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A Conference to Plan a Linguistic Atlas of the Southeastern States,

May 16-17, 1968

Lee Pederson

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On May 16-17, 1968, the Southeastern Education Laboratory (SEL) spon-
sored a conference to discuss plans for the organization of a linguistic
atlas of the Gulf States. With the support of SEL Program Coordinator, W.
Gene Watson, the meeting was scheduled to consider the composition of a
survey that might lead to the completion of regional coverage in the Eastern
United States. Informal discussions with Watson in 1967 led to a biblio-
graphical search of Southern speech for SEL, and that project was followed
by the organization of A Dialect Survey of Rural Georgia in January, 1968.
Both the Dean of the Graduate School, Charles T. Lester, and the Chairman of
the Department of English, Albert E. Stone, Jr., put Emory University behind
this early research, and Watson recommended further cooperation between the
two institutions. With the encouragement of Eunice Sims of the Atlanta Public
Schools, those alliances had been established after Pederson joined the Emory
faculty in 1966. And it was the generous and enthusiastic support of Sims,
Stone, Lester, and Watson that made possible the Linguistic Atlas of the Gulf
States (LAGS) Project.

Kurath and his associates had finished their fieldwork on the Atlantic
Seaboard, as far south as Jacksonville, with Marckwardt, McDavid, and Allen
having extended their coverage across the interior states from Ohio to the
Dakotas in the north and Kentucky in the south. Under McDavid's direction,
the Linguistic Atlas of the Middle and South Atlantic States Project had moved
as far west in Georgia as the urban centers of Atlanta and Macon. Students of

Leo Gosser at Auburn (Alabama Polytechnical Institute) had conducted lexical surveys in Alabama based on the Atlas work sheets. In Louisiana, students of C.M. Wise at LSU completed more than 100 field records based on the long work sheets of the South Atlantic States. Most of that research was conducted during the decades immediately before and after World War II, 1935-55, but more recently E. Bagby Atwood had completed a lexical investigation of Texas, and Gordon R. Wood was preparing a report on a postal survey of eight Southern states--Tennessee, Georgia, Florida, Alabama, Mississippi, Louisiana, Arkansas, and Oklahoma. William R. Van Riper had completed fieldwork in 1961 of a full-scale atlas investigation of Oklahoma dialects and had recorded on tape the full corpus of his research. James B. McMillan produced a classic statement on East Central Alabama phonology in a 1946 dissertation and was, in 1968, completing an authoritative bibliography of Southern speech studies.

Developments in general linguistics during the decade immediately preceding the conference included the most spectacular set of innovations in its relatively brief history in the United States. Harris, Chomsky, and Jakobson had developed a transformational-generative model of linguistic description that rejected most of the basic assumptions of the earlier structuralists. Shortly thereafter, Weinreich and Labov introduced methods for the study of speech that offered a comprehensive set of procedures that radically modified the methods of conventional linguistic geography. And, most important, from the standpoint of the study of American speech, Cassidy launched the Dictionary of American Regional English (DARE) Project, later called by John Algeo, the most important original lexicographical research of the twentieth century.

Applications and reflexes of all three of those developments were closely

tied to social developments of the 1960s.   The administrations of Kennedy

and Johnson had for the first time in American history made a deliberate

effort to revise the American cultural system, and general education was

a central target of their programs.   This interest and support of the federal

government led to the application of the methods of Chomsky and Labov in a

large number of research projects.   The most ambitious of these, perhaps,

were "Project English," an elaborate program aimed at public education.   At

the same time, a series of urban language studies coordinated by the Center

for Applied Linguistics under the direction of Roger W. Shuy were organized

to gather data and disseminate findings.

With all of that work going on, McDavid and others had argued for the

continuation of basic research in the traditional atlas method as the best

means for gathering a broad data base for the specialized studies that had

captured the interest of students of American speech.   Sustaining the work

on linguistic atlases, however, had become difficult.   Only McDavid and Allen

had been able to continue their respective research in a steady development,

and considerable criticism--much of it quite accurate--had made it difficult

for others to find support to continue the conventional approach of the

American atlas projects.

At the heart of that problem were four important factors, all of which

were inseparable from the social and technological developments that emerged

during the 40 years since Kurath and his associates planned the Linguistic

Atlas of New England.   Specifically, these were 1) the total dominion of urban

over rural American society; 2) a renewed consideration for the rights of

various social groups; 3) the development of lightweight, high quality tape-

recording equipment; and 4) a growing understanding of the uses of computer-

assisted programs in the processing of linguistic data.

All of those considerations were incorporated in a proposed agenda that was sent to 14 American linguists in February, 1968, with invitations to attend the conference in Atlanta. Of that group, ten were able to attend. These included Harold B. Allen, University of Minnesota; Hans Kurath, University of Michigan; Raven I. McDavid, Jr., University of Chicago; James B. McMillan, University of Alabama; Lee Pederson, Emory University; William R. Van Riper, Louisiana State University; A. Hood Roberts, Center for Applied Linguistics; Rudolph Troike, University of Texas; Juanita V. Williamson, LeMoyne College; and Gordon R. Wood, Southern Illinois University.

The conference was organized in four sessions with the first three set aside for discussions of the plan for a survey of the Gulf States. The fourth session brought the participants together with local educators and other interested Atlantans for an informal conference that extended through the afternoon of May 17. The following summary of the proceedings is limited to the first two sessions--those of the morning and the afternoon of May 16. On the morning of May 17, progress reports were made by Allen, McDavid, Troike, Williamson, and Wood on their respective research activities, and these were followed by a discussion of possible sources of funds for the support of a linguistic atlas of the Gulf States. Both of the sessions reported here were fully recorded on tape by Howard G. Dunlap, then a graduate student at Emory University.

THE FIRST SESSION

The meeting convened at 9:15 a.m. in the conference room of the SEL, Hapeville, GA. After the participants had been introduced to the staff with brief remarks by the Laboratory Director, William Hopper, Watson and Earl W.

Brockman, the SEL linguistic consultant, reviewed the general plan of the

meeting. Pederson then offered the proposed agenda for consideration: a

discussion of theoretical, methodological, and practical questions, suggesting

that the morning session be limited to preliminary aims and methods and the

afternoon session given over to the seven topics outlined in the agenda. This

schedule was unanimously approved, and Hans Kurath was asked to begin the dis-

cussion.

Kurath reviewed the principles of selective sampling, beginning with

recommendations for the composition of a preliminary questionnaire and con-

cluding with the essential considerations for the selection of communities and

informants. Although his remarks closely followed the outline of "From Sampling

to Publication" as later published in Studies in Area Linguistics (1972), Kurath

qualified all of his suggestions with these four recommendations:

1) Nothing can be adopted and translated into an operational
   plan until the aims and methods of the survey are identified;

2) the implementation of those aims and methods will be dependent
   upon the available personnel to carry out the work;

3) the plan should be carefully developed, step-by-step, beginning
   with a brief questionnaire administered in a small geographic
   area and enlarged according to the needs and resources of the
   project;

4) close attention should be given to attitudes about usage be-
   cause it is here that the survey might offer a valuable resource
   for educators.

On that final point, Kurath concluded:

    I don't know if there is anything else I should say now. I just
    want to emphasize again this assumption. You cannot hope to teach a
    better dialect--that is, a cultivated type of English--effectively
    unless the teacher knows the local type of speech and, in a scholarly

way, respects it as a system of communication that works within the community. To knock the family dialect, first of all, is an insult to the child and his family. Besides, such an approach does not work. Instead, I think, the emphasis should be on convincing the students that it is to their advantage to learn a somewhat different dialect which they can use on proper occasions. The student himself will learn by experience that if he uses "School English" in playing baseball with his friends, with his fellows, that he is not going to be in good standing with them. So I would say in teaching English, emphasis should be placed upon the desirability of acquiring a standard variety for uses in particular situations. How can the teacher do that? We, the teachers, must begin with reliable information on both types of speech.

Raven McDavid affirmed Kurath's generalizations and emphasized the appropriateness of his remarks in the context of Southern speech. Without adequate data, the educational process is severely debilitated:

What Mayor Daley of Chicago said of other matters will also hold true for language: what Chicago needs is a Point-Four program for Mississippi and Alabama. That is because those are the sources of these geographically transposed dialects that create problems for educators in Northern cities.

Teachers--with very little accurate description of any form of English other than the written varieties, with very little awareness of regional differences, and very little awareness of actual social differences in language within the area from which the migrants come--simply flounder around, either giving up completely or else trying to make a head-on attack upon all features of language in the speech of the Gulf States student. As a result, they create a certain amount of hostility. They try to do too many things--emphasizing matters that are trivial to any well-bred Georgian, at the expense of the grosser realities. For that reason, the language programs in many Northern cities simply will have to have a drastic renovation in light of the information that could be made available through a survey such as the one we are discussing here today.

Harold Allen recommended that even in the earliest stages of the investigation a questionnaire should be flexible and not overly dependent upon the work that had been done before in other regions:

I realize now--and this is relevant to something Hans said--
that at the very beginning I was too timid about modifying the
questionnaire to fit the region. Of course, when I started out
in 1947 my intention was to add to Marckwardt's North Central
States project, and I simply used his materials. I did add
about 25 or 30 items, but many more might have been included.
And this is one thing that I think you might consider: the
value of including in your questionnaire items which have not
been in the original materials but which you find now to be
significant.

McDavid suggested that an immediately practical application of that con-

sideration would be a review of the LANCS material, especially the Kentucky

records. Those will identify many patterns that can be expected in Tennessee,

Georgia, Alabama, and Mississippi, especially in the Cumberland system that

extends from Virginia and Kentucky through Middle Tennessee into Upper Alabama.

In the larger pattern of American dialects, McDavid noted, the Kentucky records

will provide a link that will join the subregions of Southwest Virginia and

West Virginia with the territory to be explored in the Gulf States. Such a

data base would offer a large and diversified source of information to be con-

sidered in the proposed survey.

Kurath reminded them of Gilliéron's maxim that the best time to compose

a questionnaire is after the fieldwork is complete and, for that reason, the

fieldworkers must always be alert in their observation on variation. Implicit

in this remark was Kurath's earlier-stated recommendation that a questionnaire

must include a core of phonological, grammatical, and lexical items that will

offer systematically contrastive data to be evaluated with research already

completed in other parts of the Eastern United States.

After Allen suggested a review of Cassidy's preliminary findings in the

DARE fieldwork, Kurath emphasized the importance of a minimum questionnaire.

Here, he contended:

> ...since the survey of the type that we have in mind must
> always use a limited vocabulary, the omission of an item or
> the addition of an item is of relatively little consequence.
> As long as you start out in setting up your questionnaire
> for the Gulf States with lexical variants that are already
> recognized along the Atlantic Seaboard, you will have a de-
> pendable base. Again, you see, I am emphasizing that a
> minimum questionnaire must be set up first. You can always
> add to it later.

Allen then turned the discussion to questions of elicitation procedures
in fieldwork. Relating this to editorial problems with the LAUMS data, he
mentioned the difficulty in evaluating data drawn from a variety of interview
situations and asked for opinions on the method used by fieldworkers under
Orton's direction in England. Specifically, he asked, "I'd like to raise the
question of the desirability of having central questions printed so that the
fieldworker must read it aloud."

Kurath said he thought that approach is "damnable" because it results in
forcing the informants to respond as they might in a testing situation. This
is a particularly doubtful procedure when interviewing members of the working
class, upward mobile speakers who might well seek out the form believed to be
preferred at the expense of the usual pronunciation or expression of the local
dialect. Furthermore, he contended, the interview situation will never be a
constant and fieldworkers will invariably use different methods of elicitation
in different circumstances. The generalizations that are derived from data
gathered in that way may lack simplicity, but that does not diminish the use-
fulness of the material:

> The lack of clear semantic differentiation according to the
> situation...gives you some information on the range of meanings

that actually are there. Or, perhaps, you will find uniformity
and agreement, but you may realize certain complicated things
such as deviations between records made by different interviewers
to be significant as well.

McDavid concurred with Kurath's observation:

   Now from the point of view of some of Orton's records, I do
know there is no statement anywhere in his handbook about dif-
ferences in fieldworkers' practices. Nevertheless, when I tried
to check certain grammatical items with the original material,
it was very clear that there are strong personal boundaries among
fieldworkers. Certain of them just never got the colloquial
forms. It seemed odd that here and there would be geographical
territories where only the standard forms of the verb would occur
and these would be shared by all informants within that area.
And central to that problem are the frame questions that Orton
insisted his fieldworkers use. I think some of the fieldworkers
were following too literally the instructions of getting a par-
ticular form in a particular way.

Allen agreed that a rigid questionnaire was not the answer, but he in-

sisted that all fieldworkers must understand the significance of every item

in the work sheets. He freely confessed that when he began his work he hadn't

always done that himself; there had been some misunderstanding about some

items.

   Kurath reemphasized the importance of recognizing differences among field-

workers:

   The range of variants you record will also vary tremendously
on the basis of the different fieldworkers who gather the infor-
mation. If in your questionnaire you ask 'a very common name for
an awkward fellow,' well, one informant will go all out on a
favorite item and run off a whole string of synonyms with real
delight. But here the fieldworker must be considered. Lowman,
for example, never had the patience to listen to the string. So,
you simply have to recognize the fact that the fieldworker's
personality, his particular interest and, of course, and this is
an important point, the extent to which he is familiar with the
local culture and local habits, are all going to be reflected in

the material he collects. There is no avoiding it.

William Van Riper, having collected all of his Oklahoma fieldwork on tape, made the first reference at the meeting to the use of the machine:

> If the interviews are taped all the way through, of course, the editor will have firsthand information of the interviewing technique. He'll get variant responses, more than he wants, and will probably go crazy in his office.

Pederson agreed with Van Riper's observations and mentioned the fact that even the most basic procedures reflect the particular interests of those who organize the plans. He stated the example of the directive in the Cassidy and Duckert monograph, A Method for Collecting Dialect, that the fieldworker must be subordinated to the lexicographer:

> He must not merely gather what pleases him individually, or present it according to a personal system; if materials gathered by many people over a very broad area are to be evaluated, they must be comparable. Collectors must therefore be prepared to follow a system common to all--so far as possible to collect the same sorts of things in the same way; and these must be the kinds of information that will, as nearly as may be, make possible for the editor objective judgments.

McDavid offered a compromise solution. Asking Allen if he had consulted the notebooks indicating the ways in which Bloch and Lowman habitually asked the questions in New England, McDavid suggested that the forms would be useful in evaluating the New England work and might have application in other work as well.

Next was the probable range of regional coverage for the survey. Pederson suggested a consideration first of research already completed and asked Van Riper

to comment first on the status of his project in Oklahoma and of the Wise

collection at LSU. Van Riper explained that his fieldwork was completed for

Oklahoma, the tapes were ready for transcription, and composition and pub-

lication of the material would proceed as time and funds became available.

As for the collection of field records made by the students of C.M. Wise in

Louisiana, Van Riper noted that Wise began in the 1930s with the long work

sheets for the South Atlantic States. He undertook that work as a preliminary

survey with the intention of gathering data that would be useful to Kurath in

planning the extension of the LAUSC Project into the Lower Mississippi Valley.

All of these records were made by students of widely different backgrounds and

include almost as many sets of personal idiolectal boundaries as records because

most of the students did only one interview. On the basis of an informal re-

view of the material, Van Riper mentioned that some of the responses suggested

questionable field procedures, as, for example, the recurrent elicitation of

"it is I" from lower and middle-class informants.

After Kurath recommended that the initial survey be limited to the core

area of Georgia, Alabama, Mississippi, and Tennessee, Pederson asked if the

inclusion of Florida would make the primary area too large. Kurath replied

that it all depended on the availability of resources and that "with nearly all

types of federal grants now you are going to have a time limit, and you had

better be sure you are correct and don't undertake more than you can finish."

Gordon Wood, who had recently completed a postal survey of Tennessee,

Georgia, Florida, Alabama, Mississippi, Louisiana, Oklahoma, and Arkansas, was

asked for his advice. Of the entire area, Wood observed, Mississippi seems to

be the most complicated subregion in the area, and that state should receive

very careful attention. The patterns observed in the postal questionnaire

there seem inconsistent with findings elsewhere, with vocabulary items turning

up in some counties that had no apparent association with responses recorded

in neighboring communities. Elsewhere, said Wood, the territory is quite co-

herent. North Florida and South Georgia share a common culture, as do the

border communities of southern Tennessee and those in northern Georgia, Alabama,

and Mississippi.

After a brief exchange of intelligence on bibliographical resources and on

some of the local historians and folklorists who might help, Juanita Williamson

spoke first on the rural/urban factor. As in Memphis, where the speech of

Upper Mississippi has been heavily influenced by the Memphis focal area, she

observed, the urban dialects preserve many features associated with neighboring

rural areas. This is most clearly documented in phonology, but the verb forms

and vocabulary should also be investigated with an eye to this subregional inter-

action.

McDavid returned the discussion to a consideration of the primary settle-

ment areas and their relationships to the development of later speech communities.

James McMillan recommended a consideration of local settlement patterns in the

determination of the target area for the survey:

> Population movement here was not like that along the northern
> boundary of the North Midland dialect area, where it was due west
> and branched out from there. The Florida Panhandle and all of
> North Florida, I should say, down to Gainesville, must be included
> in this project. Historically, it belongs to the region, and, stra-
> tegically, this Lab includes Florida in its constituency. That's
> the answer to the question Hans raises. There were three movements,
> for example, in Alabama. First, there were migrations from East
> Tennessee. Then settlers came from Georgia and South Carolina--
> skipping over the Indian Reservation in eastern Alabama; that lasted

40 years beyond the settlements in the rest of the state and
may account for the dialect boundary Gordon finds at the
Georgia-Alabama border. Then a third source was the population
movement inward from Pensacola to Mobile and, later on, down
to New Orleans. In the central part of the Gulf States, you
have the Midland speakers moving from the North, the Southern
speakers coming across from the east, and God-knows-what coming
in from the coast, and that population spread up from Mobile
Bay in a pattern completely independent of those established
from the other two directions.

The final topic of discussion during the morning session concerned pre-

liminary field procedures. Van Riper began the discussion:

I'd like to mention a point of procedure. Would it be pos-
sible to take a fieldworker and load him down with a tape re-
corder and all the tape he could carry and send him into the
coastal regions of Alabama and Mississippi? We might tell him
to find somebody who is representative and then let the man
talk for 40 or 50 hours.

Kurath, McDavid, and Allen followed with discussions of the strengths and

weaknesses of the tape-recorded interview. The principal questions here con-

cerned the effect the tape recorder might have on the interview situation,

the enormous quantity of data the auditor would have to control, and the great

amount of time it would require to transcribe from tape.

Van Riper, however, persisted:

Well, I'd like to speak for tapes. This has to do with Harold
Allen's suggestion of a frame which reduces the editor's job.
If you use tape, you are certainly putting a tremendous burden
upon the editor, but, on the other hand, the burden is there
because you have more information, more accurate information.
Now, when the fieldworker elicits one response, he's getting
ready for another, and the informant maybe does not shut off,
and he will keep going and the recorder will record. And
there, maybe three or four items a fieldworker might not notice,
spontaneous items come out, items he will feel obliged to re-
quest later in the interview, unless he takes time to listen to
the tape between sessions.

McDavid affirmed Van Riper's contention of the value of gathering more data with the tape recorder, but he rejected the notion that it would significantly shorten the interview. He spoke further on the abuses of the tape recorder by inexperienced fieldworkers, but he stressed the importance of the tape recorder in gathering syntactic information, responses to items that are rarely satisfactorily elicited through direct questions.

Pederson explained that his Chicago, Minnesota, Missouri, Georgia, and LAMSAS fieldwork had all been preserved fully on tape and that the current Dialect Survey of Rural Georgia was being recorded in the same way. McDavid noted that Shuy's Detroit survey was recorded on tape, but Kurath and Allen reminded him that was not an Atlas survey. After Pederson acknowledged their observation, Allen asked, "Are you thinking of taping all of every interview?" Pederson replied, "Yes, sir; it's the only way I know how to do it." No one contested that, and the meeting adjourned at 11:30 a.m.

THE SECOND SESSION

The meeting convened at 1:30 p.m. on the afternoon of May 16. Pederson outlined seven questions for discussion:

1. How many communities should be selected?

2. How many informants could be interviewed?

3. How will ethnic representation be handled?

4. How much contemporary linguistic description can be used?

5. How much of the grammatical system can be realistically surveyed?

6. How closely can suprasegmental phonology and paralanguage be sensibly covered?

7. To what extent can the phonetics of the LAUSC Project be refined?

McDavid began the discussion:

Let's take the North Central States sample as a point of
departure. The coverage there was originally determined in
Ohio, where the earliest records were made. Marckwardt planned
to investigate 25 communities in each of the states. In each
of these 25 communities, he planned to conduct two interviews,
although in many places we ended up with considerably more. We
did four in Detroit, which was obviously not enough for a pop-
ulation of three million people, but it provided a framework
for Roger Shuy's work there, and the interviews we did in Chicago
provided the framework for Lee. I think we have two other things
to think about: the optimum number of informants and the range
of social variation to be covered. We will probably find sharper
social distinctions in the Gulf States than in the Midwest because
of the peculiarities of their respective social histories. Those
questions can only be answered in terms of the range of the plan
we develop.

This opened a general exchange among all participants, with emphasis placed

on the importance of basing selection of communities and informants on the best

available social history and on the findings of dialect research already com-

pleted in the Gulf States. McDavid finally recommended a minimum sample of 50

records for each of the core states, Mississippi, Alabama, Florida, and Tennessee

in 25-30 communities in each of those states, with fewer in Georgia.

McMillan returned the discussion to the question of informant types:

Lee, before we leave this question, I want to emphasize some-
thing Raven said earlier about the finer network of social dif-
ferentiations here. Whereas the surveys of the Upper Atlantic
Coast could presume a classless society, we just can't do that.
And it is not a primary class distinction. There are three his-
toric distinctions of three historic social groups in states
like Georgia, Alabama, and Mississippi. You simply can't start
with three informant types--cultured, old uncultured, and younger-
middle-aged uncultured representatives of a single social group.
You've got to recognize two distinctive white groups. And his-
torically, there is as much segregation between the sawmill or
cottonmill workers and what sociologists call the "community," as
there is between white and Negro, although it is done in a dif-
ferent way.
People going back to Rupert Vance, his students and followers
later, have made this three-part distinction consistently, and they

can draw sharp lines. And it seems to me, by the way, that
two of them--the cultivated white and the Negro groups--have
both prestige and non-prestige dialects. The third group--
[the millworker or sawmill worker group] does not have a prestige
dialect. So, this gets complicated and you cannot cover the
problems with spotty sampling.

Pederson asked if that white "community" is a coherent unit, a single

piece, or if it might need further analysis according to factors of caste and

class. McMillan suggested "it's pretty unitary." McMillan and Pederson agreed

that more than two informants would be necessary in the coverage of communities

such as those described by McMillan.

Kurath then added:

I'd like to comment on this too. It's not true that the
plans for the New England States were made without regard for
social evaluation, but the limitations imposed upon the pro-
ject finally resulted in a simpler schedule. We started out
with the notion of representing the principal groups, and you
can see from the record that a plan was pursued in a deliberate
way.
That experience reinforces what I said before about the prac-
tical problem of setting up a minimal plan and then, as I sug-
gested this morning, such a schedule can always be elaborated.
I think the practical procedure is to set up first a minimal
plan for communities and a minimal plan for sociological sampling.
Then, as far as the communities are concerned, you can prepare
a list of secondary or alternate communities that can be covered
if resources are available. That secondary sampling will be
decided by the fieldman, who must decide what kinds of information
are needed, which kinds of information can only be obtained in
certain localities, and what kinds of informant selection is avail-
able to provide such information. You simply must have a minimum
plan. Otherwise, you start in with a large plan and may not be
able to get through it.

McMillan then asked: "Hans, would you agree that a minimum number of

communities with a maximum number of informants might be desirable?"

Kurath replied: "There's something in that."

McMillan then elaborated the value of such a survey being in the fact that we would learn more about social stratification, and that in the long run would be more interesting than learning the results of a fine-mesh of geographical distribution. Rudolph Troike concurred and mentioned a stratificational study that was planned for the summer in Northeast Texas.

Pederson suggested deferral of further discussion on sampling until the remaining four topics of the agenda had been covered. Kurath was asked to comment on the appropriateness of the work sheets for the South Atlantic States, supplemented by findings of Atwood, Van Riper, and Wood, as a source of items to be selected for a minimum questionnaire. Kurath acknowledged that most of the essential items would be there. Pederson then asked more specifically what kinds of changes in those work sheets might Kurath recommend now on the basis of his experience editing the LANE and LAMSAS materials. Kurath noted that the most useful changes would be in providing more environments for the study of phonic variation and phonemic incidence. This had been an early criticism of his method by the structuralists, and a legitimate one within the framework of a reasonable questionnaire.

Those comments provided the transition to the fourth item of the agenda, "contemporary linguistic description" and its relationship to the revision of methods in conventional linguistic geography. Pederson suggested as a starting point a consideration of the criticism organized in Glenna Pickford's essay in Word. To this, Kurath replied:

> She was the California sociologist who said everybody knows American society is divided into five groups. The editor of the journal of the American Sociological Society asked my opinion of the manuscript, and I told him. Later, she sent it to Weinreich at Word, and he published it. After it appeared in print, he invited me to write a reply. And I said, in effect, 'I'll be damned

if I will.' So there it is. I think it should stand by itself.
Any sociologist or linguist who might take it as a serious state-
ment will have to deal with everything it says, and that will be
fine. But it is interesting reading.

Pederson then asked whether the questions about structural dialectology

raised by Stockwell and Weinreich might be usefully incorporated in plans

for the proposed survey. Kurath responded in this way:

Weinreich asks, 'Is structural dialectology possible?' Well,
if a language has structure and dialectologists are aware of the
fact, then, of course, it is possible.

Troike then recommended serious consideration of Sledd's "Breaking,

Umlaut, and the Southern Drawl." He pointed out that many valuable obser-

vations about phonology can be identified with such an analysis, but the

question others asked him concerned how such an analysis could be anticipated

by the organizers of the questionnaire and how such analysis could be con-

sistently sustained by fieldworkers during the interview situation. The sense

of the discussion can be summarized in the comments of Kurath and McMillan.

Kurath concluded:

One can say that the dialectologist can afford to be quite
humble. As we provide the data on the basis of observation,
we depend heavily upon the training of the observer. The in-
terpretation either in sociocultural or structural terms should
be determined by the needs of the analysis, e.g., the point of
view of a psycholinguistic study might be perfectly legitimate
in one instance, but it surely would not serve as the sole basis
for all observations. What we need is data, and, as dialectologists
and as linguists, I hope that better data will make better structural
descriptions, better transformations.

To that McMillan added:

I don't think it makes a great deal of difference to a dialectol-

ogist whether or not you describe the data with transformational
rules. You simply list the correlations: <u>here's my bill</u>, <u>here's</u>
<u>my billy</u>; <u>here's my field</u>, <u>here's my filly</u>. They have a relation-
ship. Structurally, you can simply list them and show close re-
lationship. If you want to, you can go on and state this relation-
ship in terms of some sort of rules way off yonder, but you do not
change the basic data.

Troike then suggested:

Since we are talking about methods, I would like to bring up
the problem that seems to be very crucial, the recognition of
multiple styles of individual speech, as introduced in Labov's
work. Some correlations between styles and phonological varia-
tion have been outlined in his work, and these might be useful
considerations in planning future fieldwork.

To this recommendation, several responded that the problem with the approach

is that it imposes great restrictions on the way the fieldwork must be con-

ducted and that this might interfere with the gathering of general information,

which seems of a higher priority. Kurath noted that a beginning was made in

New England by marking responses that occurred in free conversation and that

these "c" forms are there to be compared with those elicited through direct

questioning.

Pederson asked if styles, as well as suprasegmental phonology and para-

language, could not be observed later if the interviews were preserved on

video tape. If the equipment is too cumbersome, he suggested bringing every

tenth or twentieth informant to Atlanta to be interviewed in the Laboratory.

Watson noted that video equipment was owned by the Laboratory and that it was

easily transported, concluding that SEL had the "largest amount of video

equipment in the three-state area," i.e., South Carolina, Georgia, and Florida.

Williamson suggested the use of other machines to be used with reading

passages, as a way to get more nearly systematically contrastive data, data

that would be especially useful in the analysis of the language skills of

students. Van Riper emphasized the importance of keeping the informants

relaxed and observed that taking the informant's picture would certainly

not help remove tensions.

Roberts and McDavid noted that the study of suprasegmental phonology and

paralanguage were really inseparable from the study of speech--the segmental

units, the consonants and vowels. They urged that Kurath's advice be heeded

and that the survey concentrate on the primary responsibilities of conventional

linguistic geography.

Pederson suggested that the remaining time might be usefully spent con-

sidering the question of recruiting and training fieldworkers. McDavid was

asked to begin this discussion, and he said:

> I suppose the best thing to be done about getting good field-
> workers is to go to church regularly and hope the Lord will pro-
> vide for you. You can do a good deal to train some people, but
> I would say from my own experience both in the field and in look-
> ing over material others have done in the field and my attempt
> to train fieldworkers, a person either has to have a natural lik-
> ing for talking to all kinds of people or he has got to learn to
> acquire that liking. I think that phonetic training and other
> things of that kind are extremely important, but just looking at
> some of the evidence turned in, the fieldworker's ability to re-
> late to a wide variety of people and a wide variety of situations
> has got to be there either by nature or by nurture. Otherwise,
> you're just not going to get your data. Now I was one of those
> who had to acquire that particular knack. Now Lee and Ray O'Cain,
> who is working now in Charleston, come by it naturally. Fred
> Cassidy said O'Cain is the best natural fieldworker he has in the
> DARE Project. He also seems to be a natural field phonetician.
> Ray has a good deal of training in various kinds of systematic
> linguistic analysis, but he is capable of suspending that when he
> goes into the field.
>   I think a fieldworker has got to be interested in people,
> interested in regional culture. To a great extent, he must also be
> able to eat and drink almost anything. I would say the person who
> could be a good practicing politician and a person who could be a
> good practicing fieldworker have a great deal in common. You cannot

be aloof. You've got to be able to get out there and stick
to it.

Van Riper had a phrase about a certain group of linguists.
He said they were like the American people. They like the
music, but they hate to walk and they would rather get on the
bandwagon of the moment rather than go out and get their own
data. I think that this particular yearning to see what
actually is out there has got to be part of the make-up of
the fieldworker. I think that if this is present, then you
can overcome some of the personality problems. This is im-
portant because he must learn to get on with people, to learn
to trust what he hears rather than what he thinks he ought to
hear. Finding such people, however, is largely a matter of
accidence, whether they come from a department of English,
linguistics, history, sociology or from somewhere else.

After this basic statement, Kurath, Van Riper, and Allen offered sug-

gestions for recruitment and training, with McMillan, Roberts, Williamson,

and Wood suggesting students who might be good candidates for the work.

During this exchange, an important question concerning the recruitment of

Negro fieldworkers was raised by Williamson. She pointed out the need for

such investigators if we intended to gather the unveiled usage of Negro in-

formants. Pederson suggested that if we intended to use four regular field-

workers to gather 425 records, half of which would represent Negro speech,

that two of the four fieldworkers should be Negro.

Kurath then summarized the training program used in New England, and

questions were raised as to the ways such a schedule might be best implemented

now. Summer sessions were recommended with Kurath insisting on the fewest

number of fieldworkers to get the job completed in the prescribed time. McDavid

noted that the members of this conference were in quite the same situation as

were those who planned the Linguistic Atlas of New England and that it would

be important for all to cooperate in the recruitment of workers and in the ex-

change of intelligence.

The meeting was adjourned at 3:30 p.m. with plans for the following

day outlined: Roberts' comments on sources of outside support and Allen's

summary of editorial plans and problems in the editing of the Linguistic

Atlas of the Upper Midwest.

THE CONFERENCE IN RETROSPECT

In 1981, any reader of the Basic Materials for this project will rec-

ognize that most of the procedures implemented in the project had their

origins in the discussions of May, 1968. The following remarks are intended

to bridge the gap between that conference and the first of the four interim

reports, published in 1972.

Shortly after the conference, McDavid and Pederson edited material

from the aforementioned surveys and combined them with the long work sheets

for the South Atlantic States. These were organized in an 85-page format,

an instrument more elaborate than the generalized short work sheets used

elsewhere, but considerably shorter than the South Atlantic base form. The

principal additions are summarized in the "Introduction" to the Manual.

In November, 1970, at the annual conference of NCTE, McDavid prepared a

training tape for fieldworkers that became the standard reference for all LAGS

fieldwork. On the Monday afternoon following the convention, Pederson met

with William Labov, who had remained in Atlanta gathering data for his own

research, for a two-hour conversation. In some respects, that meeting was

as important as the conference of 1968. Pederson asked Labov directly how

he would organize a general dialect survey. Labov responded: "Just the way

you are doing it." Now, that observation did not imply that Labov would be

interested in doing such work, but it convinced Pederson that detractors of

the atlas who cite Labov as an authoritative critic of Atlas methodology

clearly miss the mark. Labov stressed the importance of Pickford's
criticism of atlas method and acknowledged that much of that had been pre-
empted by general surveys in urban areas, such as those done in San Fran-
cisco, Memphis, and Chicago after the publication of her essay. He did,
however, urge that LAGS keep the central arguments in mind concerning
sensitivity to social organization, racial distribution, and urban sampling.
Those comments of his are inseparable from later plans to deepen the urban
coverage and to provide the best possible representation of black speech.

Preliminary fieldwork was then initiated in Upper Alabama by Charles W.
Foster, in Middle Alabama by Anne Malone Fitts, in Lower Mississippi by
Christine Unger, and in Upper Georgia by Rueter, Pederson, and students from
Georgia State under the direction of Charles E. Billiard. This information
was transcribed by Foster, Unger, and Pederson, and the findings were combined
with those of the Dialect Survey of Rural Georgia in the composition of the
revised LAGS work sheets in 1971.

In January, 1971, Pederson completed social research on the territory
and organized a grid system and a classification of locality types that
determined the selection procedure for the project. Following Kurath's
recommendation, 157 (later expanded to 176) units were identified for
minimal coverage. The initial base plan was to interview a single folk
speaker in each of 157 communities (i.e., counties or sets of counties that
had been grouped according to their social history).

In April, 1971, Pederson received a grant from the National Council of
Teachers of English to survey the dialects of East Tennessee within the frame-
work of the grid. The following year, Foster received a similar grant. Peder-
son's findings are summarized in Working Paper #8, "The Regional and Social

Dialects of East Tennessee: A Preliminary Overview," the final report of that project to NCTE.

In 1972, the decision was made to depend exclusively upon the tape-recorded interviews, to restrict the data base to evidence that was preserved on tape, and the ramifications of that decision, of course, marked a departure from Kurath's approach but a reaffirmation of the methodology developed by H. Rex Wilson and W.R. Van Riper. The basis for this decision emerged from the Dialect Survey of Rural Georgia, where it was found that if the transcription was limited to a small number of scribes—there, Hall, Rueter, and Pederson—it was much easier to evaluate widely disparate field records. And when the decision was made, the project had no assurance of recruiting full-time workers because it was without funds.

Among the suggestions raised at the conference, two were pursued without success. These concerned the use of video equipment in the conduct of the interview and the employment of black fieldworkers. The mechanical problem was never resolved because resources and logistics made the effort ultimately impossible. The human problem was much more complicated than members of the conference might have anticipated. The failure of LAGS in both of these areas of field investigation surely diminished the potential value of the research, but the problems implicit in each of these areas discouraged future efforts to develop them.

Video equipment was included in a proposal to the U.S. Office of Education in 1968 and in another to the Stone Foundation the following year. In both instances, the proposal was rejected in part because of the large budget, and the video component of the budget was the most convenient way to reduce

substantially the size of future requests. Secondly, considerable attention was being given to Southern culture by public and private broadcasting companies, all of whom were trying to document on film as much of the regional life as possible. Since they were not getting a systematic view of the local language, it seemed appropriate to place our emphasis exclusively upon the audial element. Finally, the introduction of video equipment in the field or the conduct of interviews in a laboratory did not promise results that would be useful. For all of these reasons, the video component of the program was abandoned in 1970.

The tactical problem of using black fieldworkers was quite insoluble under the present social conditions in the rural South. The regional caste system made it virtually impossible to expect a black fieldworker to operate effectively among whites, especially in rural areas. Furthermore, black students who might have made good fieldworkers invariably had other plans. Usually members of the first college-educated generation of their families, they had responsibilities to pursue careers. None of those, unfortunately, had selected linguistics as a permanent occupation. Finally, among those black student fieldworkers who did contribute single records in the project, few demonstrated talents that could be developed in the field. All had urban orientations and had been quite isolated from rural life, and none of them had time to travel from city to city in the Gulf States. In the South in 1970, a black fieldworker's range was limited to studies of rural black speech and, perhaps, to much of the urban society, with the certain exclusion of working-class and indigent whites.

In the spring of 1973, with the assistance of A. Hood Roberts, a small grant was awarded by NEH to support the fieldwork of Barbara Rutledge in Middle

Tennessee. McDavid remarked after auditing Rutledge's fieldwork that she was perhaps the best natural fieldworker to surface so far in the project. She was also remarkably intelligent, responsible, and energetic. Rutledge agreed to work a full two years as a fieldworker if further funds became available, and Edward W. Crist agreed to join the project for a full year also. With the completion of the Manual and the commitment of these two able fieldworkers, a large proposal was submitted to and approved by NEH. That marked the beginning of the systematic development of the project. With the cooperation of Emory University, scribes were recruited and awarded tuition and fee awards that made possible the completion of Ph.D. work at Emory, and several additional regular fieldworkers were added to the staff.

The plan articulated by the participants at the SEL conference and the final form of the LAGS survey are broadly different in many respects, but the aims and spirit brought to that meeting by those linguists has been sustained through the past decade with their steady support. Almost all of them have remained active consultants and have freely given of their time and advice as the program has moved along.