sales in the British Commonwealth and the United States. The result has been that a generation of scholars has grown up with little first-hand exposure to the actual findings in New England, and their students in turn know of this work largely by hearsay, and frequently misunderstand and misrepresent its attention and accomplishment. Some of this ignorance, fortunately, may be dissipated in the next few years with the reprinting of the *New England Atlas* and a new edition of its accompanying *Handbook to the Linguistic Geography of New England* — the last with inventory of the contents of its maps and a word index of all forms recorded.

II

With postwar grants from the Linguistics fund of the American Council of Learned Societies, Kurath was able to manage the completion of the field work along the Atlantic Seaboard, but was never able to secure funds for editing and publication. Some of this difficulty, of course, stemmed from the fact that he moved to the University of Michigan in 1946 as editor-in-chief of the *Middle English Dictionary*, and thenceforth had to regard the *Linguistic Atlas* project as a spare-time activity. Finally, with his retirement in 1962, he began negotiations for transferring the Atlantic Seaboard archives to an institution where editors and support might be available; the end of these negotiations saw them moved to the University of Chicago and the Illinois Institute of Technology, and editing was gradually resumed. It is now hoped that by Christmas the first fascicles will be in the hands of the publishers. About a quarter of the items have received at last preliminary editing for the area from southern Ontario through North Carolina, and another quarter from southern New Jersey southwards.

Before turning over responsibility for the editing, Kurath had made a decision about the format of the *Atlas of the Middle and South Atlantic States* — one regional Atlas for the area from southern Ontario through northeastern Florida. Because of the size of the territory and the number of interviews included — nearly 1200 — presentation of the data in cartographic form would be exceedingly difficult. There are also two other practical considerations: draftsmen's wages, for preparing the phonetic overlay for base maps, have risen far more than academic salaries; and the size and weight of the New England volumes, with only four hundred in a smaller, has made for difficulties in shelving in libraries and for consultation by students. As now envisaged, material will be presented in tabular form — like that of Orton's *Survey of English Dialects* — but on a somewhat larger page, roughly that of the *Oxford Dictionary*; each

volume — and at this stage I have no idea how many volumes there will be — will have a map indicating the principal topographic features and communities, and the places investigated. At this moment we have not yet decided whether to publish by letter-press or photo-offset; the former is neater, but considerably more expensive, and involves another stage of proof-reading. We are certain, however, that there will be only one keyboarding.

III

In the meantime, many derivative and supplementary studies have made some of the findings for the Atlantic Seaboard accessible to an audience wider than that which has a copy of the *New England Atlas* in the reference room of their library, let alone those who could travel to where the unedited archives are shelved. Even before the Atlantic Seaboard field work was completed, Kurath himself made available the findings for a selection of the vocabulary items in his *Word Geography of the Eastern United States* (1949). E. Bagby Atwood summarized the largest group of grammatical items in his *Survey of Verb Forms in the Eastern United States* (1952), and Kurath and R. McDavid provided a third summary in their *Pronunciation of English in the Atlantic States* (1961). Two articles by McDavid, "The Folk Vocabulary of New York State" (1951) and "The Position of Charleston Dialect" (1955) present vocabulary evidence that was not available when the *Word Geography* went to press. And three of Kurath's students — Walter S. Avis, Thomas Wetmore, and W. R. Van Riper — have discussed in detail in their dissertations the Atlantic Seaboard evidence on the mid-front and mid-back vowels, the low-central and low-back vowels, and postvocalic /r/. The first was published in summary in *Language*, and the second in a slightly reduced form in *PADS*.

IV

The progress of the autonomous regional surveys has been uneven, depending on the presence of an energetic prospective director, the availability of competent field workers, and financial support — the last usually meaning a long-term commitment by a major university. But they have seen two innovations in the process of data-gathering: (1) the revival of the correspondence questionnaire (2) the introduction of the tape recorder.

Correspondence questionnaires are old in dialect study. They were used in the gathering data for the *Deutsche Sprachatlas* of Wenker and Wrede, for the complementary *Wortatlas*, and for the surveys in the Netherlands and Norway. But they came under severe criticism from such scholars as Gilliéron and Jaberg on the ground that there were too many personal variations in phonetic trans-

criptions. In 1947 A. H. Marekwardt, in process of launching full-scale work for his *Atlas of the North-Central States*, suggested to a seminar at the University of Michigan that the difficulty was not in the correspondence technique but in the use to which it had hitherto been put; that a multiple-choice vocabulary questionnaire, restricted to items for which there could be no social taboo, would at least serve as a preliminary survey instrument, and could provide useful supplementary evidence. These hopes were realized in the dissertation of A. L. Davis, *A Word Atlas of the Great Lakes Region* (Michigan, 1948). Marekwardt himself did not incorporate the checklist in his own survey, but investigators in Ohio, Indiana and Illinois have utilized it for special studies, or just as a means of familiarizing students with the methods of linguistic geography. And in other regions it has been utilized from the beginning.

The tape recorder, a by-product of electronic research in World War II, has been used in many investigations, including some of the last interviews for Marekwardt. It has a number of advantages: it provides a permanent record; it makes it possible to divide the task of data-gathering between a skilled interviewer and a transcriber; and above all, it permits the gathering of conversational data, especially grammatical forms — about which even the least literate informant is apt to be on guard — on a scale that cannot be matched by even the best interviewers. On the other hand, it has liabilities. It is another object the field worker has to watch; it may give out at critical moments in the interview; its greatest advantage, the garnering of unguarded responses, requires very close listening, for as much as four times the length of the actual interview. Most important, its presence sometimes tricks the interviewer into believing that he can do his job in a shorter time than he could with pencil and notebook, so that there is a tendency to hurry through the interview and force or skip items that might be of considerable interest to others. It is worth noting that the earlier records in Indiana using the tape recorder provided far less evidence on grammar than did those in Michigan and Ohio and Illinois where older techniques were employed. This has led to the observation that where a survey depends on gathering the evidence by tape recording, the interviewers must be trained in the operation and maintenance of the machine, and given particular instruction in conducting the interviews and getting all questions answered — in other words, conducting the interviews as if the machine were not present. And the transcriber, in turn, except for bagging grammatical items, is advised to try to make his notations as if it were a live interview, and avoid tormenting himself by repeated playings-back in effort to put down even finer phonetic shadings.

The Atlas of the North-Central States was begun by Marekwardt in an effort to prove whether there was indeed a highly uniform "General American" in the Northwest Territory. On discovering from the preliminary investigations that the Great Lakes Basin and the Ohio Valley differ strikingly in vocabulary

and pronunciation and even in some details of grammar, he proposed a survey with a network somewhat coarser than that for the Atlantic Seaboard. Of the states in the survey, only Wisconsin was completed before American entrance into World War II, and it was after 1960 before the last communities were covered. Since most of the work was locally financed — principally by the state universities (the University of Michigan being by far the largest contributor) — there was a goodly number of fieldworkers, and a number of discrepancies in their practices, though as in New England one interviewer did the greatest part of the work, and since he was the most experienced it is possible to allow for the difference in analyzing the records.

The Atlas of the Upper Midwest was begun in 1947, under the direction of Harold B. Allen of Minnesota, one of Marckwardt's original field workers and a former colleague at Michigan. Allen himself did the lion's share of the work, and three of his investigators had received training from Kurath and Bloch of the original *Atlas* staff. The chief complication arose in Iowa, where inexperienced students took on interviewing, and divided the state along an east-west line, so that there is sometimes a serious problem of separating regional from personal boundaries. Minnesota has been so generous in its support of Allen's enterprise that editing has now been completed and publication is under way — the first regional survey since New England to be accessible to scholars everywhere. With checklist data accompanying field records, editing has involved the tricky problem of presenting two different kinds of evidence; the checklist findings will probably be presented in graphs with percentages rather than in individual plottings on maps.

The Atlas of the Rocky Mountain States was started ambitiously in 1950, by Marjorie Kimmerle of the University of Colorado. With some assistance from her university and other institutions in the state, she was able to complete the field work for Colorado in little more than a year. But elsewhere there were scant funds for field workers, and of those none has even finished a state. Two dissertations have been derived from the Colorado material, one by Clyde Hankey and the other by Elizabeth Jackson, and the first has appeared in *PADS*. But otherwise the only survey to be completed was one of eastern Montana, by Thomas O'Hare, a student of Atwood's at the University of Texas.

The surveys of the Pacific Coast have undergone various vicissitudes. During his tenure at the University of Washington, Carroll Reed — one of Kurath's students, and co-editor of *Linguistic Atlas of Pennsylvania German* — managed to obtain support for field work in Washington, Idaho and part of Western Montana, but never secured it for Oregon; and since his departure nothing has been done. Even more lamentable is the fate of the Southern Pacific survey, directed by David Reed of California — Berkeley, one of Marckwardt's students. With probably the most generous institutional support

any survey has secured, he was able to complete the field investigations for California and Nevada in a very short time — and on a heroic scale, with three hundred field records and a thousand checklists. But again, editing has lagged — because of Reed's involvement in administrative matters and the internal wranglings that have given the Berkeley campus so much undesirable publicity for the last decade. The vocabulary evidence has been surveyed by Elizabeth Bright, in a dissertation that has not yet been reworked for conventional publication, but the rest awaits what Reed will be able to do in his new environment at Northeastern, where he has brought his materials.

In the interior of the United States — the field work for Oklahoma was completed by Van Riper in 1957-63, that in Missouri by Gerald Udell in 1965-68; neither has approached publication. This past year, one of Allen's students, Gary Underwood, has begun organizing a survey of Arkansas as a part of his duties at the state university; no reports have yet been forthcoming. In Kansas, several scholars have attempted to launch a survey, but none has got under way; perhaps the arrival of James HARTMAN ASSOCIATE OF Cassidy on the *Dictionary of America Regional English*, will augur better things. In another region, the late C. M. Wise of Louisiana State provided a dozen field records toward a linguistic atlas of Hawaii; but they have not been edited, and recent interest in Hawaii has been in the complex area of pidgins and creoles.

One of the most interesting areas in the United States, and largely unsurveyed, is the Gulf States region, sometimes known as the Interior South. The difficulty in coordinating and funding work in the region is reminiscent of some difficulties the late Confederacy had in waging war in the same territory. Funds have been lacking; institutional and personal jealousies have prevented a coordination of effort, and until recently there has been no one strong and energetic enough to assume the burden of direction. Texas, with probably the most extensive linguistic operations of any American university, was unable to provide adequate support for Atwood, one of the most distinguished students of American English. He conducted a survey of the lexicon on a shoestring, using student investigators; his *Regional Vocabulary of Texas* was published shortly before his death in 1963, and no one has undertaken the similar survey of pronunciation which he planned to complement. In Louisiana, Wise's students did more than a hundred field interviews with a long questionnaire, but he never considered them a substitute for a field investigation by trained interviewers, and neither he nor his successors have been able to arrange it. For the rest of the area — from central Georgia to the Mississippi — there is as yet only checklist survey of Gordon Wood, recently published under the title *Vocabulary Change*.

Recently, however, there have been some hopeful developments thanks to the arrival at Emory University of Lee Pederson. In a series of conferences, from 1968 on, he has gradually planned his network, developed a questionnaire

for the region (including most of the South Atlantic items), and has begun to gather evidence. At present he has one investigator in Central Alabama and another in southern Mississippi, and is himself in the field in eastern Tennessee, the pre-Revolutionary settlement area yet unsurveyed.

In Canada, the greatest activity has been in the Maritimes, where Lowman made several interviews in New Brunswick. The early investigations by Henry Alexander ran afoul of military suspicion in World War II. Two local surveys — by H. Rex Wilson in the German settlement of Lunenburg County and by Murray Wanamaker in Annapolis Valley — have been supplemented by subsequent field work to the point where an atlas of Southern Nova Scotia is in the editing stage. Murray Kinloch of the University of New Brunswick has investigations in that province, and a conference toward finishing the survey of the Maritimes has been scheduled for next summer. In Newfoundland the interest has been chiefly in dialect lexicography, but there is also the beginnings of interest in the phonology and grammar. Last year the Canadian Council of Teachers of English proposed a national language survey, largely by means of checklists; though no substitute for field interviewing it should serve — as Wood's checklist study in the Gulf States has served — to provide extensive coverage of a small number of interesting items, and should make it easier for provincial organizations to organize the kind of work that is now going on in the Maritimes. Except for Atwood's book, the only broad gauge survey for any region, is my wife's dissertation on verb forms in the North-Central States and Upper Midwest.

V

All of the projects covered so far have been in the tradition of Gilliéron and Jaberg and Jud, expanded and modified by the use of correspondence materials for the vocabulary. All of them explicitly or otherwise recognize the traditional affiliations of linguistic geography with historical linguistics, as a means of tracing the affiliation of various regional types of speech with each other and of helping to reconstruct the earlier stages of the language. For this reason there is a skewing in the direction of older and more traditional usage — through a somewhat heavier representation of rural and small-town society, a heavier selection of the oldest and least educated segment of population, and a search for the old fashioned elements of the vocabulary than current and population usage might seem to justify on a purely mathematical basis. But the peculiar linguistic situation in the United States, with no single regional mode of speech having cultural preeminence, made it advisable to include representative cultivated speakers in a large number of communities; and in almost every community a member of the intermediate group. With these basic types of informants, it has been possible to make some judgments about social differ-

ences at the time the data was collected, and about the indicated direction of change at that time. But no one involved in the Atlas project has ever asserted that it could be definitive statement of social differences in any community in a nation which has been characteristically one of movement and change; it is only suggested that the evidence provides a set of benchmarks as of a given point in time, against which the findings of subsequent studies might be measured.

The number of community studies undertaken within Atlas frame of reference — that is, making use of evidence of regional surveys — is already considerable. In New England Robert Parslow has studied the pronunciation of Bostonians, and the students of Audrey Duckert have reinvestigated a number of smaller communities included in the *New England Atlas*, occasionally (as Miss Duckert herself did in Plymouth) finding one of the original *Atlas* informants. For New York, Yakira Frank and Allan Hubbel completed dissertations in 1948, Hubbel adding to a selection of *Atlas* interviews a number of his own. Marckwardt has a student Lebovsky working on the speech of Philadelphia. In North Carolina, the sociolinguists Crockett and Levine have studied the effects of a generation of increased affluence and education on the speech of the small community of Hillsboro. The independent oligarchy of Charleston has been studied by Raymond O'Cain of South Carolina, who in turn has a student about to begin the study of the rival principality of Savannah. In the Middle West there is a large number of studies — Lee Paderson in Chicago, Gerald Udell in Akron, Marvin Carmony in Terre Haute, Indiana, and Robert Howren in Louisville. And from the Pacific coast we have the studies of Fred Brengelman for the Puget Sound area and David DeCamp in Metropolitan San Francisco. Utilizing the Atlas methodology but lacking a regional frame of reference because no regional survey had been undertaken at the time, are such studies as that of Arthur Norman in the southeast corner of Texas and Juanita Williamson among the Negroes of Memphis, Tennessee.

As yet there are no phonographic archives of American English comparable with those which Zwirner has established for Germany. The late C. K. Thomas accumulated several thousand tapes of a reading passage, using as informants largely the students in teachers' colleges, and out of his archives developed his successful book on the phonetics of American English; but since his death the collection has not been accessible. More recently, as part of a project in preparing English teachers in the state of Illinois, the linguists at Illinois Tech developed a questionnaire for pronunciation alone, involving short-answer questions, minimal pairs, a reading passage and free conversation; and with it they obtained some thirty specimens of cultivated speech in the United States and Canada — a variety great enough to shake the ethnocentricity of most teachers. Under the title of *Recordings of Standard English* (I proposed

Archives of Recorded Standard English, but the inevitable and unforgettable acronym made it unacceptable to the director) it has expanded its aims to a sampling of as many varieties of English as possible, wherever spoken, with a view toward exchanging tapes with scholars in other communities and nations.

VI

Dialect lexicography, as we have noted, antedates dialect geography in English; and though the original aim of the Dialect Society, to make an American dialect dictionary, was not achieved by that organization, its members have never forgotten the intent, and one of them — Frederic G. Cassidy of Wisconsin — is on the verge of achieving it. After nearly two decades of preparation, including the devising of a new questionnaire and testing it in the state of Wisconsin by correspondence, a generous grant from the National Endowment for the Humanities enabled him to launch the *Dictionary of American Regional English* in 1965. By now his operatives have completed their field investigations — including the same three cultural types as were interviewed in the regional atlases — in all fifty states, and have read extensively in regional novels, diaries, and local newspapers. With the aid of various occult devices, including computer storage, he can be said to be in the pre-editing stage, even as other evidence comes in. His ambition is to offer the dictionary at a contribution to the national bicentennial of 1976, even as Avis and his colleagues in Canada made the Dictionary of Canadianisms offering on the centennial of Confederation. Even in its unedited state, the project is a fruitful source of material for regional and local studies, and its questionnaire was used in one independent investigation, Saunders Walker's dissertation on the folk speech of the eastern Alabama Negro (Western Reserve 1956). Outside of *DARE*, the most noteworthy venture into dialect lexicography so far has been George Story's *Newfoundland Dictionary*, pursued for many years as a labor of love and now slowly approaching completion. Whatever comes to light about the speech of this oldest and most isolated English-speaking community in the New World is sure to be of interest on both sides of the Atlantic.

VII

„Dialect writing” has long been used as source material for regional and local speech, and is still being used. But as the field evidence accumulates, the process gets turned around, and scholars are able to see how authentic is the representation of dialect in a given author. Pederson and Norman have

made forays into Hannibal, Missouri, to ascertain the baseline from which Mark Twain worked in *Huckleberry Finn*; apparently there is nowhere near as fine a discrimination in the novel as Twain indicated he had achieved. Sumner Ives has approached the dialect of the Uncle Remus stories, aided by the fact that Harris's son, Julian, was one of the Atlanta informants for the Atlas. More recently, Charles William Forster of Alabama has examined the works of Charles Boykin Chesnut, utilizing the field records from Fayetteville and Greensboro, North Carolina. And James W. Downer did a magnificent study of the literary dialect in the *Biglow Papers* — a study complicated by the fact that Lowell's Cambridge has been swallowed up in metropolitan Boston, so that the speech of other communities, chiefly in southern New Hampshire, had to be taken to establish the base line. But not all students of literary dialect are so careful in their comparisons, as witnessed by a nameless study of dialect in the stories by Stephen Crane: not only was there no visit to the archives of Atlantic Seaboard material, but the study — involving New York City and the Hudson Valley — omitted the the *Word Geography*, the *Verb Forms*, the *Pronunciation*, and the various pertinent works of Thomas, Frank, Hubbell, Labov and Arthur Bronstein, a monumental achievement in omissions.

VIII

Of bilingual investigations I shall say little, partly because it is not my field, partly because it alone would be the enough for this audience. The best picture can be obtained from the work of Einar Haugen, beginning with *The Norwegian Language in America* (1953) and continuing through the various versions of his *Bilingualism in the Americas* (first published in 1956). The late Uriel Weinreich has also contributed to the study both through his *Languages in Contact* (1952) and his investigations of Yiddish dialects, largely carried on in New York City because of the lack of access to informants in their European habitat. There is a spate of competent investigations of various languages translated to the American scene, my candidate for the most noteworthy being Mrs. Janet Sawyer's study of the speech of the bilingual community of San Antonio, Texas. In recent years Joshua Fishman of Yeshiva University has become a distinguished figure in this field; I have imperfect sympathy with him, perhaps because I once read the uncut manuscript of his *Language Loyalties in the United States* for a publisher who subsequently decided not to print it. As I read Fishman's works, I find that he is full of statistics about the kinds of occasions on which a given language is used, such as the number of hours of radio programming, but has little to say about what has been happening to it as it is used.

IX

So far we have dealt with works in the scholarly tradition, conceived out of one's professional curiosity, concerned with discovering the facts, and leaving the application and doctrinal interpretation for others. But the profit in poverty consequent to the Johnsonian image of a Great Society, there has come into existence a new kind of dialect study, in which the findings are established in advance and the investigator shapes his evidence to fit them. Since there is no fun jousting with weaklings, I shall confine my discussion of this type of investigation principally to the work of the two best-known students in the field, Roger Shuy and William Labov. Both of them are personal friends, and Shuy is a former student of mine; I am saying nothing here that I would not say to their faces — in fact, I have said it in the past, and expect to do so again.

Labov, a student of Weinreich, produced a very fat book entitled *The Social Stratification of English in New York City*, based on a study on the Lower East Side of Manhattan. Subsequently he became a consultant for the Board of Education of New York City, assisted on their document *Non-standard Dialect* (which among other things failed to differentiate between the Puerto Rican and the Negro subvarieties of New York City speech), which was published by the National Council of Teachers of English, and later prepared for the NCTE his own monograph on the study of non-standard English. As a roving consultant to various institutions, he has been able to produce authoritative statements on the linguistic changes taking place in communities as disparate as Beaufort, South Carolina, and Phoenix, Arizona. His most recent publications include a five-volume report on the English of Negroes and Puerto Ricans in New York City, and a theory of tense.

Shuy's dissertation (Western Reserve 1962) was a delineation of the Northern-Midland boundary in Illinois. In 1964 he joined the faculty of Michigan State; the next year he inaugurated a study of the speech of Detroit, from which most of his subsequent publications are derived, though the bulk of the study has been summarized only in the official report to the U. S. Office of Education. In 1967 he joined the Center for Applied Linguistics in Washington, to attempt to bring some direction to its own program with the Washington schools, which in two years had managed to spend two-thirds of generous five-year funding without producing anything. A year ago he joined the faculty of Georgetown University as director of its graduate program in sociolinguistics. Like Labov, he ranges wide. Several books of his and his colleagues have been published by the Center: *Conversations in a Negro American Dialect* (Beng and Loman); *Field Techniques in an Urban Language Study* (with Walter Wolfram and William K. Riley 1968); *A Sociolinguistic Study of Detroit Negro Speech* (Wolfram 1969); *Teaching Black Children*

to Read (an anthology, with Joan Baratz as joint editor, 1969); *Teaching English in the Inner City* (an anthology, with Ralph Fasold as joint editor, 1970).

Criticism of the work of this school — and what applied to Shuy and Labov is equally true of lesser lights who will get only incidental mention — may be directed at several points, including their primary purpose, the selection and identification of informants, and above all to the use they make of previous investigation and other relevant data. On this last score the initial study of Labov can be exempted, since he specifically acknowledges examination of the *Linguistic Atlas* data for New York — whether in the original field records or in the summaries in the *Pronunciation* volume I am unable to say. But I know that of all the various new pundits in this field, not one has asked to consult the *Linguistic Atlas* materials for the Atlantic Seaboard during the seven years I have them in my custody. Even Shuy did not make use of the materials in the North-Central files, of which I have a set; this is a special disappointment because he knew I had those files, and indeed had made use of them in preparing his dissertation.

Since these studies are designed to yield instant solutions for "practical problems", they are usually well financed. A hundred thousand dollars is a puny amount. It is usual for the investigators to have a situation resembling that of a business executive or a government bureaucrat, with ample equipment and secretarial help. As one who has had no one to share even the humblest chores of my editorial operations, I am frankly envious; nor am I likely to be placated by such project is better spent than what is allocated to the procurement of ordnance and ammunition. The armed services — like the Jesuits — have earned to look out for themselves — money wasted on administrative frills in a sociolinguistic project is not taken away from bloodthirsty militarists but from the painstaking researcher who might use it to uncover the data, new or old, that would put the project in its proper perspective.

Most of these projects depend on sociological techniques, attempt random samples and bristle with chisquares and standard average deviations. In reality, both Labov and Shuy worked with populations that prevented the interviewing of genuine upper-class speakers of long-time cultural traditions. The old-line New York families are not found in the Lower East Side; and however desirable it was for Labov to reach the recent immigrant stocks and the newly arrived in New York City — groups excluded by the Atlas practice of interviewing only natives of the community with as many generations of native-born ancestors as possible — yet one can pint out with equal fairness that his population does not include a single person who would have qualified as a cultivated informant by Atlas standards. In Detroit, Shuy's restriction of the survey to the city and its two enclaves of Highland Park and Hamtramck, and of his informants to families with children in public

or Roman Catholic parochial schools, again excluded the upper group, whose influence as speech models should not be neglected; such people either live in suburbs, or send their children to non-Roman private schools, or both.

In fact, in many of these later studies there is a strange compression of the American class system. Lloyd Warner and his disciples worked with six or seven classes, and the possibility of more; with the caste dimension — racial or otherwise — there are more subdivisions. But Shuy, and Labov to some extent, worked with a binary system: two races, middle and working classes, upper and lower divisions of each. What is more, the judgment was made almost exclusively by mechanical means — income, type of job, house size, and the like — rather than the more valid, if more subjective criterion of the kind of people with whom one exchanges visits.

I am also old-fashioned enough to be worried about the facelessness of informants in these surveys of the new breed. Perhaps the kind of interviewing I did, and the kinds of places where I did it, prejudices me but I cannot help thinking of each informant as a real person, with whom I have had an interesting if often exhausting dialog, whose responses should be differentiable from other people's.

Many of the newer studies seem politically designed, with more of an eye on particular buncombe than on the traditions of the language. Because of some of the less pretty aspects of American history, Americans of African descent have not participated fully in American society; the desire for some sort of cultural autonomy on their part — and similar desires for the Puerto Ricans, Border Spanish, and American Indians — is commendable, and so is the desire to recognize their varieties of English as constituting functioning linguistic systems. But they are not the only group suffering from economic and educational disadvantages and penalized for the way they use the language, nor even the largest group. Perhaps two thirds of these disadvantaged are white Protestant gentile Americans of colonial stock. Yet I can think of only one investigation of the speech of this latter group in an urban setting.

Most of these investigations end up — or even start with a dichotomizing between Standard English and Black English (a lady linguist at Mr. Sledd's former billet has come up with the ingenious if syntactically distressing term *Child Black English*). *Standard English*, in turn, is identified with the outworn term "General American", or its more recent synonym "network English." As one who has listened to radio and television many hours, I have decided that there are perhaps two speakers of "network English" extant — Richard Nixon and Billy Graham — and these do not use it on all occasions. What seems to be meant is some form of Great Lakes Basin speech. I am sure that neither Mr. Sledd's classical Atlanta accent, nor mine with the strong overtones of Possum Kingdom, South Carolina, would recommend itself to the inventors of this term. But I am heretical enough to believe

that spoken Standard English is a very flexible term, capable of covering many phonetic packages. I am equally disturbed over the term Black English, as if it were a monolith; the speech of the Negro American comes in about as many varieties as that of the Caucasoid American. I am even more distressed by the notion — perhaps confined to our brethren in departments of speech — that a dialect is a form of speech pathology; the Lincolnland Conferences on Dialectology, promoted by Eastern Illinois University, are creatures of the speech clinic of great institution, and the one I attended was heavily committed to the notion that something called "dialectolalia" might be amenable to the tools of correctionists — and with the black constituency uppermost in their thoughts and words. In fact, one of the panel, a young gentleman domiciled at Ann Arbor but ultimately of New York origin, remarked — in a Bronx accent thick enough to need a chain saw to cut it — that he'd always thought of dialect in terms of poor black kids in urban slums.

Perhaps the most distressful part of the current emphasis on what is usually called Black English these days (it has other names, but the meaning is the same) is the careless way in which its putative origins and stigmata are bandied about. I am not going into the former in detail; I do not have the armamentarium to solve the problem, and I am far from asserting that no Africanisms survived in the speech of Afro-Americans, or indeed in the speech of Southern Americans, of whatever race. But I would feel that the dialectal origins of all varieties of American English are so complex, since every colonial community was characterized by dialect mixture, that it behooves us to examine in detail the evidence from the British Isles for sources before we leap to the more romantic notion of a general Afro-American pidgin spoken throughout the South (even in those communities where slavery came late and piecemeal). Lorenzo Turner, an Afro-American himself, who has studied Gullah, Haitian, Brazilian Negro-Portuguese and several African tongues, believes that the case for African survivals in Gullah can simply not be extrapolated to the rest of the United States, because the cultural situation in which Gullah developed had no parallel elsewhere.

Nor is one easily satisfied with the stigmata attributed to Black English, especially if one has examined the evidence from other varieties. Most of the brethren who push that case have never looked at even such relatively accessible studies as Atwood's *Verb Forms* or my wife's Minnesota dissertation. When recently challenged to cite evidence for *gwine* in white speech, I pointed out that it was common in the South and also found in northeastern New England — as Atwood had indicated (I have since discovered it is also used in Oxfordshire); and analogous forms are widespread in Southern England; but my adversary refused to believe me, or to examine the obvious references. At the same session Shuy and Fasold presented a tape from the Detroit study,

and adjudged the informant black on the basis of this monophthongal diphthong of /ai/; I pointed out that this was endemic in Southern speech of all races. Next came the pronunciation of the expletive *there* as hymonymous with *they*; I pointed out that the Dean under whom I had served at Western Reserve, a Harvard Ph. D., had this feature in his speech. Finally it was argued, on the strength of Labov's observation, that no white person would omit the relative pronoun as subject of a clause, or negate both principal and subordinate clauses in a complex sentence; I closed out the session by quoting a sentence from a poor white Georgian I had interviewed in 1947: "They ain't nobody never makes no pound cake no more." As convincing as my demurrals were to the audience, they apparently have not registered with Shuy; his last paper — distributed long after the meeting in question continued the same old argument. Nor does Labov operate much differently. In evaluating the grammatical forms used by his Harlem informants, he did not come to me for my recorded evidence, or mingle with educated Southerners on terms of equality and listen to the way they talked, but sought the opinion of a clerical worker in Washington — perhaps the worst person, since she had probably put the South behind her and was of the insecure status that Southerners attribute to "strivers and strainers." And in fact, I am daily often confronted in my editorial work with the racial misidentification of linguistic forms by informants; such folk preteritives as *chim*, common in the New England settlement area, are attributed to Negroes only by many Virginians, though the form is often used by Virginia whites, so I am hesitant to rely on even my own intuition about the racial identification of grammatical features; almost every important form which Labov's treatise identifies with Negroes is, I feel, at least potentially present in my speech, though I may not be able to recall how or when I have used it (but it was a year after Fries had proclaimed the impossibility of three-object verb when I accidentally found myself uttering none).

In *Teaching Standard English in the Inner City* Fasold and Wolfram set forth some forty characteristic features of "Black English." At about the time I read their article, I reread Harold Paddock's MA thesis on the speech of Carbonear, Newfoundland. It was an enlightening experience. Much is made over the Black English aspectual difference in the use of the copula; zero for present, *be* (occasionally *bees*) for a timeless non past. In Carbonear the aspectual distinction extends to all verbs: zero ending, regardless of person, for immediate present, *-s* for timeless non-past, so that Carbonear speaks Black English more systematically than Harlem does. Indeed, most of the stigmata of Black English proved to be used in one or more Newfoundland dialects, far overshadowing the fact that well over half of them are part of my normal usage. This leads to interesting speculations: could the Newfoundland usage derive from an Afro-Portuguese pidgin somehow brought

ashore by workers on the fishing stations? Or since many of these forms occur in Irish English or elsewhere in the British Isles, perhaps we must look further — perhaps to an Afro-Phoenician pidgin brought to Britain by tin smelters from Africa, and transmitted through Cornish to the dialects of Southern English. If one wishes brilliant theories, such intuitions are as good as any others in the saloon.

X

The development of American dialectology in the past decade or so reveals — even in aberrations — its unique qualities as a subdivision of linguistics. It is a data-oriented discipline; however fine the theoretical extrapolations one may wish to make, the dialectologist's primary duty is to present the data in such a way that any reader can replicate the conclusions — or failing to replicate them, can show where the statement went astray. However unfashionable this position may be at any given time, it is one in which he can take comfort. For sooner or later the fashion will change, and the data — like Sir Roger de Coverley's coat — will once more be in style. It is gratifying to know that most Americans who work seriously in dialectology are following its traditional principles, albeit with new technical aids and new environments.