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Sociolinguistics: Current Trends and Prospects

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Report
Of The Twenty-Third Annual
Round Table Meeting
on Linguistics
and Language Studies

Roger W. Shuy
Editor

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INTRODUCTION

The Twenty-Third Annual Georgetown University Round Table was held on March 16-18, 1972. The present volume represents the proceedings of the plenary sessions of that meeting. The theme of the meeting was: 'Sociolinguistics: Current Trends and Prospects'. It was with some hesitation that this topic was originally suggested since the development of sociolinguistics is so recent that the committee was not at all certain how widely it would be received. However, the response to the initial announcement was overwhelming. Fifteen papers were delivered at the five plenary sessions, fourteen of these are made available here. (It is regretted that the presentation by Erving Goffman will not be included at the author's request.) The formal discussion which was submitted to the editor in writing has been included although general discussion from the floor has been omitted in this year's proceedings.

The Sociolinguistics Committee of the Social Science Research Council originally proposed the topic and jointly planned the session with university representatives; members of that committee include Dell Hymes (Chairman), John Gumperz, William Labov, Charles Ferguson, Allen Grimshaw, and David Jenness who worked with the program chairman and devised nine small-group interest sessions which met for three hours each on March 16 and the five plenary sessions which began on the evening of March 16 and extended through noon, March 18. The purpose of the interest groups was to bring together active scholars and advanced graduate students in an intimate setting, discussing needed research in specific areas of sociolinguistics and evaluating current progress to date. It was decided that the group size should be limited to 20 when possible. Applicants for admission to the interest groups were requested to reserve space in advance. Interest group leaders, chosen by the planning committee,
were responsible for selecting their participants from among the applicants. The following interest groups were offered:

1. Ethnographic Semantics. Chairman, Brent Berlin, University of California.
2. Sociolinguistics and Religion. Chairman, William Samarin, University of Toronto.
6. Paralinguistics, Kinesics, and Teacher Training. Chairmen, Hugh Mehan, University of California, San Diego, and Bruce Fraser, Boston University.
8. Pidginization and Creolization. Chairman, David DeCamp, University of Texas.

The interest groups took quite different formats, ranging from predistributed working papers, which were discussed at the meetings, to a more informal discussion format. Plans have been made to publish the papers of several of these sessions. Georgetown University Press will publish the papers which grew out of interest-group sessions four and five, *Language Planning: Current Issues and Research*, edited by Joan Rubin and Roger W. Shuy and *Language Attitudes: Current Trends and Prospects*, edited by Roger W. Shuy and Ralph W. Fasold. Reports of these sessions are included as an appendix to this volume.

The papers of the plenary session are intended to represent the wide scope of sociolinguistics as it is viewed today. Sociolinguists are concerned with ways of analyzing variability in the writing of grammars and the presentations by Fraser, Bickerton, Labov, and Bailey are addressed to this issue. The relationship of sociolinguistics to social interaction is the focus of the papers presented by Gumperz and Sacks, while sociolinguistic surveys are the topic of the presentations of Fishman, Das Gupta, Whiteley, and Haugen. The intersection of sociolinguistics with education is another current concern which is addressed by Cicourel, John, and Frender and Lambert. In concluding the session, broad overviews of the influence of
the fields of linguistics and sociology on sociolinguistics are presented by Fillmore and Grimshaw while Hymes summarizes the field in general in the final paper.

Soon after the conclusion of the conference, the sad news of the death of Wilfred Whiteley was announced. His presence will be sorely missed by his colleagues and friends.

As chairman, I would like to express my thanks to The National Science Foundation and The Social Science Research Council who helped support The Round Table. Specifically, David Jenness of the SSRC provided invaluable assistance along with the Sociolinguistics Committee of the SSRC. I would also like to thank all the graduate students in the Sociolinguistics Program at Georgetown University who provided valuable assistance in the planning and execution of the meetings. In particular, Douglass W. Gordon, the chairman of arrangements, Jan McDonald, departmental secretary, and Mary Lou Shuy, my wife, worked long and hard on hundreds of details. In addition, Marcia Whiteman, Al Rey, Marcel Charland, Jackie Pickett, James Woodward, Faye Vaughn-Cook, Lucienne Skopek, Joe Bellino, Jennifer Sullivan, O. G. Harper, Peg Griffin, Carolyn Bins, Bill Riley, Marilyn Glenn, David Minderhout, Mary Alice Minderhout, Jan Smith, Judy Riley, Elaine Gordon, Mark Connelley, Larry Biondi, Margaret Booth, Emmanuel Chia, Ed Anderson, Suzanne Podvoll, and Maria Ibba are to be thanked for their excellent assistance. Special thanks go to Miss Marieluise Baur who prepared photocopy.

Roger W. Shuy
Editor
WELCOMING REMARKS

ROBERT LADO

Dean, School of Languages and Linguistics
Georgetown University

Ladies and Gentlemen, on behalf of Georgetown University I am happy to extend to you a cordial welcome to this the Twenty-Third Annual Georgetown University Round Table. In keeping with a tradition of not being traditional, but rather of exploring the frontiers of scientific linguistic and related studies, the topic of this Round Table focuses on the most rapidly developing linguistic field today, that of Sociolinguistics, its trends and prospects.

It is easier to say more or less cynically that 'man can travel to the Moon but cannot understand the problems of living with his cultural neighbor', than to do something about creating understanding through study, research, and discussion.

The vigour, diversity, relevance, and sheer excitement of the field of Sociolinguistics is clearly visible in the topics listed for discussion and the names of the contributors and participants. The result is certain to be greater insight and understanding of the linguistic problems we live with, of the invisible webs that so often deny full flight and freedom to the spirit of man.

Georgetown University is proud to share sponsorship for this Twenty-Third Round Table with the Committee on Sociolinguistics of the Social Science Research Council.

We thank the speakers who have come to share with their colleagues the results of their labors, and we thank the participants for their contribution to the discussion and for their presence.
Within the framework of generative grammar, rules have characteristically been classified as obligatory or optional. As the names imply, obligatory rules must be applied in the course of a derivation if such application is possible, while optional rules are applied at will. In the past few years, however, linguists have become aware that so-called optional rules are not quite so optional as had been initially assumed. Various studies have shown quite dramatically that these 'stylistic' rules are not in free variation, but are systematically responsive to a range of phenomena, including the particular dialect, the speaking style, the social setting of the discourse, the linguistic environment, the age, sex, and other factors. The purpose of this paper is to examine the notion of optional rule, to see how it has been adapted in the form of a 'variable rule', and to suggest its role in linguistic theory.

As a foreword, however, I would like to make clear the perspective from which I present the subsequent discussion. First, following Chomsky's basic line of thought. I take the linguist to be concerned primarily with an ideal speaker/hearer in a completely uniform speech-community, who knows his language perfectly, and who isn't bothered by irrelevant factors. This position in no way limits the linguist from being as general or as specific as desired in actual descriptive studies, but does emphasize that the results should capture the generalizations underlying the language of the speakers, not the superficial vagaries of the period during which they were observed. In particular, I do not understand recent suggestions that there is a linguistic and a sociolinguistic grammar for a speech community. It seems to me that linguistic analysis, insofar as its goal is the characterization of the language ability of the native speaker, may take as its focus certain broad aspects
of the language (e.g. English nominalizations), theoretical aspects (e.g. constraints on movement rules), or some very specific phenomenon (e.g. the study of contraction and deletion of the English copula in the vernacular dialect of inner-city black teenagers). All of these, aspects of some (hopefully) emerging English Grammar, attempt to characterize within the available linguistic theory the linguistically relevant sound-meaning correspondences. There is nothing necessarily more sociolinguistic about a grammar fragment that deals with the expletives of fifth grade boys in public school lavatories than one dealing with the description of relative clause formation in English. The level of specificity is simply different.

Second, I wish to clarify the notion of the validity of a grammar. Labov has written that

the question of validity has been raised many times in regard to generative grammar, but Chomsky has explicitly rejected any definition of validity which would depend upon correlating linguistic rules with behavioral or biological data (1965:9). In its present form, a generative grammar is one of many models that are descriptively adequate, selected by an internal evaluation measure. The competence/performance distinction serves to insulate generative grammar from the definition of validity advanced in our first section—that our theories must apply to the unreflecting language used by ordinary people in everyday life. (Labov 1971:451)

The implication of this passage is that generative grammar, in particular the competence/performance distinction, is removed from 'real life' linguistics. This is simply false.

There are two points to make here. First, the reference to 'behavioral data' in the above quoted passage does not refer to the data gathered from observations of members of the speech community being described. Reference to the page in Aspects cited will show that the discussion was concerned with the neutrality of the grammar between the speaker/hearer, and that the grammar rules were not to be construed in any way as having psychological reality, i.e. were not mirrored by neurological networks or whatever. On the contrary, Chomsky in particular has speculated on the ways in which dialect differences might be reflected in the grammar of the language.

Second, and more important, I think Labov's remark is more of an historical comment than a critical one. For the last twenty years, linguists have worked within a framework in which the intuition of the native speaker could (often) provide sufficient data for the formulation of linguistic generalizations. Predictably, the results (impressive to say the least) have been at the level of specificity at which intuitions can
play an effective role. The results do indeed reflect the unreflecting speech of ordinary people in every-day life; but they are very general results, usually involving high-level generalizations. As the general outlines of English grammar have become more clearly formulated (not to suggest that anything like a final form is in sight), interest has turned to more specific details of grammar, where intuitions often fail (Cf. Carden, 1970; Labov et al. 1968; Labov 1969; Bickerton 1971; Wolfram 1971).

As the specificity of description has had to increase, the methodology has had to change. The linguist can not be satisfied only with the paradigm of 'Can you get this sentence?' but must carefully observe the actual language use as well. Carden (1970), for example, in attempting to characterize a part of the English quantifier system found that a number of speakers changed their minds about the interpretation of a set of sentences under investigation during a second interview. Such variation in intuitive judgment strongly suggests that direct investigation of this aspect of grammar will create more artifacts than insights. In short, there is nothing insular about the competence/performance distinction: linguists are after language competence. There is, however, a variety of methodologies the linguist can utilize to get at this competence, some of which demand more direct contact with members of the speech community than others.

Finally, it is not clear to me where a grammar begins and ends (Cf. Labov, this volume). That is, what is the language ability of a native speaker such that the grammar of this speaker, or his speech community, should capture this knowledge? The mature speaker has a wide range of language competence. Speakers can tell whether a sentence is acceptable, if two sentences mean the same thing, whether a particular utterance is being used as a threat or command, that tick cannot be an English word but blick can be, that using ain't is socially stigmatized in formal speech, that we can understand some dialect speakers better than others, and so forth. All of these abilities are language abilities. Are they all part of the grammar of a speaker? Frankly, I don't know. Nor do I think it makes much sense to ask the question at this point. I think we are still at the stage of linguistic development of proceeding with three simple steps: What is going on? How can we characterize it accurately? How can this characterization be integrated into the existing linguistic theory?

On the other hand, I do think it is important to approach the issue of grammar writing from a narrow focus. Specifically, I submit that a grammar should concentrate on the language of the speech community under consideration and not on factors such as the relationship of this dialect to other dialects, the history of the dialect and the role of history on the synchronic form, the theory of linguistic change, mutual intelligibility of dialects, the social implications of certain dialect
features, the implications of the dialect for education, or how the dia-
lect is acquired. These are all very interesting aspects of the study of 
language, but are not, I suggest, germane to the writings of a grammar.

From this orientation, I would now like to look at the notion of op-
tional rule in grammar.

Optional Rules

The distinction between obligatory and optional rules in grammar has 
traditionally held (if tradition can be established within 20 years) 
an either/or position: obligatory rules are those which must be applied 
for the resulting sentence to be grammatically well-formed; optional 
rules are those which convert already well-formed sentences (or sen-
tences which, when the derivation is finished, will be well-formed) into 
other well-formed sentences.

That a particular rule has been designated obligatory is often a func-
tion of the form of the existing linguistic theory and the details of the 
language analysis, rather than a reflection on the nature of the language. 
For example, the rule of Complementizer Placement (in one version) 
takes an underlying string of the form John expected—Mary—for—to— 
leave on time and moves the for into the position immediately preceding 
the subject NP, Mary. This rule is obligatory since the sentence John 
expected Mary for to leave on time is unacceptable English, while John 
expected for Mary to leave on time is quite all right. If the underlying 
base component phrase structure rule were to have distributed the for 
and to into the appropriate positions (a task within their power), the 
rule would not have been necessary at all.

The rule of Subject-Verb Inversion (again, in one version) is an obli-
gatory rule which applies to sentences having an initial question marker 
Q. When the deep structure phrase marker contains the Q, the semantic 
interpretation is that of a question rather than a statement, and the rule 
of Subject-Verb Inversion must apply to produce a sentence with the 
appropriate surface structure form. This obligatory rule could be 
made optional, however, if the semantic interpretation of the sentence 
were determined, at least in part, by the surface structure form: a 
sentence with the question surface structure would receive the question 
interpretation. Although this approach has not been suggested (since 
Syntactic Structures) to my knowledge, changing the rule obligation of 
Subject-Verb Inversion would fit within the Interpretivist Theory (cf. 
Chomsky 1971).

There is nothing I know of which logically requires a rule, if 
in fact it exists, to be either optional or obligatory. Moreover, I sub-
mit that what we can expect to find upon further examination of various 
dialects of English, is not a sharp obligatory/optional differentiation of 
rule obligation, but a continuum of optionality, determined by the function
of the rule in the language. I am suggesting that the degree of
optionality of a rule depends crucially on what role the rule plays in the
language. Note: the language, not the grammar. The notion of rule
optionality should be extended beyond the narrow criterion of whether
or not it affects the acceptability, or well-formedness, or grammati-
cality, of a sentence to the broader scope of how it functions in the lan-
guage as a vehicle of communication.

To clarify this point, I want now to sketch out several types of rules
which have radically different functions in the language. Of importance
in this discussion will be not the precise formulation of the rule (even
if this were possible) but the fact that a particular language phenomenon
exists and is rule governed. It should be obvious that the following tax-
onomy is highly speculative and very tentative. My concern is not with
the particular taxonomic detail but that such distinctions of rule function
do exist and that they play a role in linguistic theory.

A. Rules to Adjust From to Meaning

These are rules like Imperative Formation, Subject-Verb Inversion,
or Emphatic Stress Assignment, where the meaning of the sentence de-
pends on the ultimate phonological or syntactic form of the sentence.
It does not make sense to talk about these rules as obligatory or optional
since their existence is an artifact of the form of grammar.

B. Rules to Adjust Grammatical Detail

These are rules which generally provide more specific information
about the intra-constituent relationships in the sentence but which, if
not applied, do not alter meaning and usually do not hinder intelligibility,
though this of course depends on the context in which the sentence is
uttered. For example,

Number Agreement: John run/runs every day

Relative Clause Formation: I saw the boy he/who took all my candy

Case Agreement: Mary and him/he are leaving now

Reflexivization: He shot him/himself in the leg

There-Insertion: There is someone in the room

Possessive Formation: Mary/Mary's bag is full of goodies
Such rules, while relatively obligatory in some English dialects, are quite optional in others. We can hypothesize that the obligation of such rules would increase as the contextual information available to the discourse decreases.

C. Rules to Reduce Redundancy

These are rules which take well-formed structures and alter them, thereby reducing the amount of redundant material in the sentence. For example,

Conjunction Reduction: John saw Mary and John saw Peter
                        John saw Mary and Peter

Gapping: John saw Mary and Harry saw Peter
         John saw Mary and Harry Peter

Agent Deletion: The dog was hit by someone
               The dog was hit

Copula Deletion: He's in the toy store
                 He in the toy store

Equi-NP Deletion: John expected that he (John) would go
                 John expected to go

Pronominalization: John was convinced that John was the smartest
                  John was convinced that he was the smartest

Relative Clause Reduction: The boy who was in the store was Harry
                          The boy in the store was Harry

Do so: We took the train and they took the train too
       We took the train and they did so too

/d/ ~ /t/ → [D]: The ladder [læ:dr] fell over ~ The latter [lætər] seems best
                        The ladder [læ:Dr] fell over ~ The latter [læDr] seems best

Contrary to the rules in B, above, which for some dialects are seldom optional, the rules in C are, I believe, always optional though the frequency of application and thus the degree of obligation will depend on the speech context. We can hypothesize that the optionality for such rules will increase as the style of discourse becomes less formal.
D. Rules to Conform to Phonotactics

These are rules which arise out of the need to have sentences of the language conform to the acceptable phonological outputs of the language. The stress and intonation of English (for that matter any language) are poorly understood and the following examples are very speculative.

Heavy NP’s Last: The boy who arrived from Kansas’s suitcase is empty
   The suitcase of the boy who arrived from Kansas is empty

Particle Movement: They called up John
   They called John up

   (English stress requirements have the VP stress in final position whenever possible; when not, then the verb is stressed. For example, We saw the body: but He didn’t see it. In cases where the direct object of a compound verb + particle is a non-contrastively stressed pronoun (and some other cases as well), the particle, part of the verb, is moved to the VP-final position.)

Consonant Cluster Simplification: fist fight/fis fight
   tent city/ten city
   a kept promise/a kep promise

   (English, as well as other languages, disfavors final consonant clusters and favors CVCV sequences.)

[t]-Assibilation: Let’s/Les :
   That’s/Thas :
   belts/bels [bels]
   tastes/tas : [teys :]
   tests/tes :

   (For some reasons, unknown to me, English [t] has a tendency to become [s] when preceding [s], and more likely when preceded by an [s] than when preceded by another consonant or a vowel.)

Contraction: He is sick today
   He’s sick today

   (Labov 1969, has shown convincingly that the optionality of contraction rests on at least the phonological form of the subject noun phrase; contraction occurs far more often when the preceding noun ends with a vowel than with a consonant. However, contraction depends also on whether or not there is stress on the verb is (or are, am, etc.) thus suggesting that contraction is influenced by additional English phonotactics.)
E. Rules to Topicalize

These are rules which create the topic of a sentence by fronting the relevant constituent. For example,

Passive: John saw Mary
        Mary was seen by John

Clefting: It was John who we all considered to be the scapegoat

Pseudo-Clefting: Who was wanted for dinner was the Pope

Adverb Preposing: I went into the park at daybreak
        At daybreak I went into the park

WH-Fronting: You saw the kittens where?
        Where did you see the kittens? (Subject-Verb Inversion has also applied)

The relative obligation of these rules rests primarily with the discourse in which the sentence occurs; i.e. what the discourse is about.

F. Rules to Increase Variety

These are rules which appear to fit the original conception of optional rules: rules which account for stylistic variation within the language. For example,

Dative Movement: I gave a book to the boys
        I gave the boys a book

Slifting: I think that Martin has a cold
        Martin has a cold, I think

Adverb Positioning: Certainly we shall leave on time
        We shall certainly leave on time
        We shall leave on time, certainly

Extraposition: That Lisa has laryngitis is obvious
        It is obvious that Lisa has laryngitis

Negative Hopping: I believe that John hasn't left yet
        I don't believe that John has left yet
This taxonomy, presented quite tentatively, indicates some six different types of functions that rules of grammar fulfill. I don't for a moment think that this taxonomy will stand the test of careful examination; some of the rules presented (e.g. Negative Hopping) probably don't exist in English; some of the rules (e.g. Contraction) probably don't function in the class they have been placed in; some of the rules (e.g. Reflexivization) are perhaps really obligatory even though one can occasionally find them not being applied; and some rules of English grammar (e.g. Complementizer Placement, Negative Concord, Adjective Preposing, Nominalization Formation) I have omitted from discussion for a variety of reasons. However, for whatever faults this tentative taxonomy may have, I suggest its consideration as a way to examine grammar rules in a much broader context than heretofore assumed. Some of the reasons why a rule will take on more optionality at one time than at another may involve more than the immediately linguistic environment. But insofar as these various 'forces' for rule application interact, they all must be appreciated before any coherent explanation for rule variability can be presented.

With this view of the notion of optional rule in mind, let us now look at one highly formalized type of optional rule: inherently variable rules.

Variable Rules

Labov et al. (1968) looked at a variety of optional rules of English grammar in their study of the dialect of English vernacular spoken by Harlem teenagers and concluded that the application of optional rules is logically governed by two factors. First is an input variable which sets the over-all frequency with which the rule is selected. Second, there are variable constraints which differentiate the frequencies with which the rule applies according to the syntactic and phonological features of the environments... (Footnote 17). And third, of course, there are extra-linguistic factors such as age, sex, ethnic group, social class, and contextual style... (Labov 1969:733)

Given an optional rule, there is one variable associated with the rule which determines its overall frequency of application: the number of times it is applied compared to the number of times it was applicable; and second, there are variable constraints which determine, given that the rule is going to be applied, the relative frequency of its application in one linguistic environment as opposed to a second, third, fourth, etc. (Whether these two factors can be shown to be independent of one another is an interesting open question which will not be explored here.)
Optional rules exhibiting systematic variation of application correlating with linguistic environments were labeled 'inherently variable rules'.

This position, originated by Labov and adopted by others, entails that variable constraints are a part of the rules of grammar.

It is evident that rules 1-17 [a set of rules, some variable-BF] are a part of the speaker's knowledge of the language, and if some of these rules are cast in a different form than traditional categorical rules, then we must clearly revise our notions of what it means to know a language. (Labov 1969: 759)

However, it is not the actual absolute frequencies that underlie the thrust of Labov's variable rules. Bickerton has suggested that Labov does not envisage that the behaviour of a member of a rule-sharing group will necessarily be isomorphic with that of all of or even any of the other members, though it is true that he does not expect it to vary much: 'It is unlikely that it will be important for us to know that the copula is deleted 82% of the time by speaker A and 79% of the time by speaker B' (Labov 1969:740). However, in order that the average for his group should remain constant, the variation of the individual must be confined within a relatively narrow range. (Bickerton 1971:461)

But Bickerton has misconstrued the nature of Labov's claim. For on the line below the quote, Labov goes on to argue that 'the structures we are examining are not a series of numbers, but rather a series of relationships--between the environment and the /z/ [the representation of the copula] and between one environmental contrast and another' (Labov 1969:740). Inherent variability is relative to alternative linguistic constraints and is not of an absolute nature. The claim of a variable rule is not that rule R produces X in linguistic environment Y 77% of the time on the average for an individual of the speech community while it produces X in environment Z 23% on the average, but rather that speakers using this grammar will characteristically apply R to produce X in environment Y more often than in environment Z. For example, if Y and Z are 68% and 32% for speaker A and 56% and 44% for speaker B, the rule R is accurately describing the R-facts of these two speakers.

A number of linguists have raised objections to the concept of variable rules (Cf. Bickerton 1971; Bailey 1970; Butters 1971a, 1971b; Kiparsky 1971). They question the concept, mainly along three lines. First, if (as is usually assumed) the child acquires language from greatly impoverished language data, then how is the child to determine
the relative weighting on the variable constraints and thereby acquire
the grammar of the language dialect he is speaking? Second, once ac-
cquired, how is the speaker to remember the ‘staggering number’ of the
relative weightings on the rules, not to mention the computation of their
cross products, which determine the actual probability of rule applica-
tion with respect to the entire linguistic environment? Third, if the
speakers of a dialect are actually going to reflect the relative weightings,
how are they going to keep track of their own relative frequency of rule
application while all the while thinking about what they are saying?

These objections, for the most part, don’t strike me as crushing,
though they raise some interesting questions. The acquisition issue
needs further clarification before any comment can be made, since we
don’t know at what point a child in learning a language actually acquires
the systematic variation found in the studies of Labov and others. The
problem of keeping the relative weightings and their cross products in
mind doesn’t appear to be any more staggering than what linguists have
proposed in the detailed specifications of, say, the stress rules of Eng-
ish (Cf. Chomsky and Halle 1968). We have so little idea of what it is
exactly that the mind can keep track of, learn, deal with, etc., that
this sort of objection has little force at present. The third objection
has been answered in a brief discussion by Anshen (1972) in which he
points out that a speaker using variable rules does not have to keep
track of what choices he has taken; only that each option has some prob-
ability of selection different from the others, much as the throwing of
dice will provide different probabilities for the occurrence of 2, 3, 4,
5, etc.

I think there is a much stronger argument against the concept of
variable rules, namely, that variable rules describe the observable lan-
guage behavior but do not explain this behavior in any sense of the word
‘explain’. I think variable rules are extremely valuable tools in linguis-
tic analysis and lead us to ask the deeper questions: why are optional
rules applied more often in one environment than another and why is the
over-all frequency of one optional rule greater than another. To stand
pat with a linguistic analysis having variable constraints on optional
rules is to avoid asking these questions. My objection, then, is not so
much with rules having relative weightings assigned to them (though I
must admit I find this unaesthetic, if it is possible to have this feeling
about linguistic rules) but that to stop with variable rules is to be satis-
fied with data collection and not explanation, much the same as demo-
graphic information describes but doesn’t provide an explanation.

Two Examples

In an attempt to indicate how we might push further I will discuss two
eamples of optional rules from English. The first concerns Labov’s
rules of Contraction and Copula Deletion, simplified below to deal with *is* only.

Contraction

\[ \varepsilon \rightarrow \emptyset / \begin{array}{c}
\text{[Pro]} \\
V
\end{array} \begin{array}{c}
\text{[v]} \\
\text{gn}
\end{array} \begin{array}{c}
\text{[yNP]} \\
\text{Vb}
\end{array} \]

\[ \text{Vb} = \text{verb} \]

\[ \text{gn} = \text{gonna} \]

Deletion

\[ [s] \rightarrow \emptyset / \begin{array}{c}
\text{[v]} \\
\text{yPro}
\end{array} \begin{array}{c}
\text{[yNP]} \\
\text{[v]} \\
\text{[v]} \\
\text{gn}
\end{array} \begin{array}{c}
\text{Vb}
\end{array} \]

Labov discusses how Contraction favors a pronoun subject (which ends in a vowel—thus the categorical '*' for the Pro subject) and other subjects that end in a vowel, and how Deletion favors subjects ending in a consonant and disfavors subject pronouns (which end in a vowel). He argues that the former rule acts to reduce CVVC to CVC sequences, while the latter acts to reduce CVCC to CVC sequences, and concludes that this favoritism rests with the phonotactics of English.

I think it will be worthwhile to look further at the phonological environment in which Contraction and Deletion occur, rather than stop at the point of the above rules which characterize the probability of contraction and deletion in terms of syntactic categories. What would be most desirable would be to find a principle or set of principles which would predict the weightings of \(<, \beta, \gamma, \ldots,\) on these and other rules. What would be 'learned' by the speaker would be these principles which would affect a wide range of rules. Changes in the relative inherent variability of an optional rule would be explained by the change in the relative relevance of a principle, e.g. the force towards maintaining a CVCV pattern. And, of course, a test of the validity of such an analysis would be that the change in the relevance of a principle, which is accounting for the change in one optional rule, should infiltrate other optional rules as well as the appropriate changes observed. Although I do not have the original Labov data, nor other data on which to draw any conclusions, I wish to argue that the following additional phonological facts may be relevant in an explanation of Contraction and Copula Deletion:

1. Pronouns in subject position normally receive less stress than an NP in the same position. For example, in
   
   He is here today
   Lee is here today
   The man is here today
   The man and Lee are more heavily stressed than he.
(2) Gonna begins with a consonant and the likelihood of a verb beginning with a consonant is greater than for an NP, either because of the determiner a or nouns without determiners which have initial vowels.

(3) The stress on a NP following is normally on the noun, the determiner being normally unstressed. The stress on gonna, which precedes a verb, is lower than the following verb and in some cases is unstressed, the vowel reducing completely to [ə]. The verb following is is stressed.

How these phonological facts interact with those facts discussed by Labov remains an open question. The force of my argument is that we would do well to look at these and other facts in detail before we adopt inherent variability with variable constraints.

The second example involves the rule of Dative Movement (to dative only) which converts sentences like John gave the book to Mary into sentences like John gave Mary the book. Based on native intuitions of a few English speakers and a limited amount of observation of the speech of university students, I predict that an analysis of the application of this optional rule will show at least the following variable constraints:
(favorable examples given first)

C-1 The rule favors direct objects that are not pronouns (We gave John the book—We gave John it)

C-2 The rule favors pronominal direct objects that are heavily stressed (Sheila sent John THEM—Sheila sent John them)

C-3 The rule favors indirect objects without a drop in intonation (I offered the big boys the books—I offered the boys who were big the books)

C-4 The rule prefers verbs that are monosyllabic (e.g. send, give, loan) or bisyllabic and initially stressed (e.g. offer, promise, whisper) to other verbs (He told me the message—He communicated me the message Harry sent him the car—Harry transported him the car)

C-5 The rule favors non- phrasal verb (We sent the people the letters—We sent out the people the letters)
C-6 The rule favors direct and indirect objects with determiners
(Douglas fed the dogs some food—Douglas fed dogs food)

One alternative would be to create a variable rule with the appropriate
weightings, determined by the actual observed language use. A second
alternative would be to predict this behavior from some general prin-
ciple(s) of English verb phrase phonotactics.

As a first cut, I suggest the following phonotactic principles will be
relevant:

PT 1 Try to place the intonation center on the verb phrase final
position

PT 2 Avoid sequences of stressed or unstressed constituents.

These two phonotactic principles predict that a sentence like We loaned
him books is more likely than We loaned John books because the latter
sentence violates PT-2; that the The teacher handed them out some of
the papers is more likely than The teacher handed out them some of
the papers since the latter also violates PT-2; that We'll feed the boys
them is more likely than We'll feed the boys it since the former with
them can carry more stress than it, thereby more closely abiding by
PT-1. Note that C-3 above is not covered (at least not obviously) by
PT-1 or PT-2. I leave that predicted 'frequency of application' as
another unanswered question.

Conclusion

This discussion on optional rules has had two emphases. First, I
have suggested that the notion of optional rule is both broader than usu-
ally construed and that many optional rules are applied by reason of
their function in the language (e.g. clarification of semantic detail, re-
duction of redundancy, stylistic variation, conformance to syntagtactics
and phonotactics). Second, I have argued that the notion of an inherently
variable rule, while an important step in the analysis of the linguistic
competence of a speaker in a speech community, is only a first step,
and that one cannot be satisfied with variable rules until all other
avenues of explanation have been carefully explored. The purpose of
the paper has been to provoke further examination of the broader ques-
tions such as: Why should a rule be in a grammar? And if it is, why
is it applied? What functions does a rule have in the overall linguistic
competence of the speaker? If, after such investigation, we find that
there is nothing more there than we have today, we shall have at least
looked.
REFERENCES

Anshen, Frank. 1972. Bickerton, Labov, and inherent variability. (Ditto.)


What should be the relationship between sociolinguistics and grammars? One of the purposes of sociolinguistics is surely, as Fasold (1970:562) puts it, to provide 'revealing insights into language and society'. Unfortunately, the social insights it has so far offered are mostly along the lines of the following:

Kessler 1969 has been able to show that four social classes are discriminated linguistically on the basis of the environmental conditions under which the plural suffix can be deleted... This analysis discriminates in a remarkable way the same social groups as does an independent sociological analysis based on occupation, house type and dwelling area. (Fasold 1970:557-8)

Indeed, the type of sociolinguistic study pioneered by Labov (1966) and subsequently developed by Wolfram (1969) and others has many points of excellence, but also two very grave limitations. First, it tells us things we already knew about society, and while it is useful to have things confirmed, labelled and quantified, such processes hardly qualify as insights. Second, it represents a dead end; having done it once, one can only, like the Young Lady from Spain, do it again, and again, and again, and again, on another class, in another country. It does not light the way to ever-deepening studies, as some lines of research do. Nor has there been any stampede of sociologists into the linguistic corral, looking to see what there is for them there, as there has of educationists or philosophers, for example; it looks like being a long time before we can add a 'vice versa' tag to Hymes' recent (1967) title--'Why Linguistics Needs the Sociologist'.
The story is different when we look at the language side. The other half of Fasold's prescription has fully justified itself; by studying, in Labov's (1970) words, 'language in its social context', we have been made forcibly aware of both the extent and the patternedness of linguistic variation, and of the consequent necessity that grammars, even if they aim at no more than observational adequacy, must be capable of generating a variable output. Indeed, Labov's more recent work (e.g. MSa, MSb) has moved far from the quantifying, social-survey-oriented pattern of his earlier studies, and concentrates increasingly on the purely linguistic consequences of social variation. My own position, at present—and I would like heavily to underline both phrases—is that the most useful purpose of sociolinguistic enquiry is to provide the linguistic data that will serve as an input to polylectal grammars.

In taking such a position, I am likely to fall foul of at least two camps. Pure linguists will object to the pollution of their idealised grammars by masses of performance fallout, while 'socio' sociolinguists will reject the straightjacketing and returning to formal linguistic incarceration of a lusty if immature interdiscipline. I do not propose specifically to defend myself against either here, partly because most of both sets of argument will be familiar already to most of us, but mainly because argument is less powerful than demonstration. I shall therefore try to demonstrate that polylectal grammars can be written, and that by writing them we can obtain insights into language that could not be obtained in other ways. I cannot, unfortunately, claim that such grammars will also provide social insights; I can, I think, claim that they would be more useful to the sociologist than grammars of existing types, and at the very least, they could hardly be less so.

However, those who would write polylectal grammars are immediately faced with a major difficulty. Since extant grammars are all monolectal, they can have only the vaguest notions as to what are the properties and structure of a polylectal grammar, and which of a number of possible forms it might best take. What follows should therefore be regarded as a rough first sketch, maybe no more than a basis for subsequent revision or amendment. One could, like some linguists (e.g. Dixon 1965, Ellis 1966), spend forever discussing where and how to start; it seems more useful simply to get started.

People who have envisaged grammars that would generate more than one variety of a language (e.g. Klima 1964) have often done so in terms of a unitary core grammar with sets of additional rules (alternatives) at the end. This accords with the generative supposition that dialects of the same language will differ only in low-level rules (cf. Chomsky 1964); one which can easily, if circularly, be reinforced by appropriate definitions of 'language' and 'dialect'. Unfortunately, no one has yet succeeded in deciding whether the varieties of language we shall be discussing here are 'dialects of English' or 'separate languages' (for some
discussion of this perhaps vacuous question see Taylor 1963, Brown 1968). Since, as I hope to show, some of these varieties differ from English at a relatively deep level, it is hard to see how a ‘core-and-appendage’ grammar could generate all of them.

One might next consider a model based on quantified data, expressing consistent relations between variable elements, along the lines of Labov 1969. I have discussed this model extensively elsewhere (Bickerton 1971, MS) so I shall merely repeat that grammars which contain Labovian variable rules must be based on the behaviour of groups, and that group behaviour can be very misleading with regard to the contents of individual rule systems. I shall give just one example. Loflin (1970) claims that speakers of Black English have no have in their grammar, and that the tokens of have occasionally appearing in their speech are performance features due to Standard English interference. Labov (Labov et al. 1968, I, 223) makes an exactly contrary decision: have is in BE grammar, but is deleted, in the vast majority of cases, by phonological rules. However, in Labov’s data, occurrences of have seem to be outnumbered by informants, i.e. there must be speakers who, at least while being monitored, never produced have. If a speaker, given sufficient opportunity to produce a feature, fails to produce it, it seems not unreasonable to suppose that it forms no part of his grammar. Assuming this would enable us to resolve the contradiction very simply: some BE speakers have have, and others do not.

But the belief that, even if nations don’t have homogeneous grammars, groups must have them leads both Loflin and Labov to assume unquestioningly that either all BE speakers have a particular feature, or else none do. This assumption is perhaps supported in the present case by a feeling that have is a feature too central to the grammar to permit of intragroup variation. Yet in Guyana, with a population less than that of the District of Columbia located in an area smaller than that of Rhode Island, we can find much grosser variations whose fault-lines cut across, as well as between, primary groups.

The contrary assumption—that BE speakers include both haves and have-nots—would, however, accord with what is maintained by Bailey (1969-70), i.e. that synchronic variation reflects diachronic change, and in this specific instance, that a rule-change affecting have has reached some, but not all BE speakers. Such an assumption would lead us to a model which in accounting for synchronic data would rehearse the phylogeny of the varieties under consideration—and if to any generativist this should sound a funny kind of grammar, he should reflect that it would merely be doing for morphosyntax what Chomsky and Halle (1968) have already done for the phonological component.

A model of this kind could be based on implicational scaling of the type pioneered by DeCamp (1971) and further developed in Bickerton MS. The limitations of scaling must, however, be clearly realised.
Firstly, scaling of heterogeneous and unrelated items can be singularly unrevealing, as Fasold (1970) has rightly pointed out. Secondly, it is far from foolproof, as I shall later show. Thirdly, even if it were foolproof, it cannot of itself constitute a grammar. However, implicative scaling enables one to set up hypotheses about hierarchies of precedence among features (e.g. which of two features is the older, which is cause and which effect, etc.) and thus to create a provisional order among apparently conflicting data which may serve as a basis for further investigating and the eventual writing of formal rules.

One may legitimately ask whether a bidimensional scale is adequate to convey all the complexities of a dialect continuum. It may well be that, for large and complex communities, where rule-changes generalise out from a number of distinct foci, and cross one another in series of overlapping waves, a multidimensional scale might be required. But a bidimensional scale approximately fits the Guyanese data, especially if one distinguishes constrained and contingent implications. For it seems likely that, among the rule-changes that give rise to implicative phenomena, some are contingent, i.e. happen by historical accident to have diffused at roughly consistent rates in roughly consistent order, while others are constrained in that adoption of a rule $R_j$ is impossible before a rule $R_i$ has been adopted, or conversely, that adoption of $R_i$ automatically and necessarily enforces the subsequent adoption of $R_j$. Contingent implications may (within narrow limits) be randomly ordered in the grammar; for instance, in the Guyanese pronominal system, adoption of feminine-subjective $shi$ and neuter-objective $it$ precede all other changes, but occur in different order for different speakers, without, apparently, causing any difference in the ordering or form of subsequent rule-changes. If this is so, we can legitimately ignore many apparent breaches of strict implicational series. In any case, a Newtonian idealisation strategy which produces first an abstract, anomaly-free model which can be subsequently adjusted to the flux of empirical data is clearly advantageous in the early stages of any enquiry. The bidimensional implicational scale is simply our ‘ocean of uniform depth and width’.

But it still remains for us to translate the information contained in an implicational scale (such as Table 1, on page 27) into a series of grammatical rules. Such rules should be capable of generating all varieties of language in communal use in any speech community that is not unequivocally bi- or multilingual, i.e. where there is a speech-continuum with no clearly-defined break. A process of this kind is sometimes conceived of as somehow sorting the continuum into strata (the ‘coexistent systems’ approach of Tsuzaki 1970, based on that of Fries and Pike 1949) and then showing transitions between strata. For instance, Labov (Labov et al. 1968, I, 11) observes:
One would assume that the identification of these strata must be accompanied by switching rules which state when, where and how the speaker moves from one system to another.

The point is well taken, but misleadingly put. Confronted by an unbroken implicational scale, one can only ask, which are the systems and which the movements between systems? An implicational scale consists merely of ranked isolects, each differing from its immediate neighbours by only a single rule-change. There is nothing, qualitatively or quantitatively, to distinguish any isolect qua isolect from any other. It is true that some isolects will be more heavily populated than others, i.e. there will be more speakers who share the particular selection of rules which yields as output the particular array of surface features shown on a given horizontal on the scale, but no one has yet claimed that what defines a system is a given number of speakers. It may be that some isolects have properties which render them more stable than others, but we obviously cannot yet know what such properties might be. A grammar based on implicational scaling cannot therefore generate discrete strata with linking rules, but must generate theoretically-equal isolects sequentially, and presumably in the order in which they first occurred in time.

It follows that a polylectal grammar will contain no 'switching rules' as such. It will have a series of rules similar in form to existing types of generative rule, but some of which will in effect be rewritings and re-rewritings of 'earlier' rules; in addition, it will contain a 'rule-shift component' which will specify the selection from the overall series required to generate each successive isolect. However, in a grammar of this kind the precise form of the base rules becomes rather more critical than it does in traditional grammars. Ideally, one would like an invariant base of PS rules, so that all the necessary rule-changes could be specified in terms of alternative spelling rules or transformations. One way to achieve this result would be to expand Predicate and NP nodes by using semantic distinctive features, more or less in the way that Jakobson-Halle phonology uses phonological distinctive features. Context-sensitive rules would then determine the combinations in which various bundles of SDFs could occur, while spelling rules would assign specific shapes to the various bundles. The invariant section of the base might then look something like the following:

(1) PS1. $S \rightarrow P + NP$
(2) PS2. $NP \rightarrow NP + (NP) + (S)$
(3) PS3. $P \rightarrow SDF_{a...n}$

NP)
(4) PS4. SDF_{a...n} → + verb(al), + loc(ative)
+ stat(ive), + comp(letive),
+ cont(inuative), + pro(verb),
+ att(ributive), + past, etc.

Such a sketch leaves much unsaid. There would have, for instance, to be semantic redundancy rules, just as there are phonological ones. In all probability, the semantic features specified here are not the most appropriate — (+ past), for instance, as a semantic feature is likely to cause confusion with its grammatical equivalent — but a weakness at this level does not necessarily invalidate the process as a whole.

We must next select a particular area of the grammar for description, since the whole clearly lies beyond the scope of this paper. I have chosen the copula, for a variety of reasons. First, it is an area of Guyanese grammar already treated fully (if not, as we shall see, quite adequately) in Bickerton MS. Second, it is a critical area in the continuum, involving changes at relatively deep levels, and thus constituting a harder test of a polylectal grammar than would, say, the development of case and gender in the pronominal system. Third, the copula in general is an area on which a great deal of useful work has been done recently, in particular the series of studies edited by Verhaar and papers by Bach (1967), Ross (1967), and Darden (1969). Fourth, the interest of the specific problem involved (the precise genesis of so-called 'zero copula') is not confined to Guyana, or even to the Caribbean, since it has been claimed by (among others) Stewart (1969:242-4) that BE has zero copula and that this feature is due specifically to its creole ancestry.

Although (as we shall see) Stewart's overall analysis is insightful, one must question the applicability of the term 'zero copula' to the Guyanese situation, since merely to use it implies that the language (or dialect) under consideration must have 'copula' as a category. But as Graham (1967:14) points out, the category 'copula', embracing as it does that whole ragbag of uses distinguished by Bach (1967:477), is probably limited to Indo-European languages, while the semantic area which the category covers is dissected by other language families in a variety of different ways. Thus to use the category in the present context would be implicitly to commit Guyanese Creole to the Indo-European camp, a move which for many reasons it seems unwise to make. Fortunately the problem can be avoided, or rather shown to be improperly phrased, if we can show that 'copula' has no place in deep structure anyway, and there is quite independent motivation for taking such a course. Following Darden (1969), we can accept Ross's claim that _be_ is a main verb in English without necessarily accepting that _be_ or any other copulative or quasi-copulative verb is in deep structure, since if 'deep structure is semantic, and if the copula has no semantic function, then the copula cannot exist at the level of deep structure' (Darden 1969:34). Instead,
we would insert, under a higher predicate node, a bundle of features (probably no more than (+verb), (+pro)) to be spelt be at subsequent lexical insertion; indeed, the bundles of features representing all modals and auxiliaries would be inserted at this (or if necessary, at a higher) node. The predicate in the next lowest S could then be nominal, adjectival, or verbal, as illustrated in rule (5) and Figure 1:

(5) RR1. \[ P_H \rightarrow (+\text{verb}) \]
\[ (+\text{pro}) / P_L (-\text{pro}) \]
(Where \( P_H \) is a higher, and \( P_L \) a lower, predicate)

FIGURE 1. Deep structure of John is angry, John is a mammal, John is leaving.

We can now examine in detail the process by which Guyanese basilectal quasi-copulative forms give way to the single English all-purpose copula. But first some corrections must be made to the analysis in Bickerton MS. There, for instance, bin was treated as a first-replacement substitute for quasi-copulative particles which took neither tense nor aspect. In fact all these are (+stat) and therefore the two following general rules apply to them also:

(6) RR2. \( (+\text{verb}) \rightarrow (+\text{verb}) (+\text{verb}) \)
\[ (+\text{pro}) (+\text{pro}) / P_L (+\text{stat}) (-\text{cont}) \]

(7) RR3. \( (+\text{pro}) \rightarrow \emptyset / P_L \{ (+\text{verb}) \} \)
\[ (+\text{stat}) (-\text{past}) \]
\[ (+\text{verb}) (-\text{stat}) (+\text{past}) \]
The first of these rules blocks cooccurrence of (+ stat) verbs with markers of continuative aspect, while the second yields zero aspect (the stem form of a (-pro), (+verb) item) before nonstative verbs with past reference and stative verbs with nonpast reference; together they give rise to patterns such as the following:

(8) a. awi bin gat no disturbans \( (8/6/22) \) ‘we had no disturbances’
    b. *awi a gat no disturbans
    c. di kuliman bin prapa fraikn di blakman \( (27/25/2) \) ‘the coolies (Indians) were very frightened of the blackmen (Negroes)’
    d. iif yu na plant ar yu plant, \( 8 \) bornam want i rent \( (137/183/27) \) ‘whether you plant or not, Burnham wants his rent’
    e. mi na bin wan sen mesij hoom \( (121/155/15) \) ‘I didn’t want to send a message home’
    f. bot mi faal wan taim a trensh \( (198/262/1) \) ‘but once I fell in a canal’
    g. na haumotsh a moni dem a tshaaj fu di ye? \( (186/240/7) \) ‘now how much money do they charge per year’?

The faulty analysis arose because copula-type verbs express mostly timeless relations, and are therefore usually nonpast; since they are also stative, they are seldom marked for aspect. Thus \( \text{bin} \) seldom occurs with them, and the few tokens that showed up in the sample analysed could be scaled, without violating implicational series, on the assumption that they were first replacements (cf. Bickerton MS, Table 2). The moral, however, is not to abandon implicational scaling, merely to be more sceptical of its results, particularly where only a few type-tokens are involved.

The treatment of ‘predicate adjectives’ in the previous paper is also open to question. Since such items come under rules (6) and (7) just like verbs do, and since they behave like verbs in a variety of other ways:

(9) a. hau i taal! ‘how tall he is’!
    b. hau i wok! ‘how he works’!
    c. a leezi shi leezi ‘lazy, that’s what she is’.
    d. a tiif shi tiif ‘steal, that’s what she does’.

it would seem advisable to treat them all as verbs, and indeed many linguists would treat English adjectives as deep-structure predicates (though Ross 1966 suggests a contrary view). Furthermore, it is doubtful whether basilectal grammar contains a productive adjective transformation—the few prenominal adjectives that do occur may well be acquired as fixed idioms. This leaves, as a counterargument, only the fact that some of these items will take continuative \( \text{a} \), an apparent
breach of rule (6). However, one should beware of supposing that words and semantic features are indissolubly linked; in English, both to be tired and to be female are stative expressions, but one is open to a processual interpretation while the other is not: she is getting tired versus *she is getting female. In just the same way, some, though not all, Guyanese items marked (+verb), (+stat), (+att) can undergo conversion to (−stat):

(10) a. yu redi fi sliip 'you are ready to sleep'.
    b. yu a redi fi sliip (28/29/31) 'you GET ready to sleep'.

On this analysis, the last possible environment for 'zero copula' disappears from the basilect.

We are then left with the following four basilectal categories:

I. de, a locative verb that can be used existentially:

(12) a. noting hapi doz de eni mor (243/323/3) 'there's nothing enjoyable (left) any more'.
    b. wen di imij lak hau di imij de de (248/327/10) 'when the image is just as it is there'.
    c. i de a stshootvil (125/169/31) 'he is (living) in Stuartville'.
    d. dem de aal oova di plees a shuut wail (13/14/15) 'they were all over the place shooting wildly'.

II. a (cognate with da, de in some other creoles) which I shall assume, pace Stewart (loc. cit.), is identical with the homophonous continuative marker, and which occurs in NP environments:

(13) a. wa rang wid yu a wan dringk yu gat de (197/255/25) 'what's the matter with you, have you got a drink there'?
    b. mi a kyapn, yu na baas mi (243/322/20) 'I'm captain, you're not bossing me around'.
    c. dis a wan lil ting (121/154/22) 'it's nothing, really'.
    d. wel a so awi yuus tu du lang taim (9/9/20) 'well that's how we used to do it in the old days'.

III. A class of attributive verbs, similar in phonological shape to English adjectives:

(14) a. wa hapn i veks (238/305/16) 'the trouble is, he's annoyed'.
    b. da taim ting bin haad, na laik nau (28/29/4) 'things were hard then, not like nowadays'.
    c. laik di son doz hat he baad (160/198/15) 'the sun gets very hot here'.
IV. The set of aspect markers (bin, a, bina, with doz/das in the mesolect) which occur also with non-copulative verbs as below:

(15) a. de na no wa dem a du (248/327/26) ‘they don’t know what they’re doing’.
   b. i bin lash a kwaata an mi gu we bak a haus (61/72/33) ‘he drank up a quarter (bottle of rum) and then I went back home’.
   c. wan blakman an i waif bina liv abak (9/10/26) ‘a black man and his wife used to live inland’.
   d. yu doz go in klaas an waz (=wa doz) hapŋ (41/192/22) ‘you go into class and what happens’?

In addition to the rules already given, we shall need the following spelling rules to generate these forms:

(16) SR1. (+verb)
    (-pro) → de
    (+loc)
    (+stat)

(17) SR2. (+verb)
    (-pro) → hat, haad, veks, etc.
    (+att)
    (+stat)

(18) SR3. (+verb)
    (+pro) → a
    (+cont)

(19) SR4. (+verb)
    (+pro) → bin
    (+comp)

We shall also need a further realization rule for equative-a placement:

(20) RR4. (+verb) (+verb)
    (+pro) → (+pro) / PL (-verb)
    (+cont)

The rules so far given will generate the basilect within the area under study. Let us now look at the changes (summarised in Table 1) that successively occur to these rules as social pressure draws the creole speaker closer to the (Standard English) superstrate.

In my earlier treatment I described this speaker as ‘noting that some of the items in (his) repertoire are non-standard, and... dropping them,
Table 1. Introduction of English copula.

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<td>125, 192, 236</td>
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</tr>
</tbody>
</table>

Key:  
Col. 1 = continuative verbal environments  
Col. 2 = locative and existential environments  
Col. 3 = 'predicate adjective' present environments  
Col. 4 = cleft sentence environments  
Col. 5 = 'predicate adjective' past environments  
Col. 6 = noun phrase environments  
Col. 7 = impersonal expressions  
0 = continuative a + V  
1 = Ving without aux. in Col. 1; de in Col. 2; V(+att) in Col. 3; equative a in Cols. 4, 6, and 7; bin in Col. 5  
2 = ∅  
3 = iz/waz (waz only in Col. 5)  
95.5% scalable

Without in every case replacing them' (MS, 26). Since this might just be taken to mean that at least some speakers, in at least some environments, replace basilectal items by zero on all possible occasions, the remark needs some amplification. We can roughly divide the relevant environments into three types. In the first type (e.g. cleft and impersonal sentences where the copula-type is in 'exposed' position) immediate replacement of a by iz is mandatory; a sentence such as (21a) may be rephrased as (21b) but never as (21c):

(21) a. a da mi tel dis bai (148/190/24) 'that's what I told this boy'.  
b. iz da mi tel dis bai.  
c. *da mi tel dis bai.

This rule has interesting consequences. One of the main planks in Labov's argument that BE copula is phonologically deleted after contraction is the (in itself perfectly correct) assertion that BE cannot delete where SE cannot contract, i.e. in 'exposed' positions, which are clause-final in both BE and SE, but clause-initial in GC (Labov 1969: 722-7). The conclusion is drawn that, because the result is the same,
the cause must be the same, but this follows neither in logic nor in fact. SE be cannot contract in such environments because it carries too much stress to permit of vowel-reduction, but preclausal GC a and its successor iz are already minimally stressed, and their deletion is blocked for purely syntactic reasons, since it would lead either to sentences that are ill-formed, or sentences that are well-formed but non-synonymous:

(22) a. a neks gyal klaim op pan di kau pen, iz hu kau pen, an a kaal di draiva pan awi (119/150/29) 'another girl climbed on top of the cow-pen, whose cow-pen it was, and kept shouting to the foreman to come for us'.

b. *a neks gyal klaim op pan di kau pen, hu kau pen, an a kaal di draiva pan awi.

c. a hau dem pak tri hiip (186/240/16) 'how is it (= how did it come about) that they made three stacks'?

d. hau dem pak tri hiip 'how (= in what manner) did they make three stacks'?

Thus BE non-deletable 'exposed' copula could as easily come about through retention of a creole syntactic rule as through acquisition of an English phonological one.

In the second type of environment (e.g. before noun phrases and locatives), zero may appear, but not before the introduction of iz/waz, i.e. the two things appear simultaneously, or else zero is the later to appear. In the third type of environment (e.g. before 'predicate adjectives' or verbs marked (+cont)) it is perhaps misleading to talk about zero at all, since, from a basilectal or early-mesolectal viewpoint, there is nothing for zero to stand 'instead of'. It is, of course, highly likely that at a later stage the speaker 'reinterprets' his grammar (without necessarily changing its contents), thus perceiving new categories, and hence vacuums where these categories remain unfilled; but this is an altogether different matter, and it seems anyway highly unlikely that such reinterpretation could take place until iz/waz acquisition was far advanced in other environments.

The processes involved are complex, but will become clearer if we take the changes singly and in sequence. The first change, depending on speaker, is either a - -in or a - iz/ NP. a - -in can almost be seen in the act of happening in an informal interview between 41 (putting out what she thinks is Berbician creole, but what is in fact—probably because of role-pressure—several isolects 'above' her relaxed Georgetown bottom-gear) and 137, who, though talking freely, is probably in the upper level of a narrow total range. She is, in fact, only just undergoing a/das splitting for iteratives, as can be seen from:
(23) a. aagas rais das bos (137/183/4) ‘the rice crop ripens in August’.
   b. evri de mi a ron a rais fiil (137/182/29) ‘every day I hurry to the ricefield’.

Her overall distribution of aspect markers is: das 3, a 9, don 2, bina 7, bin 6, with total absence of have, be, dummy do, etc.; she is thus still pretty close to the basilect. But when 41 utters (24a), she answers not, as one would expect, with (24b) but with (24c):

(24) a. we yu bin livin bifo yu kom he (41/183/7) ‘where were you living before you came here’?
   b. mi (bin) a liv a sevntiwan ‘I was living in Seventy-one village’.
   c. mi livin a sevntiwan (137/183/7).

Of course, she is simply ‘echoing’ 41 (cf. Bickerton and Escalante 1970:266); but in order to echo a form, one must know it, if only passively.

For this change, we need another spelling rule, a realization rule, and a transformation, as follows:

(25) SR5. (−verb)
       (+pro)  −→  −in
       (+cont)

(26) RR5. (+verb) (−verb)
       (+pro)  −→  (+pro) / P_L (+verb)
       (+cont)  (+cont)

(27) T1. (−verb) (−verb)
       (+pro)  +  (+verb)  −→  (+verb)  +  (+pro)
       (+cont)  (+cont)

Many (though not all) speakers retain a in nominal environments long after they lose it in verbal ones, 98, for instance:

(28) a. dis esteet blak piipl an iistinjn piipl de livin laik tu broda (98/119/2) ‘Negroes and Indians on this estate were living like two brothers’.
   b. wan a dem a di man bin ga di bam (98/119/21) ‘one of them was the man who had the bomb’.

For others, iz/waz appears in nominal environments at about the same time or maybe earlier. But it is apparently true for all speakers
without exception that both changes must precede the appearance of *iz*/ waz in ‘predicate adjective’ environments. Even a speaker such as 99, who commands a higher range (his formal output appears in Table 1 as 125), makes this distinction in informal speech:

(29) a. if *iz* midnait i week i gon bigin kos (99/116/14) ‘if it’s midnight when he wakes up he’ll start swearing’.
   b. de bigin revop an golang so piipl doz se dem *iz* dringkman (99/117/3) ‘they start to make trouble and carry on so that people say they’re drunks’.
   c. bot leetli piipl se i baad (99/116/26) ‘but lately people are saying he’s bad’.
   d. wen i soba i wan lik yu dong (99/116/12) ‘when he’s sober he wants to knock you down’.

For this we need two further spelling rules and another realization rule:

(30) SR6. (+verb)
     (+pro) — *iz* (-past)

(31) SR7. (+verb)
     (+pro) — waz (+past)

(32) RR6. (+verb) (+verb)
     (+pro) — (+pro) / P_L (-verb)
     (+cont) (+past)

Next, *iz/waz* begins to spread to ‘predicate adjective’ environments. It would appear from Table 1 that this change affects past environments, introducing waz, before it affects present ones, and it seems possible that for at least some speakers, the basilectal completive *bin* is respelt waz prior to the further spread of the copula as such. But as this makes little difference to the overall process, for simplicity’s sake I shall ignore it here. In any case, once nominal environments are filled, the change spreads quickly to the next category, and while a speaker such as 15 retains de for all locatives, he has begun to introduce *iz* as a variable before what, to him, must still seem a subclass of stative verb:

(33) a. somtaim tu trakta de in wan fiil (15/16/10) ‘sometimes there are two tractors in the same field’.
b. som tshaaj tu dala wen plees baad (15/16/16) ‘some people charge two dollars when the place is bad’.

c. if di wata iz plenti yu get win rais (15/15/24) ‘if there’s a lot of water you get spindly rice’.

But here the trouble starts. To produce sentences such as (32c), 15 needs the following rule:

\[(34) \text{RR7.} \quad (+\text{verb}) \quad (+\text{verb}) \quad (+\text{verb})
\quad (+\text{pro}) \quad \rightarrow \quad (+\text{pro}) \quad / \quad P_L \quad (+\text{stat})
\quad (+\text{past}) \quad (+\text{att})\]

But he already has R6, which tells him that \((+\text{verb}) \quad (+\text{pro}) \quad (+\text{past})\) is equivalent to (therefore presumably synonymous with) \(((+\text{verb}) \quad (+\text{pro}) \quad (+\text{cont}))\); and he also has R3, which tells him that only \((+\text{verb}) \quad (+\text{pro}) \quad (-\text{cont})\) can be inserted under a predicate node when the next lowest predicate dominates \((+\text{verb}) \quad (+\text{stat})\). In other words, he has, for the same kind of environment, a rule that says ‘insert X’, a rule that says ‘X = Y’, and a rule that says ‘don’t insert Y’; or, R7, in the light of R6, contradicts R3.

How can this conflict be resolved? The speaker cannot simply reject R7, for that way lies English and social upliftment. But he cannot drop R3 either, for he needs it to avoid generating

\[(35) \quad a. \quad *\text{shi noin da ‘she is knowing that’}. \quad b. \quad *\text{Jaan laiking nofnof moni ‘John is liking a lot of money’}.\]

Nor can he very well drop or change R6; not only may it still vary with R4 for some speakers, but anyone whose social coordinates are similar to those of 15 will need a rule giving a-iz equivalence in nominal environments simply to understand a large part of what will be said to him. There is only one solution: keep R3 and R7, and let them alternate.

The result is that outputs like 15’s become endemic. Conflicts which arise through simple supersession of one rule by another are speedily resolved—witness the tu-for-fu/fi complementizer shift (Bickerton 1971 and Table 1b, MS) and many others. But a conflict caused by an actual clash of rules is another matter, and Table 2 shows how persistent it is in this case. Moreover, a second factor must speedily reinforce and amplify it. Since one man’s output is another man’s input, a persistent conflict in this environment will eventually produce the following spelling rule:

\[(36) \quad \text{SR8.} \quad (+\text{verb})
\quad (+\text{pro}) \quad \rightarrow \quad \text{iz}/\emptyset
\quad (-\text{past})\]

This rule would, of course, replace SR6.
TABLE 2. The spread of zero.

<table>
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<tr>
<th>Isolects</th>
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<th>4</th>
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</tr>
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<td>1</td>
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<td>1</td>
<td>1</td>
<td>3</td>
<td>11, 17, 119</td>
<td></td>
</tr>
<tr>
<td>D</td>
<td>1</td>
<td>1</td>
<td>13</td>
<td>3</td>
<td>15, 105</td>
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</table>

Key: Col. 1 = continuative verbal environments
      Col. 2 = locative and existential environments
      Col. 3 = 'predicate adjective' environments
      Col. 4 = nominal environments
      1 = Ving without aux. in Col. 1; de in Col. 2; V(+att) (or, ø + Adj.) in Col. 3; a in Col. 4
      2 = ø
      3 = iz/waz
      * = point from which copula variability disseminates

If we look at the data of Table 2, we shall find several things which seem to confirm this analysis. First we notice that of the first ten speakers in Table 2, only two use zero forms in nominal environments. Since both are speakers whose range extends much further 'upwards' (cf. Bickerton 1971:484; MS, 48-9) this should not surprise us, but in fact their deviance is dubious; both produce only one zero token each in this type of environment, and 99's is in a quote from somebody else, while 27's depends on whether one interprets prais as a noun or as a 'middle' verb (sc. 'is priced at'):

(37) a. i se a rispek yu tu di aies. yu di greet (99/116/11) 'he says, 'I respect you to the highest. You're the great(est)''.
    b. di rais prais tu nainti (27/27/24) 'the price of rice is two dollars ninety' (or) 'rice is priced at two dollars ninety'.

However, five of the next ten speakers have zero in nominal environments, and although frequencies are low, occurrences are completely unambiguous:
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(38) a. dooz pleesiz teribol rapidz (117/148/21) ‘those places are terrible rapids’.
   b. Kisam an mi laik famili (41/190/12) ‘Kisam and I are like relatives’.
   c. wa di miinin ov dat (196/254/22) ‘what’s the meaning of that’?

It is obvious that these sentences, except for zero forms, are closer to English sentences than any we have yet considered; and this is further evidence that zero cannot spread to nominal environments until movement from Creole to English is already very far advanced.

Confirmation also comes from the role of zero in locative environments. _de_ is next in line for replacement; but in contrast with prenominal _a_, it is replaceable by zero from the very beginning. This is, of course, exactly what one would expect if the English copula had now acquired a variable spelling _iz_/_Ø_:

(39) a. hi en de a wok, man (125/169/29) ‘he hasn’t been at work, man’.
   b. shi mekin gud tu, shi daun at di ool leedi (125/169/7) ‘she’s doing well too, she’s down at the old lady’s’.
   c. if di ool man waz hia hi kuda giv yu a laf (125/170/22) ‘if the old man was here he could have made you laugh’.

The remaining changes in the system are relatively minor (total elimination of _de_, the introduction of variable copula before continuative verbs, etc.) and we need not examine them in detail here (necessary additional rules are given at the end of Appendix I, where rules to date are summarised and the rule-shift component stated). It is much more important to try to evaluate the significance of what has already been analysed.

The net result of the changes outlined above has been to bring a clearly creole system to a position where it is virtually indistinguishable from the Black English system described in Labov 1969. Could this similar result have come about through a similar process? Let us examine in some detail what was claimed by Stewart at this meeting three years ago:

In an earlier recorded form of Gullah, for example, equation (i.e. predication with a noun-phrase complement) was marked, apparently categorically, by means of a morpheme which we might write _/da_/ . Verb phrases roughly equivalent to English _V + ing_ were _/da_/ followed by the unmarked verb. Thus we had both _Dem da fish_ ‘They are fish’ and _Dem da fish_ ‘They are fishing’. That these were only superficially similar is made clear from a later change to _V + ing_ for the verb
phrase, while retaining /da/ for equation, e.g. **Dem da fish** 'They are fish' but **Dem fishin** 'They are fishing'. Later still, the /da/ equative morpheme was relexified to /iz/, so that one said **Dem iz fish** 'They are fish' but **Dem fishin** 'They are fishing'. Finally, with the introduction of an optional dummy /iz/ in V-ing phrases, and a partial collapse of verbal /iz/ = 0 with equative /iz/ ≠ 0, one can see the historical process—entirely documentable—which could easily have given rise to the statistical difference in copula deletion discovered by Labov (Stewart 1969:244).

The similarities between the two accounts of decreolisation—mine and Stewart's—are very striking; indeed the only serious difference is the importance of the role which I assign to the 'predicate adjective' environment.

Several points should be emphasized here. First, Stewart's account is based entirely on historical evidence, while mine is based on recordings made over the period 1968-70. Second, the areas concerned are two thousand miles apart and have never had more than negligible contact with one another, so that changes can hardly have spread from one to the other. Third, this analysis was complete save for trivial details when my attention was drawn to Stewart's paper, so the two are mutually independent. Fourth, though questions of motive may be irrelevant in scientific enquiry, it seems worth mentioning that the demonstration of similarities between Black English and Guyanese Creole formed no part of my original intention, but was simply a by-product of the attempt to write a polylectal grammar of the latter. Fifth, the similarity between both the situations described and the analyses thereof is too great to be merely coincidental.

These points raise issues too complex to be examined in detail here. For instance, it does appear that the claim that a synchronic polylectal grammar is equivalent to a diachronic grammar of linguistic change can be abundantly justified. It also appears that there may be a universal decreolisation process, or at least one applying to all English-based creoles in contact with English. This in turn is only a special case of perhaps the most fascinating issue raised by polylectal grammars—the significance of constrained implications. Such implications, it now seems, are the surface indications of rule-relationships which may fall under the following three heads:

(i) $R_i$ blocks $R_j$ (i.e. $R_j$ cannot be acquired until $R_i$ has been deleted). One example is negative placement in Guyanese verb phrases. The basilectal negative morpheme `na` invariably occurs first in the phrase, just as the acrolectal (English) `not` invariably occurs second. But instead of spreading to all environments as most new rules do, the acrolectal placement rule can only move to a fresh environment once
all basilectal and mesolectal modals and aspect markers have moved out of it, i.e. we never find *bin na any more than *not do.

(ii) \( R_i \) entails \( R_j \) (i.e. adoption of \( R_i \) means \( R_j \) must soon follow). All arguments that what happened had to happen must be treated with suspicion, but the development of third-person pronouns as described in Bickerton MS, where adoption of gender distinction erases already-existing case-distinction, seems a likely candidate for this category.

(iii) \( R_i \) plus \( R_j \) yields \( R_k (\neq R_K) \) (i.e. acquisition of two conflicting rules creates a novel rule incompatible with the equivalent target rule.) If the foregoing analysis of the origin of zero copula is correct, we need look no further for an example.

Now let us glance briefly at how these relations might affect second language learning. The various stages through which we have worked correspond to hypothetical grammars of English (homing ever closer on the target grammar) as produced by Guyanese speakers. They inevitably remind one of the hypothetical grammars successively produced by children as they learn their native language (Cook 1969:208). Now if Reibel (1969:290) is correct, first- and second-language learning work the same way. But while everyone hits one target, how few hit the other! For this fact, traditionally attributed to some mysterious (and otherwise unevidenced) loss of language-learning ability, an alternative explanation now suggests itself. There is one big difference between first- and second-language learning; the second-language learner already has a grammar, which must affect the type of hypothesis he will make. Hitherto it has been tacitly assumed that inaccurate or incomplete hypotheses about the target language could be easily rectified or replaced. But if rules can block rules, entail rules, or combine to produce wholly novel rules, then there are quite real and hitherto unsuspected reasons why so many learners fail, either relatively or absolutely, to acquire grammars of foreign languages. The implications for language teaching would be quite literally revolutionary; alas, this paper is already far too long, and discussion will have to be postponed to some future occasion.

To sum up, then: polylectal grammars can be written, and moreover the increase in complexity is less than one might have feared. Indeed, the necessity of seeking a quasi-universal basis for such grammars may well add to our knowledge of language in general. In the course of writing it, insights were derived which could hardly have been gained in any other way. These insights were both particular (into the specific origins of zero copula) and general (into the relationships that obtain between rules during linguistic change), and the latter may have a very direct practical application in the field of education. It may also (although I would hesitate to claim this) be of some use to the sociologist in such areas as the study of acculturation, social distance, role-switching and the like. It has certainly demonstrated the
oblique fashion in which, in nature, things move from State A to State B; and perhaps, finally, there may be a moral for sociolinguists somewhere in this. Perhaps it really is the case that the best procedure in our field is not a frontal assault on 'language in society', but rather a deepening and richening of one arm of the hybrid, which may then, like a rule-change, 'spread to other environments', and, in time, inform the interdiscipline as a whole.

APPENDIX 1. Summary of rules

PS1. $S \rightarrow P + NP$
PS2. $NP \rightarrow NP + (NP) + (S)$

PS3. $P) \rightarrow SDF_{a...n}$

PS4. $SDF_{a...n} \rightarrow + \text{verb(al)}, +\text{loc(ative)}, +\text{stat(ive)}, +\text{comp(letive)},$
$+\text{cont(inuative)}, +\text{pro(verb)}, +\text{att(ributive)}, +\text{past}$
$\text{etc.}$

SR1. (+verb)
(-pro) $\rightarrow \text{de}$
(+loc)
(+stat)

SR2. (+verb)
(-pro) $\rightarrow \text{hat, haad, veks, etc.}$
(+att)
(+stat)

SR3. (+verb)
(+pro) $\rightarrow a$
(+cont)

SR4. (+verb)
(+pro) $\rightarrow \text{bin}$
(+cont)

SR5. (-verb)
(+pro) $\rightarrow \text{in}$
(+cont)

SR6. (+verb)
(+pro) $\rightarrow \text{iz}$
(-past)

SR7. (+verb)
(+pro) $\rightarrow \text{waz}$
(+past)

SR8. (+verb)
(+pro) $\rightarrow \text{iz/\o}$
(-past)
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RR1. \[ P_H \rightarrow (\text{+verb}) / P_L (\text{-pro}) \]
\[ (+\text{pro}) \]
(Where \( P_H \) is a higher, \( P_L \) a lower predicate)

RR2. \[ (\text{+verb}) \rightarrow (\text{-cont}) / P_L (\text{+verb}) \]
\[ (+\text{pro}) \]

RR3. \[ (+\text{verb}) / P_L (\text{+stat}) \]
\[ (\text{+verb}) \rightarrow \emptyset \]
\[ (+\text{pro}) \rightarrow (\text{-past}) / P_L (\text{+stat}) \]
\[ (\text{-past}) \]

RR4. \[ (\text{+verb}) \rightarrow (\text{+cont}) / P_L (\text{-verb}) \]
\[ (+\text{pro}) \]

RR5. \[ (+\text{verb}) \rightarrow (\text{-verb}) / P_L (\text{+verb}) \]
\[ (+\text{pro}) \rightarrow (\text{+cont}) \]

RR6. \[ (\text{+verb}) \rightarrow (\text{+past}) / P_L (\text{-verb}) \]
\[ (+\text{pro}) \rightarrow (\text{+past}) / P_L (\text{+stat}) \]
\[ (+\text{att}) \]

RR7. \[ (+\text{verb}) \rightarrow (\text{+verb}) \]
\[ (+\text{pro}) \rightarrow (\text{-past}) \]

RR8. \[ (+\text{pro}) \rightarrow \text{iz} / \# (-\text{verb}) \]
\[ (-\text{past}) \]

RR9. \[ (+\text{verb}) \rightarrow (\text{+past}) \]
\[ (+\text{loc}) \]
\[ (+\text{stat}) \]

RR10. \[ (+\text{verb}) \rightarrow (\text{+past}) / P_L (\text{+verb}) \]
\[ (+\text{pro}) \rightarrow (\text{+past}) / P_L (\text{+verb}) + (+\text{pro}) \]
\[ (+\text{cont}) \]

T1. SD: (-verb)
\[ (+\text{pro}) + (+\text{verb}) \]
\[ (+\text{cont}) \]

SC: 1, 2 → 2, 1.

Rule-Shifts

PS 1 - 4 unchanged throughout.

RS1. SRs: 1, 2, 3, 4. RRs: 1, 2, 3, 4.
RS2. As RS1 + 5. As RS1 + 5 + T1.
RS3. As RS2 - 3 - 4 + 6 + 7. As RS2 - 4 + 6.
RS4. As RS3. As RS3 + 7.
RS5. As RS4 - 7 + 8. As RS4 + 8.
RS6. As RS5 - 1. As RS5 + 9.
RS7. As RS6. As RS6 + 10.

Convention: \( R_i \sim R_j \)
APPENDIX 2. Speakers cited in text.

Note: For each speaker the following data are listed: (i) sex, (ii) race (A = African, El = East Indian, Am = Amerindian), (iii) age, (iv) place of residence, (v) occupation, (vi) by whom recorded (G = Guyanese, E = English).

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NOTES

1. The research on which this paper is based was made possible by a grant from the Ford Foundation. I am also deeply indebted to William Labov and Charles-James Bailey for extensive discussion of the ideas presented herein, and in particular for their detailed criticism of a previous version of the rules. Needless to say they do not necessarily subscribe to the opinions expressed here, which remain my sole responsibility.

2. I may the more readily do this since the theoretical aspects have already been extensively dealt with in Bailey 1971 and works therein cited. Bailey has there written: ‘The usual procedure...would be first to show that a polylectal grammar is feasible and then that it is the most correct model among the choices available. What has been said up till now has followed the reverse order, first showing that a polylectal grammar is what is demanded by linguistic facts, and then commenting on the feasibility of formulating such a grammar.’

3. Though the surface area of Guyana is over 80,000 square miles, the vast majority of its population is confined to a narrow coastal and riverine strip probably no more than 1,000 square miles in area.

4. I have in mind an extremely interesting recent paper by Lopreato and Alston (1970). Although it is aimed specifically at sociological metatheoreticians, much of it would apply to their linguistic opposite numbers with equal force; for instance, Chomsky and his followers might well ask themselves how much of the authors’ criticism of Weberian ideal types applies to their ‘ideal speaker-listener’.

5. This differs from the definition given in Bickerton MS, where isolects which contained rule-conflicts were inserted between each pair of invariant isolects. This is a cumbersome procedure, which can be avoided by adopting a simple convention \( R_i \sim R_j \), stipulating an intermediate stage between each pair of isolects in which a rule held by the first but not the second will alternate with one held by the second but not the first, to yield a variable output.


7. All citations followed by numbers are actually recorded utterances. The first number represents the speaker’s survey number, the second and third, page and line respectively of the transcribed version. See Appendix 2 for data on speakers.

8. This utterance might seem to contradict Rule T3, but in fact basilectal aspect markers are deletable in a wide variety of (or perhaps in all) subordinate clauses, certainly those expressing hypothetical conditions. In the mesolect, rules for tense and aspect marking become extremely confused; this area, spotted as a problem one by Labov (Labov et al., 1968, I, 7) in his discussion of Solomon 1967, is currently under study.
It is possible that a, da, de all come from a common pidgin ancestor which combined location and continuity. This point is of rather more than merely historical interest in view of the suggestion by some generative semanticists that locatives and continuatives derive from the same source.

Since completing this paper, I have heard that Richard Day, working on Hawaiian English, has discovered the same implicational relations between environments for absence of copula that Labov found in Black English and I found in mesolectal Guyanese Creole. This is all the more interesting because while Black English and Caribbean Creoles arguably form part of the same dialect-continuum, one could hardly claim this for Hawaiian English.

Properly speaking, these should be listed in the lexicon, together with their features. I have inserted them here, using a vintage-TG-type notation, simply for the sake of completeness.

An alternative, and perhaps superior, way of dealing with changes of this kind would be to add in features to the description of the copula, e.g. in this instance it would add (+ loc). But this together with other dubious points in the rules is probably best left as fluid as possible at this stage.

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WHERE DO GRAMMARS STOP?

WILLIAM labov

University of Pennsylvania

Introduction. As linguistics begins to turn outwards toward the speech community, it will once again be possible to offer a theory for those who are deeply committed to writing the grammar of a particular language. There are of course many open questions on the internal organization of grammar, but before anything else it must be decided what the grammars are about; I take it as agreed here that the only valid grammars will be about the language used in every-day life by ordinary citizens arguing, greeting, gossiping, calculating, persuading, and running off at the mouth in countless other ways.

But we must also decide where these grammars begin and where they end—their external and internal boundaries. How broad, or how narrow is the grammar of a speech community? This is quite a different question from that of determining the boundaries of the speech community itself which is defined by a much wider range of phenomena besides its grammar: differences in vocabulary, in cultural traits, in phonetic posture, and discontinuities in lines of communication. The grammars that we are discussing must be broadly conceived to include phonology, semantics, and discourse rules, but they would not include the dictionary or a whole ethnography of speaking. The study of the external boundaries of a grammar is therefore a subset of the problem of drawing dialect boundaries, but the most central one for linguistic purposes. We will adhere for the present to the Chomskyan notion that the grammar might represent the knowledge that the native speaker needs to produce and understand the language in native fashion. There will be other things that we want to say about the grammar which will not fit within this framework: the distribution of lexical items among derivational suffixes, the differential rate of change of cer-
tain parts of the grammar, the extent to which the morphology fits in with general tendencies of marking and unmarking—such facts are of no use to the native speaker, and must be put into a meta-grammar. We will then have a base for the empirical investigation of grammars that speakers actually have available, trying to discover which speakers actually use which rules.

As we will see, 'use' is not a single dimension: there are many ways in which a variant form may be recognized, understood, evaluated, projected and controlled by speakers. We will examine some of these properties first in relation to the proposition that we only need one grammar for a language: that the knowledge of a native speaker, potential and actual, reaches out to embrace the patterns used by all other speakers.

1. The outer limits of a grammar

The notion of a pan-dialectal grammar was put forward most forcefully by C.-J. Bailey in 1969, at the Los Angeles Conference on 'Historical Linguistics in the Light of Generative Grammar' (Bailey 1972). He pointed out that we can and should write a single grammar to encompass all (or nearly all) of the dialects of a language, since the competence of the (fully adult) native speaker reaches far beyond the dialect he uses himself. Bailey argues for such grammars on the ground that (a) as native speakers become older, they become familiar with an increasingly large number of other dialects; (b) they have the ability to understand and interpret the productions of those other dialect speakers, analyzing their rules as extensions of limitations of their own rules; and (c) they can even extrapolate from their own rules and predict the existence of dialects which they have never heard. Bailey's argument also invoked the existence of universals of change as proposed by Kiparsky (1968) in which the existence of one (marked) rule ordering presupposed the possibility of another (unmarked) order.

We can then project the possibility of a single grammar in which various rules and environments or rules are seen ordered along a single dimension, one implying the other, and the writing of a single grammar justified by the uniform ordering of these features. Such ordering may be universal, or fixed for a given area; on the other hand, the transition to a different ordering would be a nice argument for a separate grammar.

Simplicity considerations play an important role in such constructions. They are the basic argument for bringing together various sub-rules in different environments into a single rule. If native speakers do this in the rule systems they form themselves, then sub-dialects will be forced into alignment by the tendency to compact individual rules into a single larger rule and to simplify the environments. There is considerable disagreement on whether or not there is empirical evidence to prove the existence of such simplifying activity (Bach and Harms 1972, Schane 1972, Labov 1972a, King 1969).
The argument as to whether rules are added only at the ends of grammars is also important here. If rules were added only at the end, or at the end of certain sections, then it would be possible to write general grammars down to a certain point and stop, leaving the details to be worked out in later volumes by dialectologists. This is the strategy followed by Chomsky and Halle (1968), and it is not inconceivable that native speakers follow comparable strategies. If so, any grammar of English will be pan-dialectal to a point, and speakers may proceed with the work of interpretation in confidence that there are no misapprehensions behind them.

Finally, we can consider the extension of this argument to bilingual communities. Gumperz (1971) argues that we can construct a single 'repertoire' for the several 'different' languages which a speaker can use. Can this repertoire be in fact a grammar? If we consider Gumperz' findings in Kupwar, it seems quite likely. Gumperz shows that the surface morphology and the lexicon are totally different for the local Marathi and Kannada spoken in Kupwar. But Gumperz has also shown that it should be possible to write a single set of grammatical categories, phrase structure rules and transformations, and a common set of output constraints on surface ordering for syntax and phonology. This would go beyond a pan-dialectal grammar, and demonstrate the ability of speakers, over a long period of time, to bring two rule systems into close approximation so that they are in effect one system.

It will first be necessary to see what kinds of evidence we might use to decide if a given rule is potentially or actively in the dialect of a given speaker. Let us consider A', a native speaker of Dialect A, who has been in contact with Dialect B for some time. If other speakers of A have been in intimate contact with B for long periods, we can follow Gumperz in arguing that the work of translation is apt to be quite simple: a mechanical re-coding of lexical items. But if there is no history of contact, there may be deeper differences, and A' must use his own linguistic competence to ferret them out. He may not know himself how much or how little he knows about this dialect, but we should be able to put him to the test in various ways to decide how much of B he has assimilated: whether he has activated his pan-dialectal grammar or constructed one for the occasion.

There are six questions we can pose about A''s grasp of a rule of Dialect B which will help us decide what kind of a rule he himself is using. A' has just heard a speaker of B use a form B_1 that is not produced by A''s grammar. (1) Does A' recognize B_1 as grammatical for some native speakers of English? In a word, does he know that B_1 exists? Or, faced with B_1 for the first time, can he recognize it as a possibility open to speakers of English? (2) Can he evaluate its social significance—that is, see it as colloquial, formal, slang, or stigmatized so that he would know in what social context to use it? (3) Can he interpret B_1—not just in the normal favorable contexts, where it is supported
by other forms, but in neutral and unfavorable contexts as well? (4) Can A' label the meaning of B₁ in zero contexts, faced with the isolated form itself? The labelling function might seem beside the point, since the normal use of language does not require it; but it will appear that in the heartland of Dialect B, speakers will be able to choose the right label when outsiders fail. (5) With or without this understanding, can A' predict the use of B₁ in an extended range of environments—both syntactic and semantic? Finally, (6) can he use B himself productively? (6) does not automatically follow from (5), since the use of language in social interaction requires a much higher degree of skill and a kind of overlearning that is not needed to predict the use of others in a reflective mode.

The six abilities we are testing seem to form three sets. The first two depend upon the broadening of wider experience beyond the vernacular as noted by Bailey; the second two depend upon the second ability to extrapolate from one's own vernacular rules and assign the new form to the proper sub-rule; the last two represent the deeper ability to re-create the exact form of the rule or sub-rule and synthesize new forms. Ability (5) is crucial for deciding if the form is represented in a pan-dialectal grammar in the same form as in the vernacular where it originates.

With these preliminary considerations, let us consider some empirical investigations.

The major sociolinguistic studies which we carried out in the years 1963-1969 were centered in particular communities. We always had some outsiders in our sample, and so we were able to contrast speech and norms across the boundary of the speech community in question. For the past four years we have also been gathering exploratory materials throughout the English-speaking world, primarily in connection with our instrumental studies of sound change in progress. More recently, we have begun study of the internal and external boundaries of the Philadelphia speech community, including the acquisition of local rules by outsiders moving in, and the knowledge that Philadelphians have of other speech communities. The findings reported in the first half of this paper are the result of a joint investigation carried out by myself, Laura Dent, David Depue, Beatrice Lavandera, Arvilla Payne, Angela and John Rickford, and Malcah Yaeger of the University of Pennsylvania. The materials to be presented here are in the nature of a progress report, drawing on various facets of these investigations that bear on the problem of the outer limits of a grammar.

1.1 A general categorical rule: negative attraction

Before examining differences between dialects, it is important to locate a rule which we know is pan-dialectal so that we can recognize its properties and begin with the firm knowledge that at least some
The rule which operates here is NEGATTRAC shown as (2):

(2) $W - \text{Indet} - [+\text{NEG}]$

$$1 \rightarrow 2 \rightarrow 1 + 3 \rightarrow 2$$

Condition: Obligatory if 1 does not contain [+NEG] or [-FACT] commanding 2 and 3; and 2 is [-STRESS].

The condition notes that the full force of NEGATTRAC is not felt if there is a negative or hypothetical in a higher sentence commanding the indeterminate subject as in (3a) and (3b) or if any is stressed as in (3c):

(3a) I don't say that anyone didn't go.
(3b) If anyone doesn't go, he'll be sorry.
(3c) Just anyone can't go.

But in the basic situation of (1), negative attraction is absolute. In our previous reports (Labov, Cohen, Robins, and Lewis 1968:39) black and white adolescents in Manhattan could not repeat back sentences such as

(4) *Anyone doesn't sit there anymore, do they?

Listeners do not hear such sentences as meaningful, or even hear them at all. In more recent studies in Philadelphia, carried out by Mark Baltin and myself, the same confused responses are given by middle class and working class subjects.

(5) 'Anybody didn't arrive.'
  Ginny: That doesn't make any sense. Who's anybody?
  Joe: Anyone didn't arrive!
  Tom: I don't understand it actually. Anybody didn't arrive? I just don't understand it...
  Wendy: It doesn't make a lot of sense. I suppose I could get a meaning from that, but I'm not sure what.
In one important sense, violations of NEGATTRAC are meaningless. It is not that the listeners fail to understand the referential intent of (4)—but rather that the act of violation itself cannot be interpreted. The NEGATTRAC rule is one of the classic examples of a Type I Categor-ical rule.³

1. It operates at all times for all dialects.
2. Violations do not occur and are therefore not reportable.
3. When violations are constructed, they are often not heard and when heard cannot be evaluated.

It follows that such Type I rules are not likely to be limited to particu-lar dialects, and will form the backbone of pan-dialectal grammars. If they did not operate in particular dialects, some speakers in every dia-lect area would have heard them and begun to interpret violations as 'the way they talk out in X'. But we have never obtained such a reac-tion, or ever observed a violation in the speech of anyone.⁴ The unin-terpretable character of violations of Type I rules implies their pan-dialectal character.

1.2 A general variable rule: negative concord

Another candidate for a pan-dialectal grammar is the opposite type of rule: one which is optional throughout the language, so that the two forms with and without the rule applying are familiar to everyone. As an example of such a situation, we can consider negative attraction, which transfers the negative rightward to indeterminates any, ever, and either, pleonastically multiplying the expression of negation without changing meaning.

(6) He don't ever know anything either —•• He don't never know nothing neither.

In modern English, negative concord is always variable, as opposed to Spanish or Hungarian, where it is categorical. It is a typical Type III variable rule, opposed to Type I rules in the properties listed above:

1. It is variable for all dialects.
2. Since forms with and without negative concord appear frequently, single utterances are not violations, and are therefore not reportable.
3. The use of one variant or the other is heard as socially signifi-cant and can be evaluated by adult listeners.

In contrast to negative attraction, negative concord is governed by the affective and stylistic needs of the speaker: it provides a mechanism
for strengthening the act of negation. The constraint introduced into standard early modern English is that a negative in deep structure will be represented by only one negative in surface structure; the standard rule of Negative Postposing can then operate to provide emphasis in the standard language, though less effectively perhaps than in the nonstandard dialects that use negative concord. (7b) shows negative concord applied to (7a) and (7c) shows the more limited action of the nonpleonastic negative postposing.

(7a) They don’t know anything about any pocketbook.
(7b) They don’t know nothing about no pocketbook.
(7c) They know nothing about any pocketbook.

Though negative concord is used in all dialects except standard English, it does not apply in all environments in all dialects. In fact, the range of environments for negative concord forms an implicational series (Labov, Cohen, Robins, and Lewis 1968:36). Briefly, negative concord applies optionally to indeterminates any and ever, more often in the same clause (8a) than in following subordinated clauses (8b).

(8a) Nobody said nothing about it.
(8b) Nobody said for him to do nothing about it.

In some Northern dialects, and in most Southern dialects, the negative also appears in pre-verbal position in the same clause as the original negative brought into subject position by negative attraction (8c). We will refer to these dialects as NS₂.

(8c) Nobody didn’t say nothing about it.

In other dialects, such as the white New York City vernacular, (8c) would have to mean the opposite of (8a), as both the first and second negative would refer to semantic units in a deep structure. These dialects will be grouped as NS₁. We then have a classic problem for the pan-dialectal grammar: can we condense rule (9a) and rule (9b) into rule (9c)?

(9a) NEG X Indet
     1 2 3 → 1 2 <1> + 3

(9b) NEG X \{Indet
     \{Vb \}
     1 2 3 → 1 2 <1> + 3
     Condition: if 3 = Vb, only if 1 and 3 are clause mates
The economy effected by (9c) is a persuasive argument for linguists who want to write simple rules. Do native speakers have the same ability to compare, condense, and relate their negative concord rules to others? We do not know yet the full list of dialect areas that fall under NS₂, but they include a number of different geographical regions, and we can argue that most NS₁ speakers have heard the NS₂ form of the rule applied. The problem exists only for NS₁ speakers, of course, since they would have to figure out that (8c) used by a NS₂ speaker means the same as (8a). For NS₂ speakers, there is no problem in understanding NS₁ talk, since they can also produce (8a), and in fact usually do so. We can make direct inquiries about whether or not English speakers say Nobody didn’t know for ‘nobody knew’. When we do so, the responses are discouraging for rule (9c). Many NS₁ speakers will say that they never heard of such a thing. This is not just a casual response. Ursula Bellugi and Edward Klima encountered this phenomenon in the 4-year-old speech of Adam (Bellugi 1967) and argued that it must be an internal development of the grammar since no native adults used negative concord to pre-verbal position in this way. Since Klima had written an important article on relatedness between non-standard grammars, and his reaction was the product of wide reading and deep reflection, it must be argued that (9c) did not exist in his grammar at that time.

It is important to note, though, that Bellugi and Klima had no difficulty in understanding what Adam was saying. We might argue then that (9c) is justified not by speakers’ having heard sentences such as (8c) but by their native ability to extrapolate from (9a) to (9c), applying negative concord to a wider range of environments by the use of Bailey’s second hypothetical ability. We might do better then by asking speakers to interpret sentences than by asking for judgments of grammaticality.

We can now consider some results of empirical investigations which attempt to determine whether rules like the pre-verbal extension of negative concord are available to most speakers of the language, judging by their display of five of the six abilities listed above.

A series of instruments were developed which explore a subject’s ability to recognize, evaluate, interpret, label and predict the use of seven rules of English syntax which show regional, ethnic or individual variation. Our subjects have been primarily university students and college-educated adults. These speakers are not the most informative subjects for a study of the productive aspect of dialect differences. Their social and geographic mobility brings them into contact with
speakers of many different regional and class dialects; as a result of this experience and their basic attitudes towards language, college students modify their basic vernacular and add superposed varieties. For the same reasons, they are the most favorable base for examining the possibility of a pan-dialectal grammar. In looking for the outer limits of such a grammar, we can argue that college-educated middle-class speakers have a wider range of exposure to other dialects than other social groups in their region, and have engaged in more overt discussion of language as well. Limits on their grammars might show the general limits on pan-dialectal grammars.

Our successive instruments are referred to as Q-SCOM I, II, and III with various modifications. Subjects were interviewed with the overt purpose of finding out how well and how much they could understand other dialects of English. The subject is first asked to rate a number of sentences on a scale of native status: whether for a given sentence (1) any native speaker would say it; (2) some definitely would but not all; (3) perhaps some one native speaker might say it; (4) no native speaker would say it (recognition). The subject’s understanding of a given form is then tested by questions which look through the grammar to some real-life situation (interpretation). He is then asked to state explicitly the meaning of the form in question (labelling). After the correct meaning is then supplied, the subject is asked if he has ever heard such expressions, and if so, where (recognition). He is then told the region where the form is used, the correct meaning is reinforced, and he is asked to guess whether or not certain parallel extensions of the same form would be used in that area by those speakers (prediction). In this investigation, we have accumulated data on several hundred subjects with a broad range of geographic backgrounds. The main samples are focused on residents of Philadelphia; four sub-samples were drawn from populations located in regions where the particular forms being studied were native.

In this report, I will draw briefly from data on three of these linguistic variables, and examine one in greater detail.

In Q-SCOM I, 32 subjects were asked what it would mean to them if they asked someone ‘Was the party a big success?’ and they received the answer, ‘Nobody didn’t take it in.’ Twenty-six interpreted this sentence as an example of negative concord, with the pre-verbal option of (9c) applying, and the meaning ‘Nobody went’. Few of them claimed to use this option themselves; as a whole this group represented the NS1 dialect. Five subjects interpreted this answer as containing two underlying negatives, meaning ‘Everyone went’, and only one gave both possibilities.

Later investigations used question (10):

(10) Someone asked, **Who do you think is gonna be the next president?** and the answer was **Nobody can’t figure that out.** What do you think he meant?
Here we acquire two types of data: meanings volunteered by the subject; and meanings admitted by him when they are suggested. I will use the notation NEG to indicate an interpretation of 'Nobody can' and POS to indicate 'Everybody can' (derived from two underlying negatives, 'Nobody can not'). NEG(POS) indicates that NEG was volunteered and POS admitted on suggestion; NEG/POS that both were volunteered, etc. The results of five different investigations of this issue are shown in Table 1. In all cases, the ratio of NEG to POS is very high, again indicating that most speakers can freely apply the rule of negative concord to these sentences. But only a small minority of the speakers stated that they could use such constructions themselves. Many, like Bellugi and Klima, did not consciously recognize that there were dialects which applied NEGCONCORD in this way. Their ability to recognize this extension of negative concord therefore indicates that it has a firm place in a pana-dialectal section of English grammar, and the question as to whether they actually use this part of the rule is a subsidiary fact.

### Table 1. Responses to negative concord to preverbal position in question (10).

<table>
<thead>
<tr>
<th></th>
<th>Neg</th>
<th>Neg(POS)</th>
<th>Neg/POS</th>
<th>(Neg)POS</th>
<th>Pos</th>
<th>Say (10) as Neg oneself</th>
</tr>
</thead>
<tbody>
<tr>
<td>Q-SCOM II</td>
<td>9</td>
<td>6</td>
<td>1</td>
<td>3</td>
<td>2</td>
<td></td>
</tr>
<tr>
<td>(Philadelphia)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Q-SCOM III</td>
<td>31</td>
<td>5</td>
<td>3</td>
<td>3</td>
<td>8</td>
<td>4</td>
</tr>
<tr>
<td>(Philadelphia)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Q-SCOM III</td>
<td>21</td>
<td></td>
<td></td>
<td></td>
<td>6</td>
<td>10</td>
</tr>
<tr>
<td>(Kansas)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Q-SCOM III</td>
<td>13</td>
<td></td>
<td></td>
<td></td>
<td>10</td>
<td>2</td>
</tr>
<tr>
<td>(Rhode Island)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Q-SCOM III</td>
<td>11</td>
<td>1</td>
<td></td>
<td></td>
<td>2</td>
<td>6</td>
</tr>
<tr>
<td>(Utah)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

The only area where we found a significant sizable increase in the number of POS responses was in the Providence sample. Since we know that all speakers are capable of using two underlying double negatives, the high percentage of positive interpretations may mean that these speakers are less familiar with this form of negative concord. Or it may mean that the particular context favored two underlying negatives more heavily for them. Since in this sub-sample no 'suggested' responses were obtained, we are not in a position to argue further these two possibilities.
The highest percentage of those who said they could use the pre-verbal extension of negative concord was found in Utah, where half of those who favored negative interpretations reported that they or people in their neighborhood could use this form of the rule.

We then conclude that rule (9c) exists in a general grammar of English. Extensions of negative concord do not stop at this point, however. In previous investigations of the Black English Vernacular (Labov, Cohen, Robins, and Lewis 1968; Labov to appear) we found that negative concord is actually extended to pre-verbal position in a following clause. Our analysis of this situation begins with the sentence

(11) It ain't no cat can't get in no coop.

The sentence was used to mean 'there aren't any cats which can get in any (pigeon) coop'. First reactions from white speakers indicated that in their grammars this sentence would always mean 'there aren't any cats which can not get in any coop'. We have no evidence to contradict the generalization that white speakers do not extend negative concord in this way. But when the sentence is presented without comment to a group of white subjects, a good number will guess that it means 'can'. Frank Anshen reports such a result in classroom experiments at Stony Brook (personal communication). In our first investigation of this problem 14 out of 33 subjects interpreted (11) with one negative in deep structure and negative concord applying to pre-verbal position. Since 12 of these 14 subjects were white, it seemed that the pan-dialectal grammar could be reasonably enlarged to include this possibility. If half of the subjects could extend negative concord to the first example, we must assume that further examples would lead to greater recognition and understanding. We would then replace the condition on (9c) with (9c').

(9c') Condition: 3 = Vb only for WNS\(_2\) and BEV and for WNS\(_2\) only if 1 and 3 are clause mates.

(9c') adds some additional complexity since we now have to separate NS\(_2\) into WNS\(_2\) and BNS\(_2\) or the Black English vernacular (BEV), but this must be done for many other rules. The rather awkward condition (9c') might be replaced by the matrix of (12) where 0 means the rules do not apply, x means it is variable, and 1 indicates categorical or semicategorical use of the rule.
There is one additional fact which must play a large part in our interpretation of the matrix: in the BE vernacular, negative concord to indeterminates within the clause is semi-categorical: that is, it normally applies, so that exceptions are marked and reportable, with most speakers using 100%. We will return to the implications of this fact in Section 2, which considers the internal boundaries of a grammar.

The further investigations of negative concord in Q-SCOM-II, III, etc., used a question parallel to (10) above.

(13) Some asked, *Who do you think is gonna be the next president?* and the answer was, *There ain't nobody can't figure that out.* What do you think he meant?

Table 2 shows the results of the same series of studies reported in Table 1, but here cross-tabulating answers to (13) against (10). In these four-cell tables, we have grouped together NEG with NEG(POS), etc.; in every case the majority in a cell were simply NEG or POS. Some speakers show POS for both (10) and (13); they do not recognize that negative concord can apply in either case. Some recognize that it can apply to both (NEG-NEG). A third major group sees NEGCONCORD applying to (10) but not to (13). But for the fourth possibility, we have almost empty cells. Only 10 subjects out of 139 felt that negative concord could apply in (13) but not (10). The breakdown of the particular studies shows that one or two subjects behaved this way in each case. Table 3 shows another mode of analyzing the data, grouping together all the cases in which (10) favors negative concord, more than (13); all the cases in which (10) and (13) were responded to in the same way; and the cases in which (13) favors negative concord more than (10). There are only 13 cases of the latter: four of them from the first Q-SCOM-II study carried out in the Philadelphia area. The largest number of cases—67—showed the NEGCONCORD rule (9c) favoring (10). This result fits in with the matrix of (12) which was constructed from the observation of actual speech.
TABLE 2. Correlation of responses to negative concord to preverbal position in same clause and following clause in questions (10) and (13)

<table>
<thead>
<tr>
<th>Location</th>
<th>Same Clause (10)</th>
<th>Following Clause (13)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Q-SCOM II (Philadelphia)</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>NEG 10  POS 1</td>
<td>NEG 10  POS 1</td>
</tr>
<tr>
<td>Black Subjects</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>NEG 8  POS 0</td>
<td>NEG 8  POS 0</td>
</tr>
<tr>
<td>Q-SCOM III (Philadelphia)</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>NEG 30  POS 5</td>
<td>NEG 30  POS 5</td>
</tr>
<tr>
<td>Q-SCOM III (Kansas)</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>NEG 7  POS 1</td>
<td>NEG 7  POS 1</td>
</tr>
<tr>
<td>Q-SCOM III (Rhode Island)</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>NEG 9  POS 2</td>
<td>NEG 9  POS 2</td>
</tr>
<tr>
<td>Q-SCOM III (Utah)</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>NEG 4  POS 8</td>
<td>NEG 4  POS 8</td>
</tr>
<tr>
<td>Black Subjects</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>NEG 3  POS 2</td>
<td>NEG 3  POS 2</td>
</tr>
</tbody>
</table>
TABLE 3. Number favoring negative concord to pre-verbal position in same clause over following clause

<table>
<thead>
<tr>
<th></th>
<th>NEG CONCORD favored in (10) over (13)</th>
<th>NEG CONCORD same in (10) as (13)</th>
<th>NEG CONCORD favored in (13) over (10)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Q-SCOM II</td>
<td>9</td>
<td>8</td>
<td>3</td>
</tr>
<tr>
<td>(Philadelphia)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Q-SCOM III</td>
<td>33</td>
<td>18</td>
<td>12</td>
</tr>
<tr>
<td>(Philadelphia)</td>
<td></td>
<td></td>
<td>3</td>
</tr>
<tr>
<td>Q-SCOM III</td>
<td>12</td>
<td>7</td>
<td>2</td>
</tr>
<tr>
<td>(Kansas)</td>
<td></td>
<td></td>
<td>2</td>
</tr>
<tr>
<td>Q-SCOM III</td>
<td>4</td>
<td>9</td>
<td>8</td>
</tr>
<tr>
<td>(Rhode Island)</td>
<td></td>
<td></td>
<td>2</td>
</tr>
<tr>
<td>Q-SCOM III</td>
<td>9</td>
<td>3</td>
<td>1</td>
</tr>
<tr>
<td>(Utah)</td>
<td></td>
<td></td>
<td>2</td>
</tr>
<tr>
<td>Total</td>
<td>67</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

When we look at the regional groups, it is apparent that the Kansas and Utah responses are heavily conditioned in favor of NEG for (10) but POS for (13). This agrees with the higher percentage of speakers who report the use of negative concord to pre-verbal positions. On the other hand, the Rhode Island subjects do not favor the use of negative concord in either case. These results seem to reflect a dialect difference between Rhode Island and the Mid-West.

The Black speakers in our sample are from a wide range of geographic backgrounds, mostly college-educated speakers who are not speakers of the Black English Vernacular. Yet if we isolate their responses to (10) and (13), we find that they favor the NEG-NEG response which indicates a greater recognition of the possibility of using NEGCONCORD with pre-verbal positions in the following sentence. The difference is clear but certainly not categorical. All of these data on intuitive responses fit in with the implicational matrix formed from observations of speech (12). Even after subtracting the eight cases of Black speakers, 42 white speakers saw the possibility of using negative concord to pre-verbal position in a following clause.

We conclude that the matrix (11) and rule (9c') have a place in a pan-dialectal grammar in English. Not all the rows and columns of the
matrix are activated consciously for all members of the speech community; but we can argue that they are potentially present, and a moderate degree of exposure makes the rule (7c') usable for speakers of English. Note that those who are most remote from the Black community—the students in Utah and Kansas—show the lowest percentage of NEG-NEG responses. Whites from Eastern cities who have heard Black speech seem to have absorbed some predisposition to allow this extension of negative concord.

We have seen then that a Type III variable rule can also be a good candidate for a pan-dialectal grammar. Negative concord deals with relatively abstract grammatical operations which are comprehensible and natural operations for most English speakers to follow. The ability to extend one's own form of the NEGCONCORD rule to unfamiliar uses may be based on analogy with standard negative postposing, or with certain other positive-negative alternations to be discussed below. But first we will consider a feature of the negative in an English grammar which must be placed outside of the scope of a pan-dialectal grammar for the continental United States.

1. 3 Never in Hawaiian Creole English

In a preliminary investigation of the scope of grammar we have asked for an interpretation of the sentence

(14) He never like throw first.

The great majority of subjects, 28 out of 30, interpreted this as indicating that the person described did not in general, or never liked to throw the first punch. It was actually used to mean 'He did not want to throw first', as like means 'want' and never means 'did not' in Hawaiian Creole English [HCE]. There are an unlimited number of HCE examples which show in context uses of never which could not represent the English indeterminate. 6

(15) So she wen pass... she never know who was.
(16) And that thing was coming and something black on top the horse, never have head.

But this sense of 'did not' does not seem to be a possibility for outsiders who are not familiar with the Hawaiian form. It is rarely predicted or envisaged in subjective reactions, and when we look at the rule system, it becomes apparent why. To produce the preterite negative didn't in English dialects we need at least four rules: (a) placement of not in pre-verbal position after the first member of the auxiliary; (b) Do-support for the isolated tense marker; (c) A morphophonemic rule converting do + PAST into did; (d) NOT contraction. The outside English
speaker has no way of knowing that none of these rules exist in Hawaiian Creole English, and that instead we have the negative produced in an invariant position at the beginning of the verb phrase by phrase structure rules, and a simple rule which spells out the negative form:

\[(17) \text{NEG} \rightarrow \begin{cases} \text{never} / \_\_\_\_\text{PAST} \\ \text{no} \end{cases} \]

In our first tests, only five percent of our subjects reacted to (14) as a preterite. We then constructed a sentence which allowed the subjects to consider a real-life situation without focusing on grammar:

(18) A boy said, My father caught me once, so the next time I never lie.

What did he mean?_____

(a) Do you get the idea that he's going to get another chance to tell the truth?_____
(b) or he had another chance and told the truth?_____
(c) or he had another chance and lied again?_____

About 25% of the subjects in Q-SCOM-III took the second option (b) but when we examine their explanations of their choice, it is clear that very few of them endorsed the preterite meaning of never. To do so, one would have to imagine a process or route connecting didn't with never. Is there any general principle involved in the shift of the indeterminate to a preterite feature?

(19) \[
\left[+ \text{NEG} \atop + \text{QUANTIFIER} \atop + \text{TEMPORAL} \atop - \text{DEFINITE} \atop - \text{PARTITIVE}\right] \rightarrow \left[+ \text{NEG} \atop + \text{PAST}\right]
\]

Those familiar with other Creole dialects might be quicker to assimilate rule (19), since we also find such a use of never in Guyana. But the responses of our mainland speakers indicated that they have no inkling of a general principle which removes the generality of never and limits it to a single past action.

At first we thought that no such route existed, and judged that the HCE rule for never is outside of any reasonable pan-dialectal grammar we would write for mainland speakers. But in some of our later inquiries,
especially in the Midwest, we found that there were a significant number of subjects who chose (b) for question (18) and even in their overt responses interpreted never as a preterit. One man from Kansas explained that he had figured it out by analogy with I never either. It appears that this expression is equivalent to I didn’t either for many American speakers. When it is used as a response to I didn’t go to the party last night, I never either shows never operating as a preterit negative marker. When we realize how constricted English never can be, as compared to French jamais or Spanish nunca, its further limitation to a particular point in the past does not seem such an extraordinary step. We are still reluctant to include this possibility in a general grammar of English, but it would be going too far to say that there is no systematic route by which a speaker of mainland English could properly interpret (18).

Let us return for a moment to the situation in Hawaii. What kind of grammar would we write for standard speakers who live on Hawaii, and know HCE from many years of close contact? They understand He never like quite well. Do they then have larger pan-dialectal grammars than mainland speakers? If we allow each person’s experience to rewrite the rules, we will all have different grammars, and there will be no prospect of a findable boundary of a speech community.

Before we come to such a conclusion we must examine the kind of knowledge that the SE speakers on Hawaii have. It is obvious that they recognize never in its Creole use; that they evaluate it as ‘Pidgin’; that they understand it as ‘did not’. Some may be able to label it correctly. But the crucial question is whether or not they can predict allied and parallel uses. We have not found that non-Creole speakers living in Hawaii have this predictive control over never. One crucial structural question is: if never means ‘negative preterit’ then how does HCE express the idea of SE never or ‘universal temporal negation’. Even the most qualified outside experts are uncertain on this point, which leaves us in some doubt as to whether they have fully incorporated HCE rules into their own grammars.

The crucial issue for pan-dialectal grammars is not understanding or evaluation, but prediction. It turns out that the rule used by outsiders often differs in its shape, its place in the grammar, and its detailed structure from the insider’s rules. Other evidence indicates that a speaker’s basic vernacular is the most systematic form of language that he controls, and the superposed varieties learned later in life have a much coarser, less systematic character. Before considering other examples from the use of the negative in English, let us consider a very clear case which illustrates this fact.

1.4 Third singular -s in Black English

Our own studies of the BE vernacular have shown clearly that the third singular -s is not present in the basic vernacular, though all BE
speakers sometimes use \(-s\) on the verb. Five properties consistently demonstrate this absence of the feature in the vernacular: its very low frequency, absence of phonetic conditioning, wide individual variation, correlation with informal situations and extreme hypercorrection. All other studies of BE which have examined these points produce parallel findings (Fasold 1972, Wolfram 1969, Legum et al. 1971). In general, the only basis for subject-verb agreement in BEV is the limited differentiation of finite forms of be in the present. But if BEV speakers participated in a pan-dialectal grammar of English, they would be able to predict and control third singular \(-s\) more easily than they do.

At the same time, it is plain that even young Black children in the first and second grades can apply abilities (1) and (2) to verbal \(-s\). They recognize \(-s\) in walks as Standard English (1) and they evaluate it as a speech form supposed to be used in school (2). But the work of Jane Torrey shows that they have only partial understanding. Working with second graders in New York City, Torrey developed comprehension tests in which third singular \(-s\) was the only signal of the present vs. the past and of singular vs. plural. One photograph showed a man in the act of hitting a dog, and another a dog after having been hit by a man. Children were asked to point to the right picture when they heard (19a) or (19b):

\[
\begin{align*}
(19a) & \text{ The man hit the dog.} \\
(19b) & \text{ The man hits the dog.}
\end{align*}
\]

The Black second graders were very successful in this test, almost as much as with the plural. Table 4 shows the figures for spontaneous speech, as a pre-test and a post-test after a training period. In another test children saw pictures of one cat splashing in the water and of two cats splashing, and were asked to point to the right picture when they heard (20a) or (20b).

\[
\begin{align*}
(20a) & \text{ The cats splash.} \\
(20b) & \text{ The cat splashes.}
\end{align*}
\]

Since the plural \(-s\) on cat could not be heard, the children had to use third singular \(-s\) to distinguish singular from plural. As Table 4 shows, they did far worse than random, and showed almost no improvement on training. We must conclude that the third singular \(-s\) is introduced into their grammar by a very different kind of rule than in other dialects.

\[
\begin{align*}
(21) \quad \emptyset & \rightarrow <s>/[-\text{PAST}] \text{ Vb } \_\_\_/<\text{HIGH}>^7
\end{align*}
\]

This \(-s\) is variable, and is introduced without regard to a fixed position of the tense marker (He can gets hurt) or person (I walks away) or
TABLE 4. Interpretation of -s morphemes by Black second graders on picture-meaning test.

<table>
<thead>
<tr>
<th></th>
<th>Plural</th>
<th>Verb</th>
<th>Possessive</th>
<th>Copula</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Before training program</td>
<td>1.6</td>
<td>0.1</td>
<td>1.1</td>
<td>1.3</td>
</tr>
<tr>
<td>After training program</td>
<td>1.9</td>
<td>0.3</td>
<td>1.6</td>
<td>1.9</td>
</tr>
</tbody>
</table>

N = 27; Maximum score = 2.0

--from Torrey 1972

number (They goes home). But it is not used in the past. This form of the rule persists through pre-adolescent and adolescent speakers, and is also used by many adults (Labov, Cohen, Robins, and Lewis 1968:33). It is apparent that there are many rules of a superposed dialect which differ from the corresponding vernacular rule in the following ways:

1. A variable rule in the superposed variety may correspond to a categorical rule in the vernacular and vice-versa.
2. The superposed variety often has variable insertion of a grammatical form, while the vernacular has categorical insertion followed by variable deletion (with phonological conditioning).
3. The superposed variety lacks many of the environmental constraints of the vernacular.

To the extent that speakers from one speech community learn another vernacular as a superposed variety, we can expect such differences and it will therefore be very difficult to construct a single pan-dialectal grammar to represent the knowledge of speakers from both communities. In the case of negative concord, we did not see the same kind of problem as with Hawaiian never or SE third singular -s. SE speakers are of course familiar with various forms of negative concord, but it was clear that familiarity alone will not explain the understanding of Nobody didn't say. The processes of negative concord are familiar to SE speakers through the closely allied negative postposing, and most importantly, no new grammatical categories, or surface forms processes are introduced, and no old forms are assigned new meaning. In the case of third singular -s, the process of subject-verb agreement is new to BEV speakers, the category of third-singular position is not present in the grammar, and worst of all, an -s which means 'plural' is now assigned the meaning 'singular'. It should not be surprising, then, to find that Puerto Rican speakers of English have much less
trouble with third singular _-s_. In our studies of the Puerto Rican com-
munity, we find heavy influence of BEV on many speakers, so that
listeners regularly mistake them for Blacks; but in most cases, third
singular _-s_ is well represented in their grammar in a form close to
that of other NYC dialects. Spanish has of course no third singular _-s_,
but it does have strong subject-verb agreement, and _-s_ is used as a
mark of agreement in other positions.

1.5 The most remote _been_

There are a great many items which separate BEV from other dia-
lects, and we are currently investigating them in an effort to measure
the extent of this discontinuity. On the one hand, we have such as third
singular _-s_ which is absent from BEV but present in other dialects. On
the other hand, there are specific distinctions made in BEV which are
still not grasped by speakers of other dialects. These differences do
not depend so much on re-adjustments and extensions of syntactic ma-
chinery, as in negative concord, but rather on the coding of new gram-
matical distinctions into single morphemes which are thus shifted into
new roles. The invariant _be2_ with the meaning of 'general, habitual'
is the best example known, as in *You always be doing that*. Less well
known, and more obscure, is the use of stressed _been_ to mean 'remote
perfect'. In the Philadelphia Black community (22) is used to mean 'I
have known that for a long time'.

(22) I _been_ know that.

The _been_ involved here is formally indistinguishable from the present
perfect of _be_ in many cases, as in *He been told that*, which can be de-
derived by the deletion of _have_. But in constructions which cannot have
this interpretation, like (22), _been_ obviously has a different meaning.
This form is much more common in Philadelphia than other areas, and
it is possible that this meaning of 'to have been in a specific condition
for a long time' is not general throughout the larger BEV community.
But in any case it is certainly not known, recognized, or understood by
the surrounding white community, let alone predicted or used. Thus a
five-year-old boy from West Philadelphia simply could not communi-
cate with this form to a white psychologist who had been working in his
kindergarten for six months. After the psychologist introduced Samuel
to me, Samuel turned to the psychologist and said:

(23) Samuel: I _been_ know your name.
    Paul: What?
    Samuel: I _been_ know your name.
    Paul: You better _know_ my name?
    Samuel: I _been_ know your name.
Even after this exchange, the white adult did not know that he had failed to understand something. Here we have a discontinuity as absolute as any we have seen. Samuel’s utterance was as meaningless to Paul as *Anyone doesn’t sit there*. Since it was outside his grammar, it could not even be heard, and the sounds Samuel used had to be reinterpreted.

Of all the BEV features that we have tested, *been* is by far the most obscure to whites. In our preliminary investigation we asked:

(24) What would it mean to you if you asked someone, ‘Do you know my name’ and he answered

‘I been know how they call you.’

Responses to this question by 32 informants can be classified:

- present 17
- present perfect 7
- preterit 3
- emphatic present (I do know) 2
- remote present perfect 3

Only one of the three informants who understood the intended meaning was white. In Q–SCOM II, we construct situations in which the informant can look through the grammar to discrete choices in real world situations. Thus we ask

(24) Someone said, ‘Hey, nobody knows where St. James Place is around here’ and a man said, ‘Well, I been know that street.’ Do you think he meant:

(a) that he didn’t know at all
(b) or that he was about to tell you right away
(c) or that he would have to think for a minute and try to remember where it was?

Responses on this investigation so far show that white informants regularly pick meaning (c), that is, they interpret *been* as meaning ‘used to’.

Our next step in the investigation of *been* was to construct questions in which there was no contextual support for the correct meaning: that is, where only the subject’s knowledge of the use of the particular form determined his answer. In Q–SCOM–III we developed the series (25–28).

(25) Someone asked, *Is she married?* and someone answered

She *been* married. Do you get the idea that she is married now? Yes ___ No ___
A teacher said, *Do you know your number facts?* and a boy answered, *I been know them.* Do you get the idea that he’s all ready to take the test_____
he has to brush up on this stuff_____

So what do you think *been* means in *I been know them?*

used to know_____
knew right now_____
knew but can’t quite remember_____

have known for a long time_____

Have you ever heard any sentences like that?____
Where?_________________

Those familiar with the remote *been* will recognize the heavy stress on (25), and understand the right answer as yes, she is still married. But other dialects must interpret (25) as derived from a deleted have, and in the absence of a temporal limiter there is a strong implication that she is no longer married—that the condition is completed in the perfective sense of have + be + en. In a series of 51 subjects drawn from the Philadelphia area, 24 answered yes to (25). This included nine of the twelve black subjects, and fifteen of the 39 whites (Table 5).

TABLE 5. Responses to most remote *been* by black and white subjects.

<table>
<thead>
<tr>
<th>Responses to questions (25-27)</th>
<th>N</th>
<th>(25)-a</th>
<th>(25)-a</th>
<th>(25)-a</th>
<th>(25)-a</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Whites</strong></td>
<td>39</td>
<td>15</td>
<td>13</td>
<td>8</td>
<td></td>
</tr>
<tr>
<td><strong>Blacks</strong></td>
<td>12</td>
<td>9</td>
<td>9</td>
<td>9</td>
<td></td>
</tr>
</tbody>
</table>

When we examine responses to (26) and (27), we find that seven of the fifteen whites fail to answer in a way that shows a correct knowledge of *been*. In (26), we have a verb *know* which does not permit a possible derivation from *have known* with deleted have, since the -en is plainly missing. The correct answer to (27) is ‘have known for a long time’. The importance of the test for a correct labelling appears clearly here: of the fifteen whites, only two were eliminated by answering b to (26) but five were then dropped from the list of *been* knowers by wrong answers to (27).

Of the nine blacks, none showed wrong answers to (26) or (27). If we now return to the three blacks who answered (25) wrong, we find that they all answered (26) as a and (27) as d.
The eight remaining whites included five whose knowledge of been was plainly motivated by contact with blacks. In answer to (28) several were able to specify exactly where they had learned about been from blacks. These are the only white subjects who identified the form as black in (28). Of the remaining three whites, we find that one spent 18 months in the South, but did not identify any specific black influence, and the other two are exceptions to the rule: that blacks know the meaning of most remote been, but whites do not unless they have learned it from blacks directly.

A limited series of questions of this sort will always produce a certain percentage of chance responses which deviate from expectation. In that sense, the two or three white exceptions may not be showing competence with been. But answers to the best constructed formal questions are only approximations to the linguistic competence reflected in speech and natural interaction. In this case we isolate been from that social context, and we must not always expect that its use by blacks will lead to misunderstanding by whites as in example (23). The three white speakers may actually have deduced the meaning of been through their own grammatical resources. Nevertheless, the disjunction between blacks and whites on been is as sharp as anything we have seen in our investigation. We must emphasize again that many of our college-educated black informants have never been speakers of the vernacular, and do not use most remote been themselves. We could not have learned about it by listening to their speech. But there seems to be a general grammar available to most black speakers, derived from a common linguistic experience, which includes been in a pan-Black English grammar, but not a pan-English grammar.

The evidence therefore points heavily against the inclusion of this been in a pan-dialectal grammar of English. This feature must be excluded from any realistic grammar posited for whites. On the other hand, we have seen that speakers of the BEV do not have the same third singular -s rule as other dialects. This is one of many indications that a separate grammar will be required for the BEV and perhaps for Black English in general. Although the discontinuity between BEV and other mainland dialects is not as great as that between HCE and others, there are features of BEV which are not recognized, understood, evaluated, predicted, or used correctly by speakers of other dialects.

1.6 Anymore

We will now consider one of the most interesting and mysterious examples of divergence in English syntax. Syntactic change is an elusive process as compared to sound change; whereas we find sound changes in progress in every large city in the English-speaking world, we have comparatively little data on syntactic change. Phenomena which seem at first glance to represent change turn out to be continuations of
long-standing traditions not reported by grammarians. This may be the case with the use of anymore in positive sentences, which appears to be spreading outward from its center in the mid-West. In Q-S COM-I we asked our informants

(29) What would it mean if you asked Who plays ringalevio? and someone answered A lot of cats play that anymore.

When I first encountered this use of anymore I thought it meant 'still', and this is the impression that many outsiders have. We think of it as the opposite of A lot of cats don't play that anymore, i.e. 'they still play that'. But on further exposure, it quickly becomes apparent that anymore does not mean 'still' but rather 'nowadays'.

(30) That's the trouble with airplanes anymore.
     --retired railroad engineer,
     Champaign, Illinois

(31) Those secretaries write most of the letters anymore anyhow.
     --personnel manager, raised twenty miles north of
     Evansville, Indiana

Though we do not yet know the full geographic distribution of this form, it is used automatically and frequently in Pittsburgh, western Pennsylvania, Ohio, Indiana, and parts of Illinois, Kansas, Missouri, Utah, and other western areas, and it is apparently spreading to other parts of the United States. In general, positive anymore appears to be a Midland phenomenon and its incidence reported so far corresponds quite closely to the areas of Midland settlement and influence mapped by dialect geographers following Kurath 1939. It is not a social marker or stereotype, and is not evaluated by most speakers. I once asked a young woman clerk in Cleveland if it was true that people around there could say 'We go to the movies anymore'. She answered no, they didn't say that; they all said show. I have pressed the point with speakers from this area after they used positive anymore in actual conversation, and after a while they will become confused and say that they made a mistake, and should have used don't. It is important to realize how far below the level of conscious awareness this syntactic feature lies. In 1969, an issue of LIFE appeared with a featured headline on one story next to a photograph of the author of a recent best seller kneeling on her bed:

(33) What it takes to be a lady author anymore
I have been unable to trace any response or reaction to this as an odd utterance. In Q-SCOM-II we also asked for an interpretation of

(34) We live in Columbus anymore.

When we examine the pattern of reaction to (29) and (34) we find that many absorbed a negative meaning, some understood ‘still’, but the majority got some form of ‘nowadays’:

<table>
<thead>
<tr>
<th></th>
<th>(29)</th>
<th>(34)</th>
</tr>
</thead>
<tbody>
<tr>
<td>negative</td>
<td>7</td>
<td>4</td>
</tr>
<tr>
<td>still</td>
<td>9</td>
<td>4</td>
</tr>
<tr>
<td>unmarked present</td>
<td>-</td>
<td>9</td>
</tr>
<tr>
<td>used to</td>
<td>-</td>
<td>2</td>
</tr>
<tr>
<td>nowadays</td>
<td>19</td>
<td>15</td>
</tr>
</tbody>
</table>

Only 13 informants registered ‘nowadays’ for both sentences, but this was a much higher number than I expected. Even linguists have been deceived about the meaning of positive anymore when approaching it from outside. In case after case, linguists have told me that they were familiar with the anymore dialect. When I asked them what positive anymore means, the answer is usually ‘Still’. But when they are informed that the meaning is ‘nowadays’, they begin to recall that this was the way that they heard it used. If even a linguist can be so misguided, how can we explain this high response to anymore in our subjects? We did not draw them from the mid-West, but from a wide range of areas in the Eastern United States, reaching out for the basis of a pan-dialectal grammar from our Philadelphia location.

When we consider the geographic origins of the subjects, this puzzle is resolved. We can divide all informants into (+): those who showed some response of ‘nowadays’ to at least one sentence and did not contradict this meaning in the other; and (-): those who gave at least one ‘negative’ or ‘still’, showing that they did not understand positive anymore. The following list shows the geographic distribution of 33 informants from Q-SCOM-I according to their anymore responses, listing them under the area in which they lived from 4 to 13 years old.
<table>
<thead>
<tr>
<th>Mid-West</th>
<th>Eastern Pa. and Philadelphia area</th>
<th>Other areas</th>
</tr>
</thead>
<tbody>
<tr>
<td>(+)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Fla/Pittsburgh</td>
<td>Philadelphia</td>
<td>NYC</td>
</tr>
<tr>
<td>Wisconsin</td>
<td>Philadelphia</td>
<td></td>
</tr>
<tr>
<td>Ohio</td>
<td>Philadelphia</td>
<td></td>
</tr>
<tr>
<td>Iowa</td>
<td>Philadelphia</td>
<td></td>
</tr>
<tr>
<td>W. Va/Pittsburgh</td>
<td>Philadelphia</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Berwyn</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Bryn Mawr</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Cherry Hill</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Cherry Hill</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Cherry Hill</td>
<td></td>
</tr>
</tbody>
</table>

| (-)       |                                  |            |
|          | Philadelphia      | New England|
|          | Philadelphia        | Massachusetts|
|          | Philadelphia        | Massachusetts|
|          | Hazeltown            | Mass/Fla   |
|          | Altoona              | Upstate New York |
|          | Cherry Hill         | NYC        |
|          | Glassboro           | Atlantic City, N. J. |
|          |                      | Maryland   |
|          |                      | Los Angeles|

The pattern now becomes clear, including the one exception from New York City. This young woman of 17 moved into Freeland, Pa., two years ago, and apparently noticed Pennsylvania speakers using positive anymore as 'nowadays': she calls it 'a fad'. Otherwise, Easterners, Southerners, and even Westerners fail to recognize the meaning of positive anymore. Philadelphia is divided. On the other hand, all the mid-Western speakers naturally and without comment identified the meaning and their only concern was with lexical items like cats in (29) which drew their attention.

In our further investigations of anymore, we began again with a sentence which provides no contextual clues except the meaning of the sentence itself.

(36) Someone said, John smokes anymore. Do you get the idea that

a. John hasn't been able to kick the habit____

b. or John has quit____

c. or John wasn't smoking for a while but now he is____
This sentence strikes many speakers of the anymore dialects as odd or ungrammatical at first, because it lacks the normal contextual support as in John is always smoking anymore, or John smokes a lot anymore (see below). In fact, a very large number of basic anymore speakers from heartland areas such as Kansas reacted to John smokes anymore as 'no native speaker would say it' when they first encountered it in our grammaticality test. But when they considered it in the context (36), they unhesitatingly gave the right interpretation. Furthermore, they gave the correct response to (37) and (38) which indicate that they are quite capable of using and interpreting anymore in (36):

(37) So what would you guess that anymore means in John smokes anymore?  
   a. [still] _____  
   b. [nowadays] _____  
   c. [negative] _____  

(38) Have you ever heard anything like that?_____  Where?______
               ___________  Could you say it yourself?______

In our Kansas sample of 25 subjects, eleven answered (38) by saying that they could say John smokes anymore themselves. But six of these responded to the isolated sentence in our grammaticality test by saying that 'no native speaker would say it', four checked off 'someone might but I've never heard it', and only one indicated that it was perfectly natural. This response is one of the most dramatic demonstrations of the difficulty of interpreting judgments of isolated sentences on a typical grammaticality test: if the investigator does not determine the context, various uncontrolled factors will do so for him, producing such odd and misleading responses. The basic situation here is that a sentence in which anymore carries the main grammatical meaning without support is rare, but in a slightly larger context, quite natural for the grammar to produce.

In further work on the matter, we introduced a second anymore sentence directly after (36)

(39) Someone said Harry likes rock music anymore. Do you get
       the idea that
       a. Harry's turned off rock_____  
       b. or Harry's always been a great rock fan_____  
       c. or he's finally seen the light?_____  

A correct pattern of response to anymore sentences is then (36)-c, (39)-c, (37)-b. Table 6 lists the over-all patterns of responses from
TABLE 6. Geographic distribution of subjects interpreting positive anymore as ‘nowadays’ in questions (36-39) (No. correct/total responses).

<table>
<thead>
<tr>
<th>Geographical background of subjects (4-12 yrs.)</th>
<th>Midwest</th>
<th>Phila. region</th>
<th>South</th>
<th>North and East</th>
</tr>
</thead>
<tbody>
<tr>
<td>Q-SCOM I (Phila)</td>
<td>5/5</td>
<td>11/18</td>
<td>0/9</td>
<td></td>
</tr>
<tr>
<td>Q-SCOM II (Phila)</td>
<td>2/2</td>
<td>6/9</td>
<td>0/7</td>
<td></td>
</tr>
<tr>
<td>Q-SCOM III (Phila)</td>
<td>6/9</td>
<td>10/17</td>
<td>2/10</td>
<td>6/45</td>
</tr>
<tr>
<td>Q-SCOM III (Kansas)</td>
<td>10/11</td>
<td></td>
<td>1/4</td>
<td>3/4</td>
</tr>
<tr>
<td>Q-SCOM III (R.I.)</td>
<td></td>
<td>1/1</td>
<td>1/23</td>
<td></td>
</tr>
<tr>
<td>Q-SCOM III (Utah)</td>
<td>8/10</td>
<td></td>
<td>0/1</td>
<td>0/3</td>
</tr>
</tbody>
</table>

170 subjects according to their geographic origin. A correct interpretation of anymore is a pattern of ccb, or corresponding answers in the earlier studies.

Again the pattern which emerges is quite clear. In Philadelphia we get a uniformly Midwestern response except for three speakers interviewed in Q-SCOM-III. In the Utah and Kansas samples, there are only one or two Midwest residents who have failed to grasp anymore. In the South, including West Virginia and Maryland, and in the East, we get only a small percentage of those who understand this form: the great majority interpret anymore as negative or ‘still’. The largest number of exceptions from Eastern states are those who have been in Philadelphia and have been exposed to the pattern there. For the Philadelphia area is plainly different from any other region: in every subsample we find it split with about sixty percent of the subjects understanding anymore, and the rest failing to do so. Unlike the Midwesterners, many Philadelphians say that they have never heard the form.

But why is the Philadelphia area split? Does this represent differential familiarity with anymore or different abilities to decipher an unfamiliar construction by general principles? This is a crucial question for our study of the outer limits of a grammar. For if two-thirds of the Philadelphians can calculate the meaning of an unfamiliar rule by extrapolation from their own rules, positive anymore might indeed be a candidate for a pan-dialectal grammar.

We cannot be sure that no Philadelphian ever deciphers positive anymore from first principles. But there is circumstantial evidence that most of these informants had heard anymore used in positive sentences many times, undoubtedly without realizing it. For we have had many reports of such sentences in the Philadelphia area:
(40) It's the same thing to me anymore. It's the same thing every year.

--Piano tuner, 70, Philadelphia

This sentence was written down immediately and preserves accurately the use of anymore to mean 'nowadays'. The speaker was explaining why he did not watch the Mummers' Parade nowadays though he used to watch it all the time when he was young. We have other reports of anymore being used in Philadelphia; anymore is all about us, under the surface, but is not available for conscious judgments of grammaticality.

At first glance, the positive use of anymore seems to be a simple extension of an isolated lexical item; in effect, a new synonym for 'nowadays', 'these days', 'lately', etc. But when we begin to analyze the different interpretations presented by our subjects, it becomes apparent that we are dealing with deeper principles of discourse and semantics. Let us first consider the meaning of anymore in negative sentences as in John doesn't smoke anymore.

(41) Past Present
x is the case
x doesn't any more
x is not the case

I will introduce the term 'head' to indicate the assertion made by NEG + anymore about the present: 'that he does not smoke now'. In addition there is a 'tail' indicating what was the case in the past: 'that he did'. If the head is an assertion, is the tail also? Or is it a pre-supposition, an implication, or consequence? Does John doesn't smoke anymore presuppose that he used to smoke, imply it, or assert it? One test for presuppositions is to see what happens on negation, since by definition pre-suppositions are not affected by negation. But we cannot simply reverse doesn't to does; it is generally considered that we must also change anymore to still when we do so. In this case, we find that the tail is unaffected, and we must conclude that it is a pre-supposition of NEG + anymore.

(42) Past Present
x is the case
x doesn't any more
x is not the case

That is, still and anymore are opposed only in their heads, not in their tails. But when we turn to the sentence John smokes anymore as
understood in the mid-West, we find that both head and tail are reversed:

\[
\begin{array}{c|c|c}
  & \text{Past} & \text{Present} \\
\hline
x \text{ is the case} & \text{NEG + anymore} & \text{POS + anymore} \\
\end{array}
\]

Thus we see that anymore is more than a new lexical item. It represents the filling of a new grammatical category which did not exist before except in lexical form: but it involves the implicit restructuring of \text{NEG + anymore} so that the tail is no longer a presupposition but an implication or a simultaneous assertion with the head. This now means that still and anymore are not polar opposites as in other dialects.

Given the abstract character of this semantic shift, it is no wonder that speakers outside the anymore dialect area ('outsiders') are not able to decipher anymore correctly. Although our first results made it seem possible that anymore was a good candidate for a pan-dialectal grammar, it now seems as if there are indeed two separate dialects with different understandings of the meanings of anymore. The development of positive anymore is not the filling of an empty hole, which might be done by the extrapolating and generalizing ability of the native speaker; instead, an understanding of anymore requires actual contact with the new use and new attitude towards the discourse structure of anymore.

Outsiders persistently interpret the tail of positive anymore as a pre-supposition. The fact that this presuppositional analysis is well below the surface may be responsible for the strong re-coding effect. In each area that we have investigated, we find linguists who have regularly encountered anymore in natural speech and interpreted it wrongly through their own framework. While they may have interpreted it correctly at first, the re-coding mechanism seems to produce the wrong result again and again. There seems to be no connection between linguistic insight and the correct perception of anymore. One Northeastern linguist was quite familiar with anymore: he had learned about it from a 'Pennsylvania Dutch' speaker. He said that it meant 'still'; when we reminded him that it meant 'nowadays', he said, 'Yes, that was the way he used it.' A graduate student was quite familiar with anymore: she had almost married someone from Oberlin who had used it, and it had irritated her very much. It meant 'still', she said. When we reminded her that it meant 'nowadays' she said, 'Yes, that was the way he used it.' A linguist from one of the Midwestern areas was present in a group where we were using Q-SCOM-III: he was the only person present not raised in the Midwest, though he had lived there himself for several decades. In the discussion that followed, he disagreed with our analysis
by pointing out that *anymore* meant ‘still’. The native mid-Westerners contradicted him, identifying it as ‘nowadays’.

This recurrent pattern of sophisticated but erroneous responses demonstrates more clearly than anything else that positive *anymore* is not part of a pan-dialectal grammar. It is true that linguistic tradition has turned attention away from meaning towards distributional questions. But if linguists do not have built into their grammars a mechanism for properly interpreting positive *anymore*, we can conclude that it does not belong in any general grammar for English speakers. We are dealing with an irreducible difference between two grammars.

But we do not mean to imply that there is no transition between the two. Many people first react to a discussion of positive *anymore* as if it were an interesting example of lexical change, but no more than a synonym for ‘nowadays’. This view dissolves as we begin to probe more deeply into other changes in the quantifier system which accompany this shift. I cannot do more than sketch lightly here some of the issues involved and the areas of investigation, but I will try to indicate the general directions of our current research in Philadelphia and its environs.

First we note that there is a cline of *anymore* possibilities for all dialects. In Q-SCOM-III we follow (38) with a question which asks the subject to predict parallel uses of *anymore*:

(44) *Anymore* actually means ‘nowadays’ in these sentences, the way it’s used in the mid-West. Would you guess that if someone says *John smokes anymore*, meaning ‘John smokes nowadays’, that he would also say

a. That’s the trouble with airplanes anymore. 

b. Anymore I don’t go there. 

c. Anymore football is more popular than baseball. 

d. Anymore you’re talking! 

e. It’s so hard for him to walk across the room anymore. 

f. Where is he anymore? 

g. That’s impossible anymore. 

h. It’s hard to do that anymore. 

i. It’s easy to do that anymore. 

j. When would you rather live, in 1920 or anymore? 

k. When was the best beer brewed? Anymore. 

l. Secretaries write most of the letters anymore anyhow. 

It is clear that *anymore* is not just a simple synonym for *nowadays*, since speakers everywhere laugh at j and reject both j and k. There is an implicational scale of *anymore* used which may be shown as
(45) a. It's impossible to do that anymore.
b. It's so hard to do that anymore.
c. It's hard to do that anymore.
d. It's easy to do that anymore.

As the strength of the negative presupposition falls off, the use of anymore becomes less likely. For outsiders, (45a) is perfectly acceptable; (45b) can be questionable (though we have observed it in actual speech from a New Haven resident), and (45c, d) are out of the question. It is interesting to note that mid-Western speakers who use all four naturally seem to find it very difficult to know which of these forms would be used by Easterners, but we are only beginning to explore this converse view of the matter.

The factivity dimension is a very sensitive one here, for (44g) is generally much less acceptable to outsiders than (45a). The presence of the non-factive for-to complementizer seems to strengthen negative presupposition. If the negative presupposition is strong enough lexically, we can begin to observe the use of positive any even beyond the anymore construction. Question (46) is based on a sentence spoken to me by a Philadelphia druggist:

(46) Someone said, 'These razor blades are going like hot cakes. I hope there's any left.' What would he mean?

One of the most striking things about (46) is that in the course of our investigations, not one speaker failed to interpret it correctly. There is no tendency whatsoever to supply a negative meaning to any after hope. We therefore observe that the movement of any into positive contexts is not limited to anymore.

Finally, we may point out the dramatic implications of (44b) for another rule of English grammar. To the extent that one recognizes a connection between any and anymore, it appears that this sentence violates the categorical rule of negative attraction discussed in Section 1 above. It is therefore not surprising to find that this is the most limited context for the application of positive anymore. Table 7 shows the implicational scale which proceeds from our Kansas exploration of anymore. Each anymore subject is identified by a triplet such as ccb indicating his pattern of response to the basic questions (36-39). The + or – following the triplet indicates whether or not the informant stated that he would say John smokes anymore himself. Thus in the basic pattern we see three ccb+ speakers and one ccb- subject; differing from them on only one point, there are five other ccb+ subjects. Next is a group of five informants who differ on two points, including two cbc- types, and 3 whose responses showed no resemblance to the native anymore pattern on (36-39), listed as xxx-.
TABLE 7. Predictions of positive *anymore* in environments of Question (44)

<table>
<thead>
<tr>
<th>Subject code: answers to</th>
<th>(36)</th>
<th>(39)</th>
<th>(37)</th>
<th>(38)</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>c</td>
<td>c</td>
<td>b</td>
<td>+</td>
</tr>
<tr>
<td>2</td>
<td>c</td>
<td>c</td>
<td>b</td>
<td>-</td>
</tr>
<tr>
<td>3</td>
<td>c</td>
<td>b</td>
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<td>-</td>
</tr>
<tr>
<td>4</td>
<td>x</td>
<td>x</td>
<td>x</td>
<td>-</td>
</tr>
</tbody>
</table>

Sub-cases of (44): Predicted that anymore speakers would say

<table>
<thead>
<tr>
<th>Subject</th>
<th>e</th>
<th>h</th>
<th>a</th>
<th>g</th>
<th>i</th>
<th>c</th>
<th>l</th>
<th>b</th>
<th>f</th>
<th>k</th>
<th>j</th>
<th>d</th>
</tr>
</thead>
<tbody>
<tr>
<td>1, 1, 1, 2</td>
<td>1</td>
<td>1</td>
<td>1</td>
<td>1</td>
<td>1</td>
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<td>0</td>
<td>0</td>
</tr>
<tr>
<td>1, 1</td>
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among the first ten in this matrix, but several are listed at this lower level. Conversely, as we look further down the matrix, we find only one ccb+ subject at a lower level (see below).

At the bottom of the implicational scale we find subjects who predict only (44e, h) and reject all other forms. In the intervening area, there is a set of five subjects who generally do not accept the foregrounding of anymore in (44c, b) or the use of anymore in WH- questions (44f). This includes one ccb+ type and two ccb-, so this pattern seems to have some claim to native status.

We have only begun to explore the linguistic implications of a matrix such as Table 7, and to study the intermediate stages in the marginal Philadelphia area. But the data presented here should be sufficient to indicate that we are dealing with deep-seated differences in the section
of the grammar that deals with quantifier systems. The difference between the anymore grammar and the outsiders appears in Table 7 as a gradual expansion of the conditions under which anymore is predicted in positive sentences. Yet the strong re-coding process in sophisticated speakers, and the sharp cleavage in reactions to (36-39) makes it doubtful if we can justify the inclusion of the positive anymore dialect in a pan-dialectal grammar. Table 7 is an overview of a number of sub-grammars. But it is not similar to the matrix for negative concord (12), since those at the foot of Table 7 seem to have no view of those at the head. Further investigations may modify our view, but the weight of current evidence says that in the grammar of Easterners and other outsiders only the strong presupposition of a negative outcome will allow anymore in a positive sentence. At this point their grammar comes to a stop, because further extensions seem to demand a new analysis of anymore which challenges and defeats their linguistic imagination.

2. Inner limits

Though the competence of native English speakers ranges far beyond their own use, and beyond their own speech community, we have seen that there are limits to its reach. Our grammars must have subdivisions, not just diacritics for different dialects, because there are some rules in my grammar that you cannot predict, and outputs that you cannot understand. Furthermore, we have seen that superposed forms may be governed by rules quite different from the same forms in the vernacular. Given this situation, let us now consider the inner limits of a grammar. What are the minimal units of grammatical description: where must grammar stop?

We do not expect that these minimal units will be invariant. But neither are we willing to accept the superficial variation that we first encounter. Our job as linguists is to reduce, eliminate or simplify that variation as far as we can by rule. Some of these rules will be invariant, and others will be optional. Our work is finished when we have written the most specific rule we can that constrains as tightly as possible the language of a given group. Such rules often combine a number of parallel sub-cases, some invariant, others variable. These maximally specific rules are one kind of minimal unit. They will apply to a group of speakers who use, interpret, and evaluate them in a relatively uniform way. There will of course be some differences among them, but grammar must stop at some point, and declare (perhaps for the moment) that those differences are not grammatically significant.

There remain two questions on where this process of grammatical description ends: on the size of the group, and the scope of the rule.

(1) We know that some speakers have the same rule. But are there groups of more than one speaker who share a set of many rules, so
that it is worth our while to write a minimal grammar for that group?

(2) We know that some environments are optional. Can we establish any consistent order among these variable sub-cases, so that some apply more often than others?

The standard answers to both questions (as seen in both structuralist and generative traditions) are no. The minimal object of description is to be the group of one, or the idiolect, and within that idiolect, free variation is not to be constrained. Rules will apply always, optionally, or not at all, and it is not permitted to say that a rule applies more often in one environment than another. In combination, these two positions construct an object of grammatical description that I will call the ‘freely varying idiolect’ or ‘free idiolect’.

In ‘Empirical foundations for a theory of language change’ (Weinreich, Herzog, and Labov 1968) we traced the recent history of this free idiolect (from Paul to Bloomfield, Bloch and Chomsky) as it retreated further to become the speech of one person talking to one other on one topic for a short time. It can instantly be seen that the idiolect represents a defeat for the Saussurian notion of langue as a social product, the possession of the speech community.

In practice, linguistic description demands a broader object than the free idiolect. No one can write a grammar based on such small bits of speech, and linguistics leaks past the idiolect in many shapes and forms. The current solution is to take as input to grammar the intuitions of the theorist, free of any context. Originally, it was hoped that such judgments would be quite uniform, and that the major problems of variation would disappear into ‘performance’. It now appears that intuitions are even more variable than behavior; but strangely enough, all such variations in intuitions are taken to be linguistically significant. This has required the construction of a new entity, known as ‘my dialect’, representing all the points on which one theorist’s intuitions differ from another’s.

These are psychological reactions of some interest. But from a linguistic point of view, such idiosyncratic ‘dialects’ have a very different status from the syntactic dialects which reflect regional or social diversity. Included in the Q-SCOM series are experiments which show how the eliciting context controls intuitive judgments on quantifiers and negatives, as in differing reactions to All the circles don’t have dots in them (‘not all have’ vs. ‘none have’). We have come to the tentative conclusion that individual variations in reactions to such sentences are products of presupposed, imagined or constructed context. These differences are much less stable than the syntactic dialects considered in Part I of this paper; they might appear as variable constraints on interpretation, open to all speakers (Carden 1972). In this paper, I will not consider such constructs further, but return to the study of grammars based on linguistic rules that are actually used, interpreted, and evaluated by native speakers.
Turning to empirical studies of language in use, we find that the earliest studies treated each linguistic variable as a separate unit and each group as a collection of individuals. The classic study of Gauchat in 1899 (1905) had this character; Fischer's observations of (ing) in 1958; my own studies in Martha's Vineyard in 1963 and the phonological variables of New York City in 1966; Shuy's work in Detroit, Trudgill's recent study of Norwich and others. Correlations between linguistic variables are observed and charted in these studies, but are not combined into a single formal rule or ordered series of rules. Occasional observations were made of social interaction, but the individuals who were studied were relatively isolated from their social context in face-to-face interviews. In the early chapters of the New York City study (Labov 1966) I portrayed a number of individuals in great detail, and discussed their similarities and differences; one could not help but observe the extraordinary similarity of a whole set of working-class Italian women, or lower middle-class Jewish women. The final presentation showed the average values of the variables for these groups. The complex pattern of stylistic and social stratification can be presented in a single rule (Labov 1970) and it is an interesting question as to whether such diagrams and rules are part of the grammar in the sense I have used here: that is, whether speakers can use this relation in identifying social status and stylistic level in every-day life.

The work that we began in South Central Harlem in 1966 took a different direction in both respects. Influenced by Gumperz' work in Hemnes (1964), we began to work directly with groups in long-term participant-observation (Labov, Cohen, Robins, and Lewis 1968:2). In our group sessions, each individual was recorded on a separate track, but interacting with his peers rather than outsiders. We also gathered individual interviews with each member and lames as well, and conducted a survey of 100 adults in the community. At the same time, we began to dissect and re-assemble the linguistic variables in much more complex ways. We found a great deal of order among the environmental sub-classes. The three cases examined in the greatest detail were negative concord (Labov to appear) contraction and deletion of the copula (Labov 1969), and simplification of clusters ending in -t, -d. In its basic form, t, d deletion gives us the clearest and most general illustration of the principles involved.

\[(47) \ t, d \rightarrow \langle \emptyset \rangle \ / \ [+\text{cons}] \ \langle \emptyset \rangle \ 
\]

In this simple form, the rule states that t or d is deleted variably at the ends of words, more often if the next word does not begin with a vowel, and more often if there is no grammatical boundary between it and the preceding segments. The angled brackets around the zero output of the rule indicate that it is variable, and the angled brackets in the environment show variable constraints which favor the operation of the rule. In
this form, suggested to me by Bruce Fraser, the variable constraints are not ordered, and the rule applies not only to the BE vernacular, but to almost all speakers of English. (For some very formal speakers, the grammatical boundary is categorically absent.) The grammar of t, d deletion does not stop with this rule, but goes much deeper and variation can be even more tightly constrained. But first I would like to consider some basic questions raised by Derek Bickerton, who has entered this discussion from a very different direction. Bickerton has carried out an important series of analyses of variations in the Guayanese Creole continuum which have led him to a somewhat different point of view than the one I have just presented. His article in Foundations of language (1971) is an excellent model of the principle which I began with: we must not lightly accept surface variation, but reduce it to the smallest confines that we can, always searching to find more invariance. He shows that a poor choice of variable constraints for the alternation of complementizers _tu_ and _fu_ can leave us with more variation than another set of choices. But then he also argues that his example of a bad analysis uncorrected by a better one may account for the inherent variation reported by myself, Shuy, Wolfram, and Riley (1967); Wolfram (1969, 1972); Fasold (1966); Legum and his associates (1971); Ma and Herasimchuk (1971); Cook (1969); Mitchell-Kernan (1969); Anshen (1969); Trudgill (1971); Cedergren (1970); Sankoff and Cedergren (1971), and many others. His conviction stems from his acceptance of the traditional view that free variation cannot in principle be constrained, taking up DeCamp's notion that the +, +, o machinery is adequate for our purpose (1971). He also questions the existence of dialects spoken by a relatively uniform group. We are in profound agreement with Bickerton on the general program for the study of languages and context. But in effect, Bickerton urges us to return to the study of individuals rather than groups and to dissolve our rules into separate columns for each environment: that is, return to the free idiolect. These are important questions, and deserve a careful answer.

Bickerton's own approach is given careful statistical examination in the paper to be given by Sankoff at the Texas conference (1972), but I will confine myself here to his defense of the freely varying idiolect and the questions that he has raised about the main body of work done here and in England.

The t, d deletion rule was first presented in the 3091 Report (Labov, Cohen, and Robins 1965), and has since been the subject of further inquiry by Wolfram (1969), Fasold (1966), Legum (1971), and others, who all find the same general picture as shown in rule (47). This rule was the subject of a penetrating analysis by Shuy and Fasold at the last Georgetown Round Table (1971). Let us first see how consistent the members of primary groups are in their use of the t, d deletion rule.

Table 2-6 of our Final Report 3288 (Labov, Cohen, Robins, and Lewis
TABLE 8. Proportion of -t, d deletion in clusters for eleven members of the Jets in single interviews

<table>
<thead>
<tr>
<th></th>
<th>Monomorphemic ((KD_{mm}))</th>
<th>Past tense ((KD_{p}))</th>
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<tbody>
<tr>
<td></td>
<td>before consonant</td>
<td>before vowel</td>
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<tr>
<td>1. Stanley</td>
<td>19/20</td>
<td>7/10</td>
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<td>2. Rednall</td>
<td>25/26</td>
<td>5/9</td>
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<td>3. Hop</td>
<td>18/21</td>
<td>4/9</td>
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<td>4. Larry</td>
<td>36/38</td>
<td>2/8</td>
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<tr>
<td>5. Vaughn</td>
<td>35/42</td>
<td>4/11</td>
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<tr>
<td>6. Doug</td>
<td>28/30</td>
<td>4/8</td>
</tr>
<tr>
<td>7. Tyler</td>
<td>16/17</td>
<td>4/7</td>
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<tr>
<td>8. Its</td>
<td>9/15</td>
<td>(1/1)</td>
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<tr>
<td>9. Stevie</td>
<td>21/21</td>
<td>2/4</td>
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<td>10. Turkey</td>
<td>11/13</td>
<td>(0/1)</td>
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<tr>
<td>11. Rip</td>
<td>11/12</td>
<td>1/2</td>
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<tr>
<td>Total:</td>
<td>229/255</td>
<td>34/70</td>
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<tr>
<td>% -t, d Deletion:</td>
<td>90</td>
<td>49</td>
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1968) has been cited by Bickerton (1971) as showing great individual variation, but unfortunately he did not print the table. I have reproduced Table 2-6 here as Table 8. One can hardly account for the regularity of this table by invoking such 'performance' factors as length of utterance, difficulty of articulation, and so on. For each individual we see that more clusters are simplified before a following consonant than a following vowel, and at the same time, more clusters are simplified in monomorphemic forms than where the -t, d represents the past tense.

Thus we have four inequalities:

\[
\begin{align*}
a & : 1 > 2 \\
b & : 3 > 4 \\
c & : 1 > 3 \\
d & : 2 > 4 \\
\end{align*}
\]

These four relationships are universal and binding on each individual, except in the trivial cases where there is only one member in a cell. One might argue that an articulatory difficulty is responsible for the consonant/vowel effect, and that this is a typical 'performance' factor independent of the grammatical rules; however, the same environment
produces the reverse effect on clusters ending in third singular \( z \)--so that we are plainly dealing with linguistic rules specific to NNE. Furthermore, there is no way that the effect of the past-tense morpheme boundary can be construed as a simple performance effect--it obviously shows the speaker's knowledge of the morphemic status of \(-ed\) in the lower frequency of simplification for past-tense clusters.

There has of course been a great deal of confusion about numbers; just as in the New York City study (Labov 1966), we are looking at a regular structure of relations of greater than and less than. Numbers must of course be understood. To say, as Bickerton did, that Column 1 ranges from 60 to 100% is quite misleading. Those familiar with the analysis of variance will realize that Column 1 is extraordinarily consistent, with one item at 60% and the rest clustering tightly around 95%. Similarly Columns 2 and 3 show that even in very small numbers the cells are only one or two tokens away from the mean. But by looking at the variation in Table 8, one entirely misses the point: the basic relations here are invariant. The rule is actually an excellent candidate for a pan-linguistic grammar; we can generalize (47) to read

\[
(48) \ [+\text{cons}] \rightarrow \text{<Ø> / <+cons> <Ø> ##<~V>}
\]

In this form, the rule reads that whenever a final consonant is variably deleted, it will happen more often when it follows another consonant, when it does not precede a vowel, and when it is not a separate grammatical morpheme. In this form, the rule applies to Spanish aspiration of \( h \), French loss of final stops, deletion of \( r \) and \( l \) in various languages, etc. The general rule may indeed have escalated out of our grammar into a meta-grammar (Labov 1972).

Nevertheless, we must use the \( t, d \) rule in our BE vernacular grammar, because there is more detailed structure which would not appear in a pan-linguistic or even pan-dialectal grammar. The weighting of the variable constraints is not general—in fact, it varies from group to group, and from style to style, and from one age level to another in a meaningful manner. This weighting of the constraints would be expressed by the relations of Columns 2 and 3 of Table 8, which are the mixed cases. If the grammatical constraint is more important, Column 3 will be held down and Column 2 will be higher; if the phonological constraint is more important, 2 will be higher than 3. Figure 1 maps the distribution of Columns 2 and 3 for the eight members of the Jets for whom we have full enough information. It also shows six members of the Cobras in the same context. One Cobra lies in the Jet territory; the rest are in a completely different area of Figure 1. We see that the Cobras delete much more often in past tense clusters than the Jets. In our analysis of the linguistic correlates of peer group membership we dissect the groups and show that the most consistent form of the rules are to be found among core members, next, secondary members,
peripheral members and finally lames (Labov, Cohen, Robins and Lewis 1968:2.4). In Figure 1 we see dramatic differences between groups who are only three blocks away from each other. Figure 2 generalizes this difference in a broader context, showing the average values for Jets and Cobras, Thunderbirds and working-class adults. The Cobras and Thunderbirds are from the same neighborhood, and both shift from group style to individual style in the same direction: for all groups, deletion of grammatical clusters declines as we move from group sessions to individual interviews, but for the younger Cobras and Thunderbirds, deletion of monomorphemic clusters before a vowel rises.

Figure 2 shows mean values for t, d deletion in two styles for a number of groups. In general, the Jets are very close to the average of the working-class adults and the older Oscar Brothers, in both position and direction of shift. The Lames (isolated individuals) are distinguished by a relatively strong grammatical constraint. On the lower left corner of Figure 2 we see the values for a number of adults from other dialect areas. Some Southerners are very close to the Jets: KG is from St. Louis, and HG and MG are husband and wife from East Atlanta; and JS is from West Virginia. Moving further away we see HS from Columbus; BS and WS are grandson and son from Sonora, West Texas, and TW is from Lancaster, England.

These displays of variable constraints within the rule are of the greatest interest for a fine-grained study of dialect differentiation. If we were to discard this data in favor of +, ±, and −, Figures 1 and 2 would of course disappear immediately, and all values would be ±. Let us accept the suggestion to blur some of the data and accept 95% as equivalent to 1, and 5% as equal to zero. We then have Table 9. True enough, the uniformity of the group has disappeared. There are now
FIGURE 2. Percent deletion of cross-products in $-t, d$ deletion rule for BEV peer groups in group and single styles and other individuals.

Some writers have justified the blurring of linguistic data, reducing the facts of variation to a categorical approximation, on the grounds that children could not learn relations of more or less, and so variable rules have no 'psychological credibility' (Bickerton 1971). This is an interesting argument, because it illustrates the difference between the secular model of linguistic work that we are trying to put forward and the scholastic model that has been popular for some time. Within the scholastic model, someone may state that he knows that children cannot learn a certain kind of linguistic fact; therefore that fact does not exist, or is innate, or is universal. A secular linguistics is able to discover regularities that were obscured to others who held certain deductive principles; having observed the existence of these regular relations, we ask 'How do children learn such regularities?' This is one of the central questions of our current research.

Grammar does not stop with the form of the variable $t, d$ deletion rule that I have sketched above. We have been investigating a number of further regularities within the consonant cluster rule which are of considerable theoretical interest: for example, whether clusters in absolute final position behave like pre-consonantal or pre-vocalic clusters; the status of the ambiguous clusters in told, kept, lost, etc.; homovoiced vs. heterovoiced clusters; the deletion of single consonants, etc.; the ordering of the deletion rule. Our best current estimate of the maximally specific consonant cluster rule would include such features as
TABLE 9. -t, d deletion among the Jets without variable constraints.

<table>
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<tr>
<th></th>
<th>Monomorphic</th>
<th>Past tense</th>
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<tr>
<td></td>
<td>___K ___V</td>
<td>___K ___V</td>
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<tr>
<td>Stevie</td>
<td>+</td>
<td>+</td>
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<tr>
<td>Stan, Rednall</td>
<td>+</td>
<td>+</td>
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<td>Hop, Vaughn, Rip</td>
<td>±</td>
<td>±</td>
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<tr>
<td>Larry, Doug, Tylor</td>
<td>±</td>
<td>±</td>
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<tr>
<td>Turkey</td>
<td>±</td>
<td>1</td>
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</tbody>
</table>

\[(49) \ t, d \rightarrow <\emptyset> / \langle + \text{cons} \rangle \langle + \text{cont} \rangle \langle \emptyset \rangle \langle + \rangle \langle + \rangle \#\# \langle + \text{cons} \rangle \langle - \text{voc} \rangle \]

This form of the rule adds several more detailed constraints on the basis of our original data on BEV (Labov, Cohen, Robins and Lewis 1968:3.2), Shuy and Fasold's analysis (1971); and our more recent investigations of other dialects located on Figure 2. A preceding consonant favors the rule, as in (48), but more so if it is a continuant (-st, -ld vs. pt, -bd). The absence of a preceding boundary favors the rule as in (48) (past vs. pass#ed); but if a boundary is present, a derivational boundary + is favored over an inflectional boundary # (los + t vs. toss#ed). A following consonant favors the rule as in (48); but most of all if it is an obstruent [+ cons, - voc]; next if it is a sonorant [+ cons, + voc]; and least if it is a glide [- cons, - voc]. Some of these constraints are general, and may be candidates for a pan-dialectal grammar of a meta-grammar, and others are highly specific to particular dialects. Further exploration of these matters will throw light on sub-classification of grammatical boundaries, the nature of the syllable, the rise and fall of canonical forms.

This discussion has reviewed some familiar questions on the internal limits of a grammar. I think it has been helpful to examine once again the important issues raised here, which have indeed helped us discover some new facets of well-known rules. Finally, I would like to refer briefly to new and important developments in the mathematical treatment of variable rules, which raise the entire
discussion to a much higher level of precision and accountability.

Cedergren and Sankoff (to appear) have revised the formal interpretation proposed in Labov 1969, replacing the additive model with a product formula for the contribution of the variable constraints to the probability of the rule applying. They subject the basic hypothesis of the independence of the variable constraints to quantitative tests, predicting empirical tables of frequencies with a small number of parameters. Cedergren and Sankoff's model provides the first empirical demonstration that a complex rule schema is based on linguistic reality, for if they were not able to generate the empirical tables accurately, the rule schema would have to be rejected in favor of separate sub-rules. They demonstrate clearly that it would be a serious and unnecessary defeat for linguistic theory if one were to accept Bickerton's proposal that we dissolve rule schema into individual components, treat each individual as a separate case, and abandon any attempt to constrain free variation. The application of probability theory to variable rules by Cedergren and Sankoff represents a dramatic success for those who would treat the speech community as a coherent entity, and confront linguistic variation directly instead of adjusting the data to an older categorical model. At the same time, one must recognize that the implicational relations stressed by Bickerton are of the greatest importance, and do not depend on the categorical treatment that he has imposed on them. Our primary task as linguists is to search for invariance. To the extent that Bickerton can establish invariant relations in his treatment of the Guyanese pronoun and copula systems, his implicational series will carry force; but the linguistic significance of his findings will not be obscured where the finer analysis of variable rules is required.

In the light of the data presented in this section, together with Cedergren and Sankoff's analysis, I believe we have made some progress on the basic questions concerning the nature of variable rules. It should be possible to go ahead with many other questions on the form, shape, nature of grammatical rule and limits of grammatical competence. In that sense, the writing of grammar has no end, and it is clear that grammar does not, in the long run, ever come to a stop.

NOTES

1 The work reported in this section is the product of joint efforts by several research groups at the University of Pennsylvania. I am deeply indebted to Laura Dent, David Depue, Beatrice Lavandera, Arvilla Payne, Angela Rickford, John Rickford, and Malcah Yaeger for their contributions to the instruments used, the field work, and the analysis presented here. Mark Baltin helped in the development of a number of items through a separate project on the control of grammatical intuitions.

2 For a detailed discussion of this rule, see 'Negative attraction and negative concord in English grammars', to appear in Language.
See the article of Allen Grimshaw in this volume for a broader discussion of the rule typology referred to here.

At a presentation of an earlier version of 'Negative attraction and negative concord' before the Linguistic Society in 1968, Barbara Partee cited a sentence used by a young child which violated this rule. In a number of investigations of grammaticality, including the first part of the instrument used here, we have submitted sentences such as *Anybody didn't go there for grammaticality judgments. They are consistently rated very low, lower than any other quantifier-negative combination. But conscious judgment does not always reflect the categorical character of NEGATTRAC: some speakers think that some speakers might say it, though no one argues that they would say it themselves.

In the structural change for a variable rule, the variable element is enclosed in angled brackets. This notation is consistent with that used for variable phonological rules, such as the -t,d deletion rule discussed in the second half. For a formal interpretation of the variable notation, see Cedergren and Sankoff to appear.

The examples given here for Hawaiian Creole English are from interviews and participant-observation studies done by the author in 1970 in Hauula and Nanakuli, Oahu.

The exterior environment <HIGH> is a suggestion for one way of incorporating stylistic factors when most relevant to formal rules. By distinguishing inner environments from outer environments, we indicate the important difference between segmental and grammatical factors on the one hand, and stylistic and social factors on the other. Both are linguistic in the sense that they are deeply involved in the everyday act of communication; but the former are more tightly bound in a system of closed (or semi-closed) sets. <HIGH> is of course opposed to <-HIGH> or <LOW>, roughly equivalent to formal vs. casual. Such a notation implies the abandonment of Chomsky and Halle's use of inner and outer environments which is merely a graphic device.

I am indebted to John Rickford for pointing out the high incidence of stressed been in Philadelphia, its meaning as presented here, and its significance as a Creole feature of the BE vernacular.

This example illustrates the strong tendency of outsiders to re-code positive any more, and the unreliability of their information unless it is written down immediately. Teresa Labov has been able to elicit several spontaneous examples of positive any more from several Philadelphians, and was alert to the use of this form. She heard (40) and wrote it down immediately. At the end of the day she told me that she had observed someone say, 'We watch the Mummers' Day parade any more'. She had recoded any more to the meaning of 'negative', even though the example clearly shows that it means as always 'nowadays'.
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COMMENT ON PAPERS
PRESENTED AT THE FIRST PLENARY SESSION

CHARLES-JAMES N. BAILEY

Georgetown University

That I have seen only one of tonight's papers ahead of time puts me at a disadvantage in my role as a discutant of these talks. Besides, the subject itself is a problematic one: 'Sociolinguistics and the writing of grammars'.

I take it as obvious both that the social sciences and linguistics have a good deal to teach each other, and that neither can offer the other the models needed to exploit data from language variation for their differing ends. Sociology can tell linguists a lot about human interaction and other social phenomena that directly affect grammars; but it cannot and has not provided us with variable rules, conversational postulates, a wave model, sociolinguistic algorithms, etc., models that we need to achieve the goals of our discipline. It is also true that, while we lack the competence to provide sociolinguists with models for fulfilling their goals, we can provide them up-to-date tools for analyzing linguistic variation. If, instead of these, scholars in the social sciences employ static models whose definitions exclude patterned variation, they will fall into contradiction.\footnote{1}

One could supplement the observation of Mr. Bickerton tonight that there has been no stampede of sociologists into the linguistic corral with the further observation that there has been no stampede of linguists into the study of language variation. Even where important theoreticians have concluded that the function of language in its social context is one of the most important issues today, most of them, like the generative semanticists, have reached this conclusion without benefit of sociolinguistic influence; and their models for dealing with presupposition, implication, and the like owe nothing to the social sciences.
What is remarkable is that a convergence of diverse linguistic investigations has made it clear that there is a new view of grammar whose time has come. The concept of grammar is being broadened beyond its former narrow scope to include the investigation of—in Victor Yngve's phrase—how people communicate. This convergence may be seen in some of the most fertile work being done today: in the study of implicational patternings in natural phonology by David Stampe and Matthew Chen, in creole continuums by David DeCamp and Derek Bickerton, in time-based descriptions of lectal variation in English by William Labov and me, and in syntax by Elliott, Legum, and Thompson, as well as in the study of what Georgia Green and John Ross call 'squishes'. Moreover, Robin Lakoff, George Lakoff, David Gordon, James McCawley, Charles Fillmore, and others have demonstrated the effects of communicational situations on grammar. The convergence may also be seen in investigations of 'fuzzy'-semantic words referring to things as concrete as cup by William Labov and to concepts as relative as tall by George Lakoff. And implicational hierarchies of 'nouniness' have been observed by John Ross, by Eric Hamp, and by investigators of copula omission in creolized English.

While one might wish it otherwise, it seems clear that only because of these felicitous convergences is the work of variationists likely to have any impact on linguistics. One reason for this lies in most linguists' attributing to variationists the goals of sociolinguistics which are stated in writings on the subject by social scientists whose uses of language data naturally have goals that differ from our own. The emphasis on classification in these writings has contributed to the same result. And it must also be conceded that we variationists have not resolved the considerable differences and even confusion among ourselves over quite fundamental issues, like the relationship between relative quantities in linguistic data and the grammars internalized in language-users. Nor have the detailed attempts to explain linguistic change been matched with credible explanations of how the brain might internalize relative numbers. In addition, correlations have been explained in terms of direct causal relations without the causality of an additional factor having been convincingly ruled out.

Nevertheless, glottometry, for all its empirical emphasis on new ways of gathering unmonitored data, has still made greater contributions to language theory than it has to data recording and data analysis. Although the theoretical contributions would not have been possible without a good deal of data validity, the new day of data has yet to dawn. Phonetic transcription, for example, even among glottometrists, is as wretched as before, with few exceptions. This makes empirical studies far from adequate. Untenable conclusions about phonological processes have resulted from a confusion of unaccented [ɪ] with [ə], and studies of cluster simplification have jeopardized the validity of their findings by
apparently assuming consonant clusters at the end of *crank* [ˈkræŋk], *work* [ˈwɜːk ˈwɜ:k], and *board* [ˈboʊd ˈboʊd]. Length is wrongly shown after the nucleus in Southern and BVE *ride* [ˈrʌd]. The treatment of nuclear and non-nuclear liquids has not been laudable, and at times no difference has been recognized between the clustering at the end of *help* [ˈheləp] (nonstandard [ˈhep]) and that at the end of *hold* [ˈhould]. A rounded vowel is often written incorrectly in *could* [ˈkʌd ˈkhʌd] and *good* [ˈgʊd ˈgʌd], and the way speakers from most parts of the United States pronounce *bull* [bʌl] has been hopelessly misrepresented. The preservation of underlying †t in *eastward*, at least by many speakers in at least lento tempo, as opposed to the normal deletion of †t in *trustworthy*, has been ignored, as has the relatively sophisticated explanation that can be given for this difference. A great deal of intuition has been over-used by errant outsiders in investigating what is standard or not in speech communities.2 The speech of the upper class has been ignored in some communities, with a resultant obfuscation of the cross-over of the second-highest class. Failure to inhibit rule operation in informal styles has been misunderstood by some as rule addition, just as the inhibition of late rules or natural processes in formal styles (expounded by Stampe 1969; cf. Kazazis 1968) has also been misunderstood in some quarters as rule addition;3 this confounds the directionality of changes being investigated. Variation as to whether it is the manner or the place of articulation which determines the acceptability of non-sibilant fricatives has been overlooked.

So variationists must not rest satisfied with their contributions to linguistics, particularly the insights emerging from Mr. Labov’s investigations of unmonitored speech. They must try harder, just as much in recording and analyzing data as in theory construction. In saying this, it should be clear that I reject what I take to be Mr. Fraser’s view that the notion of a sociolinguistic grammar is a catachresis. I accept what I take to be the Labovian view that all grammars are sociolinguistic.4 I think that if variationists called themselves sociolinguists, they would be making a fatal concession to the vast majority of linguists, who believe that they are the real linguists, and that we are something else.

But the question arises: Can sociolinguistics be conceived of as more than interdisciplinary co-operation of scholars in analyzing similar data for the diverse goals of their respective disciplines? For my part; I imagine that scholars can succeed, and perhaps have succeeded, in defining the purview of an autonomous practical science standing between the theoretical sciences and the practical arts of language-planning and language-teaching, much as engineering stands between physics and the art of construction. Perhaps Haugen’s term, ‘the ecology of language’, refers to some such field, though I am not sufficiently sure of his use of the term to say.5 An intermediate discipline
of the sort that I have in mind would take its principia from the conclusions of the related theoretical disciplines and offer its own conclusions as principia to the applied arts.

A possible fly in the ointment of this intermediate discipline, from the linguist's point of view, is his extreme difficulty in keeping au courant of the rapid developments in just his own field. What chance he has of doing so is bound to be jeopardized to some degree if he spends part of his scholarly time trying to rise above the level of a naïve dabbler in another discipline, however closely related. Consider my answer to the problem referred to above concerning the place of relative numbers in the individual language-user's competence. I have claimed that a time-based wave model explains not only attested implicational patternings, but also the proportionate numerical superiority of older variables over newer ones. Simply to test out such a claim, either for the goals of linguistics or for those of the social sciences, requires more than a minimal cognizance of the subtleties of generative phonology and abstract syntax, of variable rules and marking theory, and of the other models used in connection with the wave model in question. Such considerations make a cross-disciplinary sociolinguistics a less sanguine prospect than it might at first blush seem.

I should like to stress the view that the goal of linguistics is the theory of grammar (understood as at least a description of how people communicate), in order to offset the category mistake of the view sometimes put forward that the goal of linguistics is the theory of language. (I presume no one claims that the goal of linguistics is a theory of speech.) Just as zoologists leave to poets, ecologists, and chemists certain aspects of animals, the object of their study, so linguistics must abstract away from some aspects of its object (language) and investigate others. This is as true of sociolinguistics as it is of other so-called hyphenated disciplines like biophysics or astrophysics. The question is whether the abstraction, language, goes too far beyond speech to qualify as the theoretical object of linguistics. Most linguists will be cautious, wishing to leave some aspects of language to aesthetics, poetics, rhetoric, communication engineering, neurology, anthropology, and sociology, just as some aspects of speech are left to anatomy, acoustics, and the like. Over-claiming on the domain of linguistics will create a credibility gap. On the other hand, our theory of grammar must be broad enough to include variation in the use of language in its social contexts. Broadening here can only be beneficial. But it will be harder to justify broadening our view of grammar without limit. Let us hope our colleagues in other disciplines will not entertain over-blown expectations of the capabilities of our discipline. And let us hope that they make use of the tools we can provide for analyzing linguistic variation.

Turning now to Mr. Bickerton's paper, it is important to notice that what he calls 'polylectic' I call 'polysystematic'. From the time of my
U. C. L. A. Conference paper (Bailey 1970), where 'polylectal' first appeared, I have been making a distinction between the effects on a language system of two kinds of changes: (1) changes resulting from the way (proven or credibly inferred) in which children acquire their native languages, which create new subsystems or lects within a given system, and (2) changes occurring through adult borrowing from other systems, which may create new systems. It has been my position—which I hope will eventually be empirically testable—that infant changes differentiate language subsystems without altering the essential characteristics of their over-all system, say by making significant changes in the underlying polylectal representation. Infant changes include the dropping of a feature to make a rule more general and the failure to learn a feature marking, marked weighting, or higher-level unmarking—involving, for example, rule ordering. Even such infant changes can restructure monolectal grammars, as is well known. Changing the following marked rule ordering

\[
(1) \ y \rightarrow z \\
(2) \ x \rightarrow y
\]

to the unmarked, feeding order; \( x \rightarrow y \rightarrow z \); results in the loss of rule (2) and the generalization of (1) to include both inputs—\( x \) and \( y \). This could result in a replacement of all \( y \)'s in the lexicon, if \( x \) came to be the sole source of \( y \). But a reordering of this kind in one subsystem of a polylectal system could hardly alter that system as internalized in its users, since in their understanding competence they would need both rules (1) and (2), and in an order from which the other could be predicted—viz. the marked order shown above.

It is a different matter when we come to deal with adult borrowing from other systems. The replacement of an apical trill by the much more marked uvular trill will not alter underlying representations in any way affecting the old system, but the introduction of a rule to 'mark' the feature [voice] of the final underlying voiced segments in a system where they had been devoiced in this environment will have more significant effects on that system. The introduction from another system of word-initial voiced obstruents into a system lacking voiced obstruents in this environment will restructure the lexicon, whereas some syntactic borrowings will affect a system only in minor ways. Mixture or creolization is obviously a matter of degree, and much is yet to be learned about it.

Bickerton's use of 'polylectal' denotes a formulation comprising what is found in a continuum or chain of overlapping, but not quite conterminous, systems, which have slightly different underlying representations that go beyond mere lexical usages. If a psychological justification of my kind of polylectal formulation is difficult—but convincing, as I hope
to have shown elsewhere—the psychological justification of Bickerton's polylectal formulation is vastly more difficult. At any rate, the two tasks are not equivalent, and the success of one should not be judged by the success or failure of the other. If it should be difficult to justify a new feature, it should be more difficult to justify a new component, like the rule-shift component of which we have just heard. I am sure that Mr. Bickerton would be the first to acknowledge that merely to show something works is but the first, necessary step; the sufficient step of providing a principled motivation is the main job that lies ahead. And it remains to be shown why deleting the copula in environment e is not the notational equivalent of introducing it in non-e environments.

These remarks are not intended to call into question the justifiability of the attempt to erect a formulation for a post-creole continuum. Given that all linguistically adjacent lects overlap in at least some speakers' internalized grammars, the attempt is certainly quite legitimate. It is important to know how speakers move up and down parts of the continuum by means of what is presumably unified in their heads. We need empirical criteria for allowing or rejecting alleged extension or shifting rules, if they are supposed to be internalized in any way.

Above, I mentioned system differences going beyond mere lexical usages. A few more words need to be said on that score. In any system, different speakers have different lexical items which are exceptional with regard to a given rule in the grammar, at least in their production of the language. Lexical variations have to be tolerated in any system. To fail to realize that the lexicon is a listing of exception and unpredictable materials, and is something other than the system of grammar as such, is to fall into the error of many word geographers. This is why Mr. Labov's treatment of any more tonight has not impressed me as strong evidence for heterosystematicity. Let me illustrate with an example that I have used previously. The same pattern heard in all varieties of English in break : breakfast and in retain : retention is heard in Irish English in many other words; e.g. mean : meant. This does not necessarily make these two varieties of English heterosystematic, since their rules are similar.

Another point is that I would not deny that the effects of borrowing among subsystems and of borrowing among systems may be similar. Both usually lead, for example, to leveling, the result being a higher-level unmarking or a lower-level feature unmarking. Inflectional levelings are well-known in cross-system mixture. Within English, note how Midwestern States English has resulted from levelings of New England, Pennsylvania, and Southern States English, until now it has no phonological rule, for example, that is unique to itself. To avoid confusion in interpreting plurilingual contact, we should be careful to distinguish between interference on the performance level and mixture at the level of grammatical competence, though not to such a degree as to rule out effects which the former may have on the latter.
Another important consequence of my proposal about the distinction between the results of synsystematic infant changes on a system and of heterosystematic adult changes has to do with the original and abiding concern of variationists--viz. their concern with language change--which has escaped the notice of most other linguists as our motive for the study of variation. The consequence of my proposed distinction is that in a tree model of linguistic parentage, every new system or node in a tree must have at least two parents, since a subsystem can leave its older system only as the result of plurilingual mixture. Determining at what point this has occurred lies in the future, although much work is being done already. The boundary of a system must somehow be contingent on the amount of system that a finite brain can unify.

The common core of Indo-European in languages descended from Proto-Indo-European is mixed with other systems--more in some languages than in others. Many of these languages might be called by some other name, if we knew enough about their mixture, with as good justification as by the name Indo-European. 13 Much Germanic vocabulary is not Indo-European, the genitive singular desinence in -i̯ in Keltic and Italic has been creolized from a common system, the retroflex obstruents in Indic perhaps have a Dravidian provenience, and the unaspirated [p t k] in Buchan Scots must have a non-English source. Romance passed through the filter of Keltic, Frankish, and Norse on the way to becoming the parent of two-thirds of English lexicon, syntax, morphophonics, and accentuation. 14

Two kinds of plurilingual mixture, at least, have to be distinguished, in addition to what are called 'pidgins'--speech forms which are not subject to the constraints imposed on natural languages by the manner in which children acquire them. Complete mixtures like English begin as naturalized pidgins--probably pidginized French, in this instance. But the layerized creoles, such as those described by Gumperz and Wilson (1970), have presumably never been pidgins. The first kind of creole goes the way of all languages when all of its parents leave the scene. But if one remains to dominate the scene, there develops a continuum between the two systems, as the creole progressively decreolizes, losing mixtures from systems other than the sole surviving parent, and forming a chain of systems nearer to that of the parent in question. It is such a continuum that Bickerton's models are intended to explicate. Eventually, different parts of such a continuum can be absorbed into the parent system as lectal subsystems within it, as is happening with inland Swahili (see note 7).

One may conclude by observing that Wolfgang Dressler (1971) has provided evidence--which has been confirmed by others in their work--that languages die out in ways which are opposite to the ways in which creoles are born. Where creoles overcome the monostylism of pidgins to become polystylistic, dying languages gradually become monostylistic.
in their typically diglossic uses. And where new rules begin variably in languages that are being born or are already born, dying languages—according to Dressler's evidence—change previously categorical rules into variable rules.

NOTES

1 It would be as contradictory to employ static models for describing and explaining variation as to employ Euclidean geometry to handle non-Euclidean materials, or to employ Ptolemaic astronomical explanations for the materials handled in Einstein's theory. The result would be self-defeat and frustration.

2 Unless my observations are wrong, sociolinguists usually act on the premise that our speech determines our acceptability, rather than the other way around. This may be true on the telephone when the person listening does not know us or in the limited case of jobs specifically demanding speaking ability; e.g. television announcers. Otherwise, however, it seems clear to me that it is our own total social acceptability which determines whether our hearers accept our speech. A failure to realize this has led to many ridiculous conclusions about what is standard speech in areas where the analyst is an outsider. Just as standard speakers sometimes use foreign words—in quotes, so to speak—without being thought to speak a foreign tongue, so they also use non-standard speech among their peers (who know they know better) for the purpose of conveying specific effects. Such non-standard forms are not to be regarded as standard simply because they are employed by standard speakers.

3 As more late rules are inoperative, the output will be less distant from the underlying representation. This is quite often found in creole syntax, and is typical of over-corrections in phonology and recompounding in morphology.

4 Labov (personal communication) treats sociolinguistics as a methodology. The point that I am trying to make is that an approach to linguistics ignoring what variationists do is out of date and should be replaced by one in which variation is an integral part. (See Bailey 1971.) I have shown elsewhere that, if we accept the premises of generative phonology that (1) linguistic change is rule change, and (2) that grammars are monolectal, there results a paradox that rule change cannot occur in the language of adults. Only in a polylectal framework is rule addition in adult speech a viable notion.

5 The study of why one language or form of a language prevails against the seeming odds over another, although fitting the notion of linguistic ecology, probably belongs to the social sciences, rather than to the intermediate discipline under consideration here.

6 Including borrowings from older forms of the written languages. Adult changes should also include the hypercorrections that bring
languages closer to their underlying representations, discussed by Labov in several places as an important kind of language change.

7In addition to the unrelenting, if not necessarily rapid, fissiparousness of natural changes within a system, there is another way in which lectal subsystems originate. This is exemplified in the spread of coastal Swahili inland, as it first recreolized with related Bantu lects, and then as these new creoles progressively have been decreolizing until reabsorbed into the Swahili system as lectal subsystems within it.

8A higher-level unmarking may overrule a lower-level feature marking.

9A marked segment like uvular [R] must presumably begin as the result of some high-level unmarking; e.g. assimilation to the uvular-lowering of an adjacent nasal.

10I do not, of course, mean to deny all systematicity to the lexicon; e.g. implicational lexical redundancy rules.

11It should be clear that a mapping of linguistic differences will be valid only for one combination of such social parameters as age, sex, status, class, style, the rural/urban difference, differences of future aspiration, etc. Any mapping must also disregard the reality of individual grammars which do not agree with the production grammars of other speakers having the same sociological characteristics; such deviant individuals of course have one of the output grammars generated in the polylectal system, but it may be a grammar mainly used by speakers of other sociological characteristics. Note, in addition, that spatial mappings do not necessarily juxtapose the lects that are linguistically adjacent, although this linguistic adjacency is what the systematizer is really looking for. In Bailey 1970, I tried to show why different polylectal grammars within a language system asymptotically approach a common panlectal result.

12The fact that Black Vernacular English is quite similar in Northern U.S. cities quite distant from one another indicates that Black speakers of different origins level the speech of one another in the direction of the same norm. Investigations of this phenomenon (as of Slavish, etc.; see Bailey 1970) should yield many new insights into the theory of grammar, at least if conducted in a theoretically knowledgeable manner.

13This consideration affects the important question as to whether Proto-Indo-European had a complex verb system like Sanskrit and Greek, or the simpler one of Germanic and Anatolian. The very creolized nature of the latter two families of languages (note the large proportion of non-Indo-European vocabulary) makes a simplification of an earlier, more complex verbal system an extremely likely and attractive idea, although it runs counter to some present-day thinking.

14Middle English employed the ancestor of the contemporary adverbial formatives, -ly, the wh- pronouns, and the verbal-noun formative -ing—all Germanic forms—in Romance functions. The wh- interrogatives
were made into relatives, and the gerund became a progressive 'aspect'. Eric Hamp (personal communication) has brought to my attention a study by Braaten (1967), in which it seems clear that the use of -ing in continuous tenses in English has an ultimate source in British Keltic. It was no doubt assisted in becoming current in English because of phrases like en chantant in French, which in turn (as Braaten observes) may have a Keltic provenience. Note that most of the prosodic aspects of English are Germanic, which merely confirms the hypothesis being made here.

REFERENCES


The aim of this paper is to work out an empirical method of conversational analysis, capable of recovering the social assumptions which underlie the verbal communication process by focussing on actors' use of speech to interact, i.e. to create and maintain a social situation. The basic theoretical position which sets this work apart from other work in sociolinguistics is that, in the analysis of face to face encounters, the social categories and social roles, the sorts of things that social anthropologists and sociologists refer to by terms such as role, status, social identities, and social relationships, can be treated as communicative symbols. They are signalled in the act of speaking and have a function in the communication process which is akin to that of syntax in the communication of referential meaning. Just as grammatical knowledge enables the speaker to distinguish potentially meaningful sentences from non-sentences, knowledge of the social values associated with the activities, social categories and social relationships implied in the message is necessary in order to understand the situated meaning of a message, i.e. its interpretation in a particular context.

Let us illustrate with a simple example. The utterances:

(1) They are holding a meeting to discuss the issue.
(2) They are getting together to talk it over.
(3) They're sittin' down to rap about it.

can be used to describe the same event and are thus in a sense referentially equivalent, although their social implications are quite different.
Item one implies at least some overtly recognized division of roles. There must be two or more contesting parties, possibly a chairman or at least some overtly agreed upon agenda of possible topics and allocation of rights to speaking, etc. The second description is unmarked with respect to these characteristics and could also be used in referring to an ordinary conversation. Item three definitely suggests an unstructured activity where speakers speak their own mind without any prior limitation on what can be said and who is to speak when.

How are such social assumptions implicit in messages signalled in speech? Clearly the difference is not merely a matter of the dictionary meanings of the lexical items involved. Linguists will note that the two sentences carry different variable selection rules. While sentence two allows:

They're gett'n t'gether to talk it over.

variants such as

They're hav'n a meet'n to discuss the issue.

or

They are sitting down to rap about it.

violate ordinary stylistic cooccurrence restrictions. The signalling of social meaning, like the signalling of reference therefore involves both paradigmatic and syntagmatic constraints.

It has been pointed out that sociolinguistic selection constraints are different from the cooccurrence constraints which apply among syntactic and semantic features, since they cut across the usual levels of grammar and do not allow for rule ordering. Scholars such as McCawley (1968) and DeCamp (1971) suggest that they be treated as discourse features applying to a stretch of discourse as a whole and marking it as formal, informal, polite or familiar, etc. But so far we have had little in the way of systematic study of such discourse features. Even a casual examination shows that the identification of particular discourse stretches as 'formal' or 'informal' is considerably more problematic than the identification of a lexical item as carrying semantic features such as concrete or abstract, animate or inanimate, etc. When used in natural conversation sentence one, for example, may have a variety of situated meanings which are quite distinct from what we ordinarily understand by formality. When it serves to describe a group of children talking in the playground, the effect is humor or irony. On other occasions it may convey contempt or condescension.
Not that the feature of formality is irrelevant in these cases. In order to understand a message as ironical we must know (a) that speakers in our culture distinguish among speech activities which do carry procedural norms such as those described above and those that do not, (b) that activities of the former type are appropriate for adults and not for children, and (c) that the distinction made in (a) is signalled by choice among referentially equivalent lexical, phonological, and other options. Formality can be seen as a rough, though somewhat imprecise, gloss for the association between linguistic alternants and the social characterization of activities. When the content of what is said meets the speakers' expectation as to what this relationship should be the interpretation of formality applies, and in that sense formality is an aspect of what we can call social meaning. In arriving at a situated interpretation of a message the speaker must match the social meaning of an utterance with the content of what is said and with other contextual features incident to the speech act. When these show the expected match then the description itself signals the fact that the activity is carried out in the prescribed manner. When there is a conflict between the social values of two or more features, i.e. when as in the above case the behavioral norms associated with the form of the message conflict with the context, which refers to children rather than adults, then metaphorical interpretations such as irony are generated. Given the referential meanings, the speakers' situated interpretations and a knowledge of the possible linguistic options, it should be possible to recover the social meaning of the messages in context using elicitation techniques and methods of analysis which are equivalent to those used by linguists in building a theory of grammar.

An additional analytical problem in the study of social meaning, one which does not occur in grammatical analysis, is caused by the fact that the social meaning of particular modes of speaking varies from group to group, from generation to generation in ways which are not easily relatable to the usual factors of income, social class, education, etc., and which are as yet little understood. The statement:

Come at six. We are dining early tonight.

when made in the context of a dinner invitation, is part of the normal unmarked speech of many older generation Englishmen. When used in a group of younger people, it is likely to be interpreted as ironical or sarcastic. Although both groups have grammatical competence in English, they attach different situated interpretations to the same message and, to the extent that speech is used to assess speakers' personality characteristics, they differ in their judgment of the same speaker's attitude, ability, etc. Sociolinguistic analysis of conversations, if it is to be valid, must account for such differences.
One of the first systematic attempts to go beyond the grammatical description of isolated sentences and deal with social meaning in language in terms of selection constraints is found in the recent work of Halliday and his associates (1964). Halliday argues that language can be viewed as 'behavioral potential', i.e. that social structure and grammar offer a set of options and that spoken utterances are the result of a selection process in which speakers choose among a network of social, semantic, syntactic, and phonological alternatives to express their intent. He cites the example of a mother who wants to keep her child from associating with certain playmates and utters the sentence:

That sort of place is not for playing in.

The content of this utterance can be analyzed at the social level as involving an appeal to (a) authority, (b) to general rather than particularistic norms, and (c) being object-oriented rather than person-oriented in content, since it refers to playing in a place rather than playing with people. If the mother had chosen to say:

I don’t like you playing with those children.

the appeal would have been characterized by the features (a) affective, (b) particularistic norms, (c) person-orientation. Halliday goes on to point out that the speakers’ selection of different clusters of social features also leads to significant differences in grammar.

Category clusters such as the above are considerably more detailed than simple dichotomies like formal and informal, and have the advantage of relating the linguist’s grammatical categories to the sociologist’s content analysis of questionnaire responses. Nevertheless they are subject to a number of objections when applied to the study of face to face situations.

To begin with, the analysis takes for granted the sociologist’s analytical categories. It is assumed that these categories constitute a known universal grid. What is seen as problematic is their distribution in particular cases and their linguistic realization. While it is certainly possible to show that, when studied with statistically significant samples, there is a correlation between sociological codings of sentences and language usage, Halliday’s discussion gives no indication of how and through what cognitive processes speakers utilize these categories in the interpretation of messages and how it is that two sets of people can arrive at different interpretations of the same message. Nor is there any unambiguous procedure for assigning social meaning features to sentences. On the contrary, writings on social interaction have amply documented the arbitrariness and ad hoc nature of sociological coding procedures (Garfinkel 1972, Goffman 1964).
But even if we accept the assumption that such categories are directly applicable, Halliday provides us with no explicit theory of how and by what linguistic signs they are signalled in speech. The implicit assumption is that the analysis of phonology, syntax, and semantics conducted in accordance with the commonly accepted linguistic research paradigms can also provide information on social meaning. Recent work in the ethnography of communication suggests that is not the case. It has been shown that there exist a number of other linguistic devices not covered in the ordinary type of linguistic analysis which serve to communicate social meanings. Most important among these are (a) the sequential ordering of utterances and the allocation of turns of speaking among participants (Frake 1964, Sacks 1972, Schegloff 1972), (b) the choice of message form or speech event (Hymes 1972) (e.g. whether to convey a message by conversation or lecture), (c) code switching or selection among cooccurrent clusters of variables (Gumperz and Hernandez 1971), (d) intonation, stress, speech rhythm, and other paralinguistic cues (Crystal 1971).

A major important analytical principle to emerge from the recent work in this area is that it is impossible to interpret situated meanings apart from the total context of what has been said before and what is said afterwards. The interpretation of a message is not a constant, it depends on what it is in response to and how it has been received. What is said at one point in a conversation may change the interpretation of everything that has gone before.

William Geoghegan's (1971) study of address rules in Samal, a Philippine language, is the first formal attempt to account for contextual interpretation of messages. Geoghegan distinguishes between two distinct processes for generating appropriate address forms: ‘code rules’ which specify what can be said, and ‘marking rules’ which rewrite the code meanings in accordance with context and social expectations. Whereas the output of code rules can be talked about in abstract semantic terms, marked meanings communicate only by contrast with code meanings. They depend on processes of matching such as those described above in which the speaker evaluates the social meaning of alternate expressions against the context in which they are said. It follows that marked meanings can only be studied as part of larger stretches of interaction. Analysis must deal with exchanges rather than with sentences or text, and concentrate on how speakers react to each other through speech.

In view of the novelty of this approach to language and in view of the as yet little understood variability in interpretation rules there is a case to be made for a method of study which, like the techniques of the older structural linguists, combines analysis with discovery procedure. The starting point of the analysis is the speakers’ situated interpretation of verbal exchanges. The purpose is to discover (a) how and by what verbal devices such interpretations are generated and (b) what
underlying social assumptions are necessary in order to relate situated interpretations to linguistic form. In what follows we will try to illustrate such a method.

The data for our analysis is taken from two tape recordings, approximately one hour each, made in a Berkeley, California, elementary school. One first and one second grade classroom were recorded. Both were taught by an open classroom method, where children are divided into activity groups, each group being seated around a separate table and engaged in a particular task, such as reading, building models, etc. Classes were ethnically mixed according to the usual Berkeley formula, forty percent black and oriental children and sixty percent white. The two teachers, one of whom was black and one white, were each assisted by several volunteer assistants who helped in guiding the activity groups. The materials were recorded over a period of several weeks, during which time the ethnographer acted as a regular classroom observer (Lewis 1970). Before making the recordings she spent several days in each classroom familiarizing herself with the children, the physical layout of the classroom and each teacher’s style. No attempt was made to capture everything that went on at a particular time or to make a sample of all interaction. The investigator simply moved from group to group and turned on the tape recorder whenever an interaction sequence caught her interest, sometimes starting the recording in the middle of an encounter, making sure, however, that what was recorded was of sufficient length to permit analysis.

Although all recordings were made during class sessions, not everything on the tape can be regarded as teaching. On occasion a teacher converses with another adult or with a child. At other times children engage in informal play. Learning sessions likewise are variously structured. It becomes necessary therefore, as a first step in the analysis, to divide the tape into episodes, noting natural breaks in the interaction and changes in participants and to find operational criteria for distinguishing teaching episodes from others. The criteria used in this task were (a) the focus of the activity, i.e. what was done and (b) role distribution as it emerged from the sequence and illocutionary force of the statements and responses that make up a stretch of interaction. Using these criteria, we were able to define teaching as an encounter involving two or more participants focussed about a particular task (i.e. reading, spelling, constructing a model, etc.) and characterized by a division of roles such that one actor \( S_1 \) assumes and is accorded authority to guide the action and is accorded expertise with respect to that task. Thus \( S_1 \) can be seen to command, correct, criticize, etc., while the other actor \( S_2 \) follows \( S_1 \)'s directions, asks for information, or confirmation, occasionally protests or disagrees, etc. Given this definition we were able to isolate a number of teaching events all of which fall under our definition but which vary widely with
respect to the way in which participants define their activities and their mutual relationship and in the verbal strategies they use to accomplish the interactional tasks involved. The present analysis will focus on two episodes which differ maximally with respect to these features: a second grade child giving a first grader a reading lesson and a teacher conducting a spelling lesson with a group of second graders.

In line with our analytical goal to relate situated meanings to the linguistic signs by which they are conveyed, conversational texts are transcribed at two levels (see the appendix for a complete transcription). The left side of each page gives a verbatim record of what is said, including detailed phonetic transcriptions and intonational markings wherever necessary. The right side comments on the interactional tasks being accomplished, identifying the illocutionary devices used and sometimes the corresponding perlocutionary effects, giving verbal glosses or paraphrases wherever possible. While the comments on the right attempt to capture the speakers' situated interpretations, the transcription on the left indicates how and by what linguistic signs this interpretation is conveyed.

Although we use the term situated interpretation, we do not mean to imply that we are giving a detailed account of everything that a speaker may see or understand in a message. Ultimately any message is open to a number of interpretations, depending on the extent to which personal background knowledge is brought in. The aim is to capture the kinds of judgments that members make when, for instance, they agree on saying that speaker A is being critical of speaker B, is confirming what B has said, is sounding friendly, aggressive, etc., and to show what these judgments are based on.

Since our situated interpretations are couched in everyday language, since glosses or paraphrases are given and since judgments are directly related to what is said before and after and how it is received by other participants, our right side transcriptions form an ideal basis for checking the analyst's interpretations against those of others as well as for avoiding the kinds of ambiguities that arise when the analyst's categories are used or when grammaticality judgments or other evaluations of language are elicited in the abstract or under experimental conditions that differ from ordinary life situations. Whenever possible our transcriptions were shown to a participant in the situation as well as to other speakers of different social background, so as to test the extent to which speakers can agree on situated interpretations and to isolate systematic differences in interpretation. Whenever such differences are found it is then possible through further elicitation to determine the extent to which they reflect differences in language usage rules, whether they can be traced to social values attached to particular linguistic forms or prosodic features or to speakers' use of unfamiliar rhetorical strategies such as code switching or simply to differences in rules of etiquette.
Although the two episodes studied here seem quite different on the surface, when we examine the interactional tasks that the child teacher $C_1$ and the adult teacher $T$ perform we find that these are to a large extent equivalent. Our use of the term interactional tasks here is in some sense the parallel of what recent students of classroom interaction call language function when they classify utterances as instances of framing, focussing, informing, directing, responding, requesting, etc. (For-syth 1972). But rather than attempting to isolate a limited number of such functional categories, we prefer to give somewhat more discursive and hopefully more informative descriptions, descriptions which apply to exchanges rather than to individual utterances. Our aim is not to count the incidence of functional categories but rather to isolate some similar tasks and use these as a basis for comparing the ways in which various individuals signal the fact that they are performing these tasks.

(1) Warding off interference

A. Child teacher ($C_1$):

line A. 38a C3: (not defined as learner) Boy—

A. 38b C2: (defined learner) Boys— and— girls—

A. 39 C1: 'Don't, tell 'im, 'I'm teachin' 'im.

B. Adult teacher ($T$):

line B. 30 T: ... How do you spell Ken?

B. 31 A: Where's Ken? K— C— K— E—, er—

B. 32 T: A, 'I is spelling it ...

Line A. 39 and line B. 32 are both responses to an attempt by a student other than the one designated as respondent to participate in the interaction. $C_1$'s strategy is an unambiguous direct command to stop participating, followed by an equally direct assertion of her rights in the situation. $T$'s strategy is to disqualify the interruption indirectly by stating that someone else is performing. In fact, it is not the case that I is spelling but that $T$ wants I to spell. $T$ is describing a social event, I's turn at spelling, which if understood has the effect of discouraging A from attempting to spell. Both $C_1$ and $T$ here employ a social rule that goes 'X's performance of an operation precludes Y's (or anyone else's) doing so'. However, $C_1$'s invocation of the rule follows upon a forceful indication of what she wants C3 to do, as a back-up to her command. For $T$ the very invocation of the rule is expected to have the force of a command, and no specific directive for A's behavior is given.
(2) Confirming the learner's response

A. Child teacher:
line A. 21 C2: Page thirty-three, where's thirty-three?
   A. 22 C1: Thirty-three.
   A. 23 C2: Thirty-three, is this thirty-three?
   A. 24 C1: Thirty-three.

B. Adult teacher:
line B. 8 T: Of a--
   B. 9 C: Girl.
   B.10 T: O.K. (goes on to address another student)

Here in episode A, C1 confirms C2's question of line A. 23 by an exact repetition of her own statement of A. 22, using the same intonation and stress pattern. T on the other hand uses a special lexical item 'O.K.' to signal her confirmation.

The following episodes are somewhat more complex and require more detailed analysis.

(3) Comparative elicitation strategies

A. Child teacher:
line A. 1 C1: 'Come here.
   A. 2 C1: 'Sit down around here.
   A. 3 C2: Do you know 'any ... (you know) - like this?
   A. 4 C1: 'Sit down here, now!
   A. 5 C2: 'I don't wanta read all them words!
   A. 6 C1: 'Well (let's see)'what's this' word anyway.
   A. 7 C2: 'I don't know!
   A. 8 C1: 'Large.
   A. 9 C2: 'What?
   A.10 C1: 'Large!
   A.11 C2: 'Large.

B. Adult teacher:
line B. 9 T: 'I, do you see a name on that page that you know?
   B.10 I: Ann.
   B.11 T: 'Hm?
   B.13 T: 'That's the one that I just named. How do you 'spell Ann?
   B.14 I: A - N - N
   B.15 T: How do we say the A? (pause, no response)
   B.16 I: (no response)
   B.17 T: 'J, 'do you wanta help her?
B. 18 C: 'I know.
B. 19 J: The letter capital A.
B. 20 T: 'Capital A-N-N. 'Why do we say capital, I?
B. 21 I: (no response)
B. 22 T: 'Why would we put a capital A on the Ann, E?
B. 23 E: Because it's someone's name.
B. 24 T: It's the name of somebody, I. So we make it special.
B. 25 E: A girl, the name of a girl.
B. 26 T: Would you see any other name I, that you know?
B. 27 C: I see a name, a Ben.
B. 28 T: Any other name. Let I find one. Do you see a name you know there?
B. 29 I: (long pause) Ben.
B. 30 T: All right, Ben. That's right

Both episodes can be seen as attempts by the two teachers to elicit task-oriented responses. In each case the learner begins with inappropriate answers. The teacher then obtains the desired response by a series of strategies.

The distinction between the situations is that C1 must define her role whereas T's role is clear. This also bears on the difference in verbal strategies. T's students are more or less obliged to sit in her presence by the very definition of the task. If T is going to give a spelling lesson, then the students must be there, that is why T is in the school building in the first place. This is not the case for C1. She must make the role relationship clear to her student personally and she is doing it by means of a direct command, telling him to Come here and approximately where to sit, Sit down around here, etc. When he does not respond immediately, but rather directs a question at her, she repeats her command giving specific time and location, Sit down here, now!

Even in this brief sequence C1 demonstrates a characteristic of her overall strategy, which is to change both form and style of delivery in an escalating series until her purpose is accomplished. Lines 1 through 4 are successively more specific in terms of actual verbal content. C2 is directed first to approach, then to sit in the general vicinity, then to sit in a specific place at a specific time. Line 4 shows an intonational contrast as well in the raised pitch and volume and high-fall contour on now! This sequence embodies a three-stage tactic which reaches its peak in the verbally and intonationally marked now! The tactic succeeds to the extent of focussing C2's attention on the task, albeit to protest, I don't wanta read all them words!, whereas his previous rejoinder was diversionary and unrelated, answering a command with a question. We may also assume that by now C2 is in fact sitting down, judging by
the subsequent dialogue. C1 then narrows further the focus on 'words' introduced by C2 by repeating and intonational foregrounding, as well as by specifying, What's this word, anyway? C1's new tactic is the use of the interrogative. Attention is now focussed on a single word. C2 says he doesn't know in a tone of voice implying How should I know? (fall-rise contour), and C1 then supplies the actual word. We might say she is giving an example of the kind of response she expects from C2 as the student. C2 still delays by asking for a repeat even though C1's first reading was quite loud. She repeats with escalated emphasis—the first time was a low-fall, the second is high-fall and high volume. C2 then repeats the word in low-fall tone and is drawn into the lesson relationship.

Our adult teacher example starts at a point in the discourse approximately equivalent to line A.6 in the child-child discourse, except that her focussing question does not have the simultaneous contrastive organizational effect that C1’s does. That is, there is no doubt that her students are the students and are expected to respond in the vein of the questions asked. C1’s Well, let's see, what's this word, anyway? and T’s I, do, do you see a name on that page that you know? are both attempts to elicit a task-oriented response from a student. C1 characteristically uses a question form of the type what's this, for which a content answer would ideally be the response. T uses a more indirect question form do you see, for which a yes-no answer would be appropriate, and further questioning would logically be required in order to elicit a content answer. All of T’s subsequent elicitations have this form. The student, I, accurately glosses T’s question as the functional equivalent of C1's what's this and provides in fact a content answer. Some interpretative skills beyond the purely lexical-syntactic skills are required in this case to deduce the functional meaning. After I’s answer, T asks for a repeat and I repeats the word Ann. We note, however, that I’s intonation has changed from a mid-sustained reading type of tone to a low-rise tone which might be suitable to express tentativeness. T neither confirms, denies nor repeats I’s answer but metacommments on it: That's the one that J just named.

We infer from the above statement, which has no overt value markers in it, a mild criticism of I’s answer. To interpret it requires the understanding of a rule: it does not count to give an answer that someone else has already/just given. T then asks I how to spell the word, foregrounding spell with a high-fall contour which serves as a two-way contrast: (1) with T’s utterance of line 1, V, how do you spell 'Ann'? and (2) with her preceding utterance, glossed: J just named it, but how do you spell it? I spells Ann. T asks a thematic question designed to elicit the 'capital initial letter for proper nouns' concept which has been a focal point in the lesson. Once again the surface form of her question How do we say the 'A'? requires some secondary-level interpretation
in order to produce the correct response. I does not respond and T calls on another child, J, with a do you want to request form, employing the expression help her to indicate that J should provide the correct answer. A child, possibly I, says I know as J provides the correct answer. T repeats the spelling of Ann using the correct case designation for the first letter. She then addresses I once more by name to elicit the theory behind 'capital-letter': Why do we say capital, I? I does not respond. T addresses another child, E, by name using a hypothetical mood Why would we and somewhat metaphoric language, put a capital A on the 'Ann'? E responds correctly, Because it's someone's name. T repeats E's correct response with changed surface structure, It's the name of somebody, I, foregrounding the concept 'name' with high-fall tone and employing I's name as if to draw her particular attention to it. T complements the answer with a pseudo-logical explanation, So we make it special, which conveys a somewhat generalized notion of the relation of capital letters to names. A further step of deduction is required to get T's meaning: it's a name, we make a name special, a capital first letter is a way of making a name special. E supplements her own correct answer by specifying further that Ann is the name of a girl. T does not respond to this volunteered information and addresses I by name once more, using a hypothetical auxiliary, Would you see any other name? Another C simultaneously volunteers an answer. T continues questioning I, repeating any other name, then deflects C, neither confirming nor denying his answer, employing an imperative form, Let I find one. She repeats her question to I in the same intonation as all her previous questions. After a long pause I responds with a tentative high-rise tone, Ken? T then confirms, foregrounds the answer by repeating it with a high-fall and repeats her confirmation, substituting a pronoun-copula-predicate adjective construction that's right for the purely affirmative all right, by way of emphasis.

T has now succeeded in eliciting a correct and seemingly original task-oriented response from I. It took a series of exchanges, the length of which may be due in part to the number of children involved. Contrast this with C1's swift and direct interchange with a single student, C2. Other contrasts present themselves in addition to length of sequence. We feel that the brevity of C1's sample interchange is no accident, for behind every syntactic and lexical shift we have been able to uncover a tactical purpose and a corresponding effect on C2, and this would be equally the case for any segment of the child-child reading lesson we examine. We might attribute this in part to C1's ambiguous status as a child teacher or to other reasons such as her cultural background and her shared code with C2. For whatever reason, C1 is efficient in her use of language, and at the same time she makes ready shifts in her use of syntactic forms as well as prosodic features to accomplish her purpose.
T also employs changes in syntax and lexicon and uses some intonational devices. But two impressions emerge from her overall discourse. One is that she relies heavily on the interrogative form, both for requests to perform and for actual elicitation of content responses. The other is that the succession of forms she employs in eliciting an appropriate response is in some ways a mirror image of what C1 does. While C1's strategy is one of increasing specificity of requests, T shows a tendency to become successively more indirect, moving from how do you spell to how do we say from do you see to would you see. There is also a lack of intonational variation in T's delivery.

Let us now turn to a more systematic examination of the linguistic devices used by participants in the two episodes. Upon first listening to the recording of the two children in episode A we were struck by the extraordinary degree of musical and rhythmic relatedness between C1's and C2's speech. To some extent this is exemplified in the 'thirty-three' sequence cited above, where a single word forms the pivot for a five-line exchange. We temporarily styled this rhythm as 'syncopation'. A more elaborate example of a similar interactional pattern was discovered upon reading Reisman's (1970) paper 'Contrapuntal conversations in an Antiguan village' at just the point in the tape where we were about to give up trying to transcribe a difficult passage because 'everyone was talking at once.' Similarly, on first listening to the section for adult teacher examples in this paper, the transcribers were impressed with the 'monotony' of the interaction, and with how uninteresting it was to transcribe in contrast with the two-children tape.

What kinds of perceptions do these judgments come from? Why is it possible for someone to listen to two people talking and, without understanding the content of what they are saying, know for example that they are arguing, romancing, or otherwise intently interacting? There are doubtless many reasons. We would like to suggest that the prosodic component encompassing stress, pitch, and timing along with speech features usually termed paralinguistic is as important in interpreting the meaning of interactional exchanges as referential meaning or propositional content and that it functions to maintain and control interactions in somewhat the same way as the coordination of gestures, facial movements, eye blinks, etc., described in the recent literature on kinesics (Condon and Ogston 1967). In talking of such things as question intonation, emphatic intonation, etc., linguists suggest that intonation, when choice among alternatives is optional and does not affect the grammaticality of a sentence, carries meaning. What we would like to suggest here is that such expressive meanings, although surely based on some universal signs, are not meaningful in the abstract, but are the output of marking processes generated as part of the interactional exchanges. What these features do is affirm, question, emphasize, or otherwise qualify something that has been said or is being presupposed in the
message. When seen in this light, prosodic aspects of speech become part of an optional set of communicative strategies that can be used alternatively with syntactic, lexical, or phonological variables. Choice among these alternatives is a function of the speaker's background and his communicative intent. The contrast between children's and adult strategies in our example provides some good illustrations of this phenomenon.

Here are some examples of the children's use of intonational and prosodic features and the meanings they carry. When the child teacher repeats or echoes part of the learner's utterance, using the low-fall stress and intonation contour, the meaning is confirmation or affirmation. See the use of thirty-three in item two. When an item is repeated with substitution of high rise for low falling or high falling tone, the item is being questioned or challenged. When the learner repeats or echoes the teacher's utterance with the same intonation and stress the meaning is 'I am following your directions' or 'I am doing what you told me'.

(4.1) A.10 C2: 'Large!
A.11 C2: 'Large.

(4.2) A.26 C2: Come—
A.27a C1: The—
A.27b C2: Ba—'The? 'The? The—

(4.3) A.68 C1: Say—
A.69 C2: Sa—'Say?
A.70a C2: 'Say?
A.70b C2: 'Say—

In the last sequence C1 begins by prompting. C2 starts to follow, then challenges her. As he repeats his challenge she simultaneously affirms her original statement. A rise in pitch on a particular item accompanied by shift in stress to that item and special loudness and sometimes falsetto voice suggests special emphasis or surprise or contrast with a previous statement.

(5.1) A.1 C1: 'Come here.
A.2 C1: 'Sit down around here...
A.4 C1: 'Sit down here, now!

(5.2) A.8 C1: 'Large.
A.9 C2: 'What?
A.10 C1: 'Large!
Emphasis often takes the form of a sequence of several statements, each one augmenting the pitch and stress of the preceding one, followed by a resolution which is signalled by a slight drop in pitch, as in the following:

(6) A. 8 C1: Large.
A. 9 C2: What?
A. 10 C1: Large!
A. 11 C2: Large. (resolved)

Intonation contrasts are also used by the children for more subtle maneuvers, such as bluffing or covering up errors.

(7) A. 32 C1: The—>
       [də]
A. 33 C2: The—>They!
       [də]
A. 34a C2: 'At ain't no [də].
A. 34b C1: No, '[də]
A. 35 C2: 'At's [də], 'did you say [di]?
A. 36 C1: I said...
A. 37 C2: The—>
       [də]

Here C2 has attempted to correct C1's reading. Unable to carry his protest through, he pretends to have said what she said, attributing a different articulation to her. Note how in lines 34a and 35, C2 quickly changes his mind in the face of C1's repetition, and having already denied that the word was [də], he must resort to a purely tonal contrast in order to complete his sentence and at the same time assert the correct form of the reading as his own.

In comparison with the reliance on intonation and stress in Episode A, the adult teacher, although she also uses similar devices on occasion, makes much more extensive use of lexical and rhetorical devices. Consider the following passage where the adult teacher like the child teacher attempts to correct and guide the learner's response:

(8) B. 59 B: P—> E—> T—> E.
B. 60 T: Just P—> E—> T—> E? What do you say about the first letter?
In (8) the teacher challenges B's answer by repeating it as a question preceded by the qualifier *just*, indicating it is inadequate. She then asks another question which contains a hint as to what part of the answer has been neglected. In (9) we see further examples of the adult teacher's use of strategic questions both to signal the inadequacy of a child's response and to provide cues as to the locus of the missing elements of the answer. These cues are contained in lexical indicators rather than direct hints, e.g. B. 41 ... *Do you see an [ae]-sound?*, B. 43 *What sound do you see?* Contrast CI's repetition and assertion of the right answer using intonation as an aid, with T's focus on what's wrong with the answer, using lexical items and question-forms.

(9) B. 37 T: Do you see any word there that you know, B, anyone's name?
B. 38 B: 'Pat. P—
 a. B. 39 T: 'What's that? 
B. 40 B: 'Pat. 
b. B. 41 T: 'Where do you see 'Pat? 
c. Do you see an [ae]-sound in there?
B. 42 B: 'No. 
c. B. 43 T: 'What sound do you see? 
B. 44 B: 'Pat. 
b. B. 45 T: Do you see an [ae]-sound? 
B. 46 B: No. 
c. B. 47 T: What sound do you see? 
B. 48 B: 'Pet. 
B. 49 T: 'Peter?
 b. B. 50 T: Is there an [or] on the end?

Devices: a. request for repetition,  
 b. challenge cum question: *where do you see Pat?* 
 c. attempt at focussing on salient feature

The following examples illustrate the adult teacher's strategies for confirming learner's response.

(10.1) B. 7 T: Of a—
B. 8 V: 'Girl
B. 9 T: 'O.K... (addresses another C)

(10.2) B. 28 T: Do you see a name you know there?
B. 29 T: (pause) 'Ken.
B. 30 T: 'All right. 'Ken. That's right.
Note the variety of expressions the teacher uses to confirm and guide her learners: O.K., Alright, That's right, Good, That spells Roy. She also uses the device of repeating the answer. However, echoing the child's intonation is not noticeably a part of her strategy. Nor is exact echoing of the form of the response her method so much as confirming the content. For instance, in B.24 she repeats the exact meaning of E's answer with a different surface structure, and the optional substitution of somebody for someone:

(11) B.22 T: 'Why would we put a capital 'A' on 'Ann', E?
B.23 E: Because it's someone's name.
B.24 T: It's the name of somebody, I...

The adult teacher does, however, make extensive use of echoing elsewhere when repeating and thus developing her own question strategies. Here, as in the case of the eliciting strategies discussed above, her technique contrasts with that of the child teacher.

A further important aspect of the children's speech is the sharp rhythmic distinction they draw between reading and conversational style and word game style. Compare the following example:

(12.1) A.32a C2: Boys---> and---> girls-->

(12.2) A.40 C2: The boys---> and girls-->

In the first item each word is produced separately as in a list of items, vowels are elongated and tone is sustained, i.e. neither falling or rising. In (12.2) elongation of vowels is less but the rhythm is staccato and tone is still sustained. By contrast a sentence like:
is characterized by normal word sandhi and a wide range of intonational patterns. In

is pronounced in sing-song style and draws the response

where by falling in with the style C3 signals his competence in the style and in managing peer relationships.

The contrast between reading and conversation style is used for communicative effect in the following sequence:

As we described previously, C2 has challenged C1's reading, but in the face of her repetition accepts it, asking her if she had said something else. Before she can assert what she really said he falls into reading style as if to say 'Never mind, let's go on reading'.

In episode B, on the other hand, while the teacher maintains the same rhythm throughout, the children differ stylistically between interactions with her and interactions with each other. Contrast:

where the child's speech echoes the teacher's low pitch, volume, and lack of tonal contrast and uses standard English variables, with:

Here the rhythmic and intonational contrasts are pronounced. Already is emphasized by high pitch, elongated vowel, high falling tone with 2-stage drop and stress. Note the use of black variables such as copula deletion.

The overall impression gained from our analysis of style and sequencing of illocutionary devices is that the two groups differ in their definition of the teaching task and of their social relationships, i.e. mutual rights and duties. The children see their task as teamwork focussed around the task of reading, i.e. producing the printed word orally. This is illustrated with the following:
In (19) the end of a sentence has just been reached in the ongoing reading. C1 reads the first word of the next sentence in typical sustained reading intonation and is echoed by C2. C1 turns aside momentarily to deflect interference without breaking pace as C2 again repeats the read word and augments it with another one. He then contests C1’s reading of a previous word, but she continues with the passage, picking up in sequence to the word read by C2 as she was simultaneously talking to C3. Even while talking aside she has not broken the sequence of interaction with C2. After a short pause where C2 does not speak in his turn, C1 begins reading the sentence again with C2 following in word-by-word suit in identical intonation and timing. C1 repeats the whole phrase and is echoed identically by C2. C1 then introduces a shift in mood by dropping her voice to a low pitch, and C2 repeats her word but in a contrasting slightly projecting pitch. C2 then augments by filling in the next word on the page. C1 reads a word echoed by C2, both in a low-rising tone suitable for signalling the penultimate in a series, and C1 caps off the sentence with a high-falling tone, ending the sequence. Over all the sequence there is a regularity of time intervals between the utterances of the two speakers. There are no long pauses, and in the event of a short pause, as between lines A.49 and A.50, C1 immediately fills in by starting the passage over again, successfully inducing C2 to follow suit. Variation in rhythm, such as a double-beat, may occur, as when C2 augments his repetition with an independently read word, or when C1 recapitulates a phrase, followed by C2. But the intervals between utterances remain equal over-all. In another passage, where one C initiates a change of pace by shortening the interval and raising the pitch level, the other follows suit. We see that sentences are
co-operatively produced. This is a characteristic of the children's reading throughout. Elsewhere in our analysis word count reveals C1 and C2 speak approximately an equal number of words even when they are not reading. In example (3) for instance, C1 produces 19 words to C2's 20.

Moreover, we see a certain parallel production in sequential length configuration. If C1 makes a long utterance, C2 is likely to make a long reply; if she makes a one-word utterance, he will do likewise and vice-versa. In the sample segment of Episode B which we used to contrast C1's and T's elicitation strategies, example (3) (lines B.12–B.31), we found that T used 103 words, of which 6 are children's names used in address, to 25 words for all of the children combined. Almost by definition then, her utterances are considerably longer than those of the children. If her purpose is to elicit responses from the children, her input cost is considerably greater proportionally than C1's. This contrast in relative production, along with the variety of other contrasts we have presented, arises, we feel, out of differing concepts of participant structure and differing definitions of the task of teaching held by the child and adult teacher in our data. T's definition of her task is one of evaluating the learners' ability to read and to verbalize spelling rules.

(20) B.1 T: J, how do you spell Ann?
  B.2 J: 'A--N--N
  B.3 T: 'A--N--N. What kind of an A?
  B.4 J: Capital.
  B.5 T: Why is it capital?
  B.6 J: Cause it's a name.
  B.7 T: Of a -->
  B.8 J: Girl.
  B.9 T: O.K. I, do you see a name on that page that you know?

Her technique is one of calling on the children to read, and guiding the children along certain logical or quasi-logical lines of induction by lengthy questioning. She sees the activity as a rule-segregated one in which the relevance of utterances is determined by her alone, and children's production is almost entirely in response to her elicitation. The entire episode takes the form of a sequence of questions and answers. The teacher's implicit authority is reflected in the fact that she assumes the right to continue questioning even in the face of no response. In the event of volunteered responses, deemed 'inappropriate' or 'out of turn', T may discourage the speaker by indirectly referring to rules of conduct. The child teacher in episode A is less concerned with evaluation than with making sure that C2's production is faithful to the printed words, i.e. that 'reading' is in fact taking place. 'Correct' answers are thus unmarked and she interferes only when production
deviates from what is written. Moreover, C1 appears to see her task not as eliciting performance from C2 so much as helping him in reading. This is not to say that division of authority is not operating in the situation, for we have seen C1 in various attempts to gain and maintain control. Also, she has the inherent superiority of one who has mastery of a skill that is being transmitted. However, her actual teaching behavior is fundamentally cooperative rather than hierarchical, and is centered on a certain prosodic style and rhythm of exchange. C2 is free to initiate topics and even to guide the activity to some extent. He can interrupt and there are times elsewhere in the material when C1 assumes the role of playmate. It is this alternation of roles along with the parity of production ... the sense of prosodic interplay which differentiates the child's definition of teaching as a co-operative activity from the adult teacher's focus on evaluation and control.

Conclusion

Although the present paper is intended to be illustrative rather than definitive it should be sufficient to suggest that methods based on those of linguistics, when applied to the study of conversations, can yield rich information on social interaction provided the analytical framework is adapted to the purpose, and questions asked of the data are properly defined. Frake (1964), Sacks (1972), Schegloff (1972), and Moerman (1970) have shown that the sequential ordering of information and selection of illocutionary devices and content are rule-governed. Our own work adds, we hope, to this tradition by showing how the rules of social interaction can be realized in linguistic form.

Our findings suggest the existence of a level of sociolinguistic rules which, like the marking rules described by Geoghegan, take the output of grammatical and code rules to generate socially meaningful speech. Only some of the linguistic cues which enter into the creation of social meaning have been dealt with in traditional linguistic analysis. Others, such as variable selection, code switching, choice among intonational, prosodic and paralinguistic options, sequential ordering of content and illocutionary devices, have only begun to be studied. Where they have been described they have tended to be treated in taxonomic terms and not as an integral part of the process of linguistic communication.

One reason for this neglect has been the emphasis on sentences and texts as the proper unit of linguistic analysis and on grammars as structurally homogeneous systems. A distinguishing feature of sociolinguistic marking processes is that meaning does not inhere in isolated sentences and thus cannot be listed in dictionaries. The creation of social meaning relies on juxtaposition of one utterance with another or with some aspect of the speaker's or hearer's background knowledge. It is this juxtaposition which is used for metaphorical effect and which, as
Claudia Mitchell-Kernan (1971) has suggested, may account for the persistence of linguistic variables in speech communities.

The other feature of this marking process is the variability of marking rules. Our own examples point to some important differences between children and adults. Some of these may be maturational, others are culturally determined. This variability is an important feature of interaction in any speech community. It can lead to serious miscommunication (Gumperz 1971). Yet its relation to macro-factors of class, race, ethnic origin, educational achievement is as yet little understood. Further investigation along the lines suggested here may lead to clarification of some of these issues.

APPENDIX: TEXTS

EPISODE A. Child teacher

C1 is the "teacher", a second-grade girl.
C2 is the "learner", a first-grade boy.

<table>
<thead>
<tr>
<th>TEXT</th>
<th>GLOSSES AND COMMENTS</th>
</tr>
</thead>
<tbody>
<tr>
<td>A.1</td>
<td>C1: &quot;Come here.&quot;</td>
</tr>
<tr>
<td></td>
<td>A.1 - C1 addressing C2 demands that he approach.</td>
</tr>
<tr>
<td>A.2</td>
<td>C1: &quot;Sit down around here.&quot;</td>
</tr>
<tr>
<td></td>
<td>A.2 - C1 directs C2 to sit down with measured timing and falling tone as in A.1.</td>
</tr>
<tr>
<td>A.3</td>
<td>C2: D'you know...any...you know...like 'this?</td>
</tr>
<tr>
<td></td>
<td>A.3 - C2 asks question unrelated in content and conversational rhythm (sandhi contrasted with measured 1-2 rhythm) and intonation.</td>
</tr>
<tr>
<td>A.4</td>
<td>C1: &quot;Sit down here, now!&quot;</td>
</tr>
<tr>
<td></td>
<td>A.4 - C1 repeats demand of line A.2. 'Now' is characterized by high-falling pitch, overloud stress, and separation from the preceding statement by a pause suggesting forceful emphasis. Gloss: Let's get down to business.</td>
</tr>
<tr>
<td>A.5</td>
<td>C2: &quot;I don't wanta read all them words!&quot;</td>
</tr>
<tr>
<td></td>
<td>A.5 - C2 protests, using a sustained tone. The tone level is suggestive of a mid or minor tone, and is characteristic of C2's speech.</td>
</tr>
<tr>
<td>A.6</td>
<td>C1: Well, (let's see) 'what's this 'word, anyway?</td>
</tr>
<tr>
<td></td>
<td>A.6 - C1 starts with a vocative followed by a question, in effect directing C2's attention to the material. Lexical item 'word' is echoed from C2's foregoing line, but now specifically singular. It is foregrounded by nuclearity, primary stress and high-fall intonation.</td>
</tr>
<tr>
<td>A.7</td>
<td>C2: &quot;I don't know!&quot;</td>
</tr>
<tr>
<td></td>
<td>A.7 - C2 disavows. Falling-rising tone suggests gloss: How should I know?</td>
</tr>
</tbody>
</table>
A. 8 C1: Large.  
A. 8 - C1 informs in declarative low-fall.

A. 9 C2: 'What?'  
A. 9 - C2 questions abruptly in request for repeat.

A. 10 C1: 'Large!'  
A. 10 - C1 repeats with emphatic loudness. A. 10 escalates A. 8, going from low-fall to high-fall, as if to say 'I already told you.'

A. 11 C2: [splice in tape]  
A. 11 - C2 repeats with same intonation as C1's A. 8 and volume close to her A. 10, echoing her intensity.

A. 12 Cl: Go get yo book. (You ain' gon' read.) intonation, high head, low sustained fall, vernacular phonology.  
A. 12 - C1 commands in 'stop jiving' intonation, high head, low sustained fall, vernacular phonology.

A. 13 C2: [Laughs]  
A. 13 - C2 responds by laughing, as if to deflect C1's command.

A. 14 C1: 'Go get yo' book!  
A. 14 - C1 repeats her command with stress shifted to 'book', and higher, more childlike pitch.

A. 15 C2: W-w-w, you said you would go...the teacher.  
A. 15 - C2's high-pitched falsetto voice suggests protest and challenge to C1.

A. 16 C1: 'Come on now.  
A. 16 - C1 shifts strategy to coaxing.

A. 17 C2: ...'read a' book!  
A. 17 - C2 makes a partly inaudible statement in a falsetto tone suggesting protest.

A. 18 - T's question, which serves as a directive bears out the (largely intonational) evaluation of C2's line as a protest. T then hortatively addresses C1 with a direction whose function is to make C2 act.

A. 19 C1: 'O. K.  
A. 19 - C1 expresses compliance.

A. 20 C2: -right.  
A. 20 - C2 also complies, in a low voice.

(There is a long pause on the tape; presumably C2 is getting his book.)

A. 21 C2: Page thirty-three, where's thirty-three?  
A. 21 - C2 announces the page number, then questions about its location. Note measured rhythm and syncopated timing, projecting tone.

A. 22 C1: Thirty-three.  
A. 22 - C1 repeats the salient item 'thirty-three' in a low declarative tone.

A. 23 C2: Thirty-three, is this thirty-three?  
A. 23 - C2 repeats the item 'thirty-three', then asks if the page he has
A.24 C1: Thirty-three.

A.25 C2: Kay. Well, I was, I was over here.

A.26 C2: Come--

A.27a C1: The-

A.27b C2: The? The? The-> [bae?]

A.28 C1: Morning-

A.29 C2: Morning-> is-> in-> coming?

A.30 C1: 'Over!

A.31 C2: 'Over! The morning i->

A.32 C1: The-> [de]

A.33 C2: They! [de]

A.34a C2: At ain't no [de],

A.34b C1: No, [de].

A.35 C2: At's [de], 'At's [de], 'did you say [di]?
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her reading, such that they are simultaneously producing phonetically identical but intonationally contradictory utterances. C2 reflects C1's affirmation of her own reading by finishing his sentence with the word he just negated, still using a challenging tone to mark a contrast which no longer exists. He follows up with a repetition of his effective confirmation of C2's reading, and asks her if she said something else.

A.36 C1: I said...

A.37 C2: The→

A.38a C3: Boy→

A.38b C2: Boys→ and→ girls→

A.39 C1: 'Don't tell 'im, 'I'm teachin' 'im.

A.40 C2: The boys→ and girls→

A.41 C1: Go--

A.42 C2: Go→ to--

A.43 C1: Lu--

A.44 C2: Lunch. Come→

A.45 C1: They→

A.46 C2: 'They?

A.47a C1: Get outa here.
rhythm, not breaking the pace of her interaction with C2, and employs vernacular phonology.

A. 47b C2: They go— A. 47b - C2 is simultaneously reading, using the word C1 gave him and which he previously slightly questioned.

A. 48 C2: !Not `they! A. 48 - C2 now openly contests the word 'they'.

A. 49 C1: Down— A. 49 - C1 reads in sustained tone, continuing from C2's line A. 46b, ignoring his protest of line A. 47.

A. 50 C1: They— A. 50 - C2 has not picked up his cue and C1 begins reading the sentence again from the beginning, as if to encourage C2 to read after her.

A. 51 C2: They— A. 51 - C2 repeats after C1 in identical tone and timing.

A. 52 C1: Go— (short) A. 52 - C1 reads.


A. 54a C1: They go-down— A. 54a - C1 recapitulates the sentence, bringing the words together, ending with a mid-rise sustained tone indicating that more will follow and retaining reading tone.

A. 54b C2: They go-down— A. 54b - C2 reads in chorus with C1, with identical timing and other features. In both cases there is a slight pause after 'they'; 'go down' is like one word. C1's voice is at a slightly lower pitch and volume, like a support to C2's reading.

A. 55 C1: To— (low volume) A. 55 - C1 here drops her voice still lower.

A. 56 C2: To— the A. 56 - C2 uses a more projecting pitch and volume. He follows up his echo by reading another word independently in a sustained tone, but at a lower pitch.

A. 57 C1: 'Lunch A. 57 - C1 prompts C2 with slow onset, long [l:], and low-rise tone appropriate for an item in a series other than the last one.


A. 59 C1: 'Room A. 59 - C1 completes the sentence in high-fall tone suitable for logical conclusion.
A. 60  C2: 'I be\textsuperscript{t} finish wit\textsuperscript{t} this book!  
  \hspace{1em} \textsuperscript{\dagger}What's \_that word?  
A. 60 - C2 states expectatively in conversational tone that he is/will be finished reading the book. He then returns his attention to the reading, asking C1 for the next word. Low-rise with slightly sustained tone suggests the gloss 'What's that word? I know but temporarily forget.' (Or, 'What's that word, anyway?')

A. 61  C1: Ann\textsuperscript{--}  
A. 61 - C1 reads.

A. 62  C2: Ann\textsuperscript{--}  
A. 62 - C2 echoes.

A. 63a  C2: Mother\textsuperscript{--}  
A. 63a - C2 begins voicing next word slightly before C1 but otherwise they read in unison.

A. 63b  C1: Mother\textsuperscript{--}  
A. 63b - C1 reads in unison with C2.

A. 64  C1: Is in\textsuperscript{--}  
A. 64 - C1 reads in sustained tone with continuity between the two words.

A. 65  C2: Is\textsuperscript{--} in\textsuperscript{--} \[?\textsuperscript{iz} \ ?\textsuperscript{in}\]  
A. 65 - C2 repeats the same words but with breaks in between as he normally does for reading.

A. 66  C1: 'Ann's mother is \_in the \_lunchroom.  
A. 66 - C1 reads the full sentence with continuity.

A. 67  C2: Ann's mother\textsuperscript{--} is\textsuperscript{--} in\textsuperscript{--} \[?\textsuperscript{iz} \ ?\textsuperscript{in}\]  
  \hspace{1em} \_the \_lunchroom.  
A. 67 - C2 repeats the words of the sentence but breaks between the words as he would if he were to read alone. However, he makes units of the noun phrases, reflecting advance knowledge of the structure of the sentence. At the end he abandons reading tone and gives normal declarative low-fall to 'the lunchroom'. His low-fall contrasts with C1's high-fall on the same word.

(There is a break in the recording)

A. 68  C1: Say\textsuperscript{--}  
A. 68 - C1 prompts in low-fall sustained tone, long vowel.

A. 69  C2: Sa--?Say?  
A. 69 - C2 starts to repeat but breaks off to question.

A. 70a  C2: \textsuperscript{\dagger}Say?  
A. 70a - C2 repeats his questioning of C1's word in emphatic falsetto as

A. 70b  C1: Say--  
A. 70b - C1 affirms in reading tone.

A. 71a  C1: \textsuperscript{\dagger}Ann--  
A. 71a - C1 starts a word with elongated vowel, low-fall tone.
A. 71b: C2: \textit{Ann}-- \hfill A. 71b - C2 joins her in reading the word slightly behind. He also elongates the vowel, and uses the same low-fall tone.

A. 72 C1: \textit{Is} (whisper) \hfill A. 72 - C1 prompts in whisper, long vowel.

A. 73a C2: Is--
A. 73b C3: Is-- \hfill A. 73a-73b - C2 and C3 echo C1 in chorus

A. 74 C1: (to C3) \textit{I'm 'on tell your teacher...} \hfill A. 74 - C1 warns C3 in sing-song rhythm and tone, sustained final syllable, to stop interfering. The tone interval in the nucleus could be characterized as minor.

A. 75 C3: You \textit{cain' git me...} \hfill A. 75 - C3 responds with sing-song dare, glossed 'Try and catch me'. His intonation is a development of C1's and also employs a minor interval in the nucleus drop, as well as an even more elongated musical final vowel.

A. 76 C2: \textit{What's this word?} \hfill A. 76 - C2 asks a question about the lesson in conversational tone, using a slight falsetto sustained or minor interval rise on the nucleus. In effect he supports C1 here by ignoring C3's interruption.

**EPISODE B. Adult teacher**

The teacher is represented in the text by T, individual children by their initials. Unidentified children are represented by C.

**TEXT**

B. 1 T: \textit{J, how do you 'spell Ann?} \hfill B. 1 - Quiet, almost muffled voice, T calls on J by name, following with request for information.

B. 2 J: \textit{\'A--N--N.} \hfill B. 2 - J responds by spelling out word. Her tone echoes T's quiet falling tone.

B. 3 T: \textit{A--N--N. What kind of an A?} \hfill B. 3 - T repeats J's spelling a little faster, drawing together letters in monotone, follows with request for further information.

B. 4 J: \textit{Capital.} \hfill B. 4 - J supplements in slightly sustained tone.

B. 5 T: \textit{Why is it capital?} \hfill B. 5 - T questions further in slightly sustained low-fall tone, suggestive of question intonation.

B. 6 J: \textit{Cause it's a name.} \hfill B. 6 - J answers, low voice.
B.7 T: Of a—

B.8 J: Girl.

B.9 T: O.K. 'I, do you see a name on that page that you know?

B.10 I: Ann.

B.11 T: 'Hm?

B.12 I: Ann.

B.13 T: That's the one that J just named. How do you spell Ann?

B.14 I: A—N—N

B.15 T: How do we say the A?

B.16 I: (no response)

B.17 T: 'J, do you want to help her?

B.18 C: 'I know.

B.19 J: The letter capital A.

B.20 T: 'Capital A, N—N. Why do we say 'capital', I?

B.21 I: (no response)

B.22 T: 'Why would we put a capital 'A' on Ann, 'E?'
Because it's someone's name.

It's the name of somebody, I. So we make it special.

A girl, the name of a girl.

Would you see any other name, I, that you know?

I see a name, a Ben.

... any other name? Let me find one. Do you see a name you know there?

(pause) Ken?


She addresses it to still another child, E. Note figurative language 'put a capital A on...'

B. 23 - E answers correctly, echoing T's low pitch and volume and lack of tonal contrast.

B.24 - T repeats E's answer with different surface structure, foregrounding the concept 'name' with high-fall, employing I's name to draw her attention, and supplementing an explanation of the answer.

B. 25 - E augments her answer, making it more specific.

B. 26 - T once again addresses I with a question that is functionally an elicitation, as in B.9. She uses the hypothetical mood and specifies 'other', reflecting both that I has answered previously and that her answer had already been given.

B. 27 - A child other than I gives an answer. Note that his answer corresponds directly to the surface form of T's elicitation formula, 'do you see', filling in the answer to the unspoken further question, 'what is the name that you see?'

B. 28 - T meanwhile is still phrasing her question to I, repeating the salient portion of it. T deflects C's participation with a positively framed command, without changing pace or pitch. She goes on to repeat her question elicitation to I.

B. 29 - After a pause I answers with a high-rise question contour.

B. 30 - T gives a lexical confirmation with low-fall tone, repeats the answer, then confirms once more with syntactic and intonational emphasis. She then asks I for another stage of the answer, quickly reminding her to apply the capital letter principal, using the expression 'say to that first letter' to evoke it, and the construction 'Don't forget--' as a prompt device. The prompt is sandwiched closely between the two question-requests to spell,
B. 31 A: 'Where is 'Ken? K→,  
C→ K→ E, er...

B. 32 T: 'A, 'I is spelling it. 'Capital K--

B. 33 I: Capital K→

B. 34 C: 'You messin' up the 'raser already!

B. 35 T: E→

B. 36 I: 'N.

B. 37 T: 'Right. 'Ken. Do you see any word there that you know, repeats the answer in low-fall declarative tone, then turns to address another child with the thematic question, same low pitch, low tonal contrast.

B. 38 B: 'Pat. P→

B. 39 T: 'What's that?

B. 40 B: 'Pat.

B. 41 T: a. 'Where do you see 'Pat?  
b. Do you see an [ae] sound in there?

B. 42 B: 'No.

and is marked by higher pitch and greater tonal contrast than is usual for T.

B. 31 - Another child attempts to answer the question put to I.

B. 32 - T discourages her from spelling by metaphorically stating that I is doing so; in fact she is referring to the social situation, I’s turn at spelling, and the assumption that if it is I’s turn then no one else should spell. T prompts I by giving part of the answer.

B. 33 - I repeats T’s prompt, with same intonation.

B. 34 - A child addresses another admonishingly, using a voice quality, tonal contrast and phonological register noticeably different from that of C's involved in spelling.

B. 35 - T continues prompting I, ignoring line B. 34.

B. 36 - I this time supplies the final letter of the word instead of imitating T as in line B. 33.

B. 37 - T gives lexical confirmation, repeats the answer in low-fall declarative tone, then turns to address another child with the thematic question, same low pitch, low tonal contrast.

B. 38 - B gives an answer in declarative low-fall and proceeds to the next step, spelling the name.

B. 39 - T calls for a repeat of B’s answer with high-rise intonation.

B. 40 - B repeats her answer with same low-fall tone.

B. 41 - T asks a question which is in fact a negation or a challenge of B’s answer, as we see by her following question. While the form of T’s first question could mean a simple request for information, the second question in this context could only mean that there is no [ae] sound in the word. ‘Pat’ and ‘[ae]’ are foregrounded by high-fall.

B. 42 - B picks up challenging import of T’s questions and cue that answer is
B. 43  T: 'What sound do you 'see?'

B. 43 - Note high-rise vs. low-rise tone reflecting 'do you' question vs. wh-question. Interactional significance carried by change in content; B. 41 a. and b. are rhetorical questions and B. 43 is a request for information.

B. 44  B: 'Pat.'

B. 44 - B repeats her former answer, not responding to T's cues.

B. 45  T: Do you see an [æ] sound?

B. 45 - T repeats her challenge question with identical form and tone as in B. 41b.

B. 46  B: 'No.'

B. 46 - Again B responds in the negative as demanded by the context of T's question, although substantively there is as yet no indication that she's gotten the point.

B. 47  T: What sound do you 'see?'

B. 47 - T repeats request for information made in B. 43.

B. 48  B: 'Pet.'

B. 48 - B changes the appropriate phoneme of the answer. Note lack of semantic tie throughout: where B in her answers refers to whole words, T refers to letters or phonemes. Evidently they have different implicit notions of what the activity is.

B. 49  I: 'Peter?'

B. 49 - I posits an answer with high-rise question intonation, still referring to whole words.

B. 50  T: Is there an 'er' on the end?

B. 50 - T questions using her former strategy of eliciting sounds. Gloss: 'It's not quite right. There's no 'er' on the end.'

B. 51  I: Is it 'Peter?'

B. 51 - I repeats her question using a full sentence and slightly louder, higher voice, high-rise tone. Her question shows that she has not gotten T's strategy.

B. 52  T: I's 'helping you. She's given you a 'clue. But is there an 'er' on the end of that?

B. 52 - T speaks without directly confirming or negating I's response, rather she addresses B about I's guess. The first two statements can be understood as qualifying the ensuing question, which reiterates what she has already asked in B. 50, using the same semantic approach as in all the foregoing.
However, this time she shifts to high-rise tone, possibly to contrast with the two statements B.52 a and b, which give indirect acceptance.

B.53 C: (indistinguishable) B.53 - C replies inaudibly.

B.54 T: 'What's the word?' B.54 - T asks for repeat.

B.55 C: (indistinguishable) B.55 - C once again replies inaudibly.

B.56 T: 'What?' B.56 - T once again calls for a repeat.

B.57 C: Pete. B.57 - C produces an audible answer.

B.58 T: That's right. How do you spell it? B.58 - T gives a lexical confirmation and asks for the second stage of the answer.


B.60 T: 'Just P--E--T--E? What do you say about the first letter?' B.60 - T questions, implying the answer is incomplete, and gives a clue as to what is missing.

B.61 C: Capital. B.61 - C supplies the name of the concept T is eliciting.

B.62 T: How do you spell Pete? B.62 - T once again asks C to spell the word, implying that the capital letter concept should be included.

B.63 C: Capital P--> E--> T--> E. B.63 - C performs correctly.

B.64 T: Do you see a word there, that you know, somebody's name? B.64 - Without responding to C's final answer, T turns to another child, L, with a question. Her long utterance, although it contains several embeddings, gives the impression of being unpunctuated and maintains an even tone overall, except for a slight rise at the nucleus.

B.65 L: (indistinguishable) B.65 - L responds at a very low volume.

B.66 T: 'Can you spell the word 'Roy' for us, L?' B.66 - T uses a question form to request L to give the second stage of the answer.

B.67 L: R--O--Y. B.67 - L spells, using a tone sequence appropriate to seriation.

B.68 T: 'Good. That spells 'Roy'. R--O--Y. B.68 - T confirms lexically, makes a confirming statement about the answer, and repeats what L has said, in declarative low-fall contour.
NOTES

1 This paper is based on data collected by Louisa Lewis, under a grant from the University of California Urban Crisis Project, and it draws on her ethnographic observations (Lewis 1971). We also consulted Miss Lewis on the interpretation of our transcriptions. Analysis of materials were assisted by grants from the National Science Foundation (GS30546) and the National Institute of Mental Health (5R01-MH18188).

We are grateful to Claire Lefebvre for her invaluable contribution to the transcription and analysis of the data, and to Clive Criper, William Geogheghan, Paul Kay, and John Trim who commented on parts of the analysis. Our special gratitude goes to Jenny Cook whose insightful comments are reflected throughout this work, and to Linda Viloria for her typing of the manuscript and good humor in the face of various revisions.

2 The method used for marking intonation is taken from the system presented by John Trim (1972) that is derived from the Kingdon method (Crystal 1969), with the addition of two symbols of our own. The system adopted is provisional, and in our future work we may wish to incorporate elements of the system developed by Crystal (1969). In the notation of Trim, the utterances are divided into tonal groups in which a nucleus is determined as carrying the major phrase contour. The phrase may consist of one word or a long utterance containing several embeddings, according to the number of breaks or nuclear contours. Normally the nucleus is the last stressed syllable in a phrase, though it may happen that a large number of syllables or words will follow after it. Trim describes six contours: (1) low-fall, (2) low-rise, (3) high-fall, (4) high-rise, (5) fall-rise, and (6) rise-fall. Our material required the addition of two symbols, one for sustained tone - in low, mid or high pitch, and one for emphatic +, which is a diacritic added to one of the Trim tones.

In addition to the nucleus, the phrase may have a head or heads, either high or low, consisting of stressed syllables preceding the nucleus, as well as breaks, pre-nuclear changes in pitch. The nucleus may be followed by a tail or tails, syllables coming after the nucleus and falling away from the level of the nucleus gradually, or rising, according to the direction of the nucleus.

3 The term illocutionary devices is patterned on the notion of illocutionary force, introduced by Austin (1965) and developed by Searle (1969).

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ON SOME PUNS: WITH SOME INTIMATIONS

HARVEY SACKS

University of California, Irvine

The substantive topic of this paper is 'puns'. My substantive aim is to show a conversation sequential ordering that can be found for a characterizable class of puns.

I must assume that any current audience is unfamiliar with work on the sequential organization of conversation. I have chosen my topic because it is not, in the first instance, technical, puns being manifestly accessible to all of us; because it is of some interest to the various disciplines concerned with language, permitting readers to compare results they can obtain with those we develop; and because a report on it can exemplify some of the ways we work on conversation, thereby aiding others in seeing what this work is like. I am not claiming that puns are specially interesting on theoretical grounds; in that sense this investigation is quite unmotivated. But, puns do happen in talk, are attendable, and we can then look at them to see what sorts of order their examination might reveal.

1. Puns are recognizable, though not always recognized. Since few of us collect them, it may aid the reader in thinking his own way through our argument if we proceed first to offer a group which will at least recall their variety. Here then are some puns, selected from those we have found in the materials we work with. (All cited talk, here and elsewhere in this paper, is from actual conversation.) (1) "...the terminally ill, dying, kamikaze pilots--but I ditched them." (Speaker is talking of a term paper being done about the terminally ill.) (2) "The corral's the big joke you know--it just doesn't look very stable." (Re a corral that speaker's husband built.) (3) "I'm committed to visiting my sister at Camarillo every Wednesday." (The place referred to...
is a mental hospital.) (4) "The only way it would work would be with a
good carpenter." (5) (In the following, 'A' is the mother of 'B' who is
the wife of 'C'.)

A: "This house is clean."
B: "I always clean it before you come."
C: "Only and always."
B: "No, that's not true, I..."
C: "She cleans it before my mother comes too."
B: "I always keep it clean, relatively speaking."

(6) "No, curtains look chintzy." (7) "Baloney, you don't eat meat!"
(8) "'Round here if you did it, it would just be considered a great tech-
nological feat." (Talk was about walking on water.) (9) "He's hot for
a radar range an' I need it like a hole inna head."

(10) We've uh made
lots of headway with you." (From a group therapy session.) (11) A.
"I'm tired. I have to go take a nap." B. "Well, that's always the
aftermath of having to study so hard to prepare." (The studying was
known to be for a math test.) (12) "So I hightailed it up there to the
market to get my turkey."

For purposes of much of the exposition the following fragment of
conversation is to be looked to. A few guides are in point. The three
speakers, whom we call Ken, Roger, and Louise, are, at the time,
teenagers, talking together at a group therapy session they are each
members of, a fact which I mention because the reader might feel
cheated not to know it, but which will not figure in our considerations.
As the excerpt begins, Ken is telling a story about his twelve year old
sister's affection for the Beatles, a story that is on topic with a good
deal of prior talk.

1.
(1) Ken: W'-the -her whole room she's got it wall-papered.
She just- she just got done rewallpapering it about
a month ago-
(2) Louise: -with the pictures of the Beatles.
(3) Ken: No. A month ago Mom had it done in this grasscloth
like junk yknow it looks like // Hawaiian.
(4) Louise: Yeah I know we have it.
(5) Ken: She came in there the other night with scotch tape an'
every inch of the room. You couldn't- the roof I
think she's got done in Beatle pictures and she lays
in bed at night---

2.
(6) Roger: She's doing that cause all her friends are ( // ) the
Beatles.
(7) Louise: Well they need some kinda idol you know, something
// to look up to-
(8) Ken: Idol! They look like little kangaroos.

Here we notice a possible sort of pun as seems amenable to analysis via our ways of working on conversation. Louise’s explication of the need for an idol—‘something to look up to’—plainly puns on Ken’s preceding story (with its references to pictures on the ‘roof’ (sic), and the sister’s lying in bed.)

Proceeding to work on a possible sort of pun we can have relatively weak initial goals because we suppose that uneducated assertions about puns would say, acknowledging that they are noticeable and have ways of having their noticed presence attended, that they occur here, there, and anywhere in conversation and that there is no formal relation between the positioning of pun and punned on. Then, if we can find some sort of orderliness in these areas we will have learned something. So, an initial interest being in seeing if we can at least partially distributionalize puns and relations between pun and punned-on, we have an idea of the sorts of observations we seek.

2. A first such observation is: (Sometimes) puns occur in ‘proverbial’ expressions. That is obtained from an examination of the instant fragment (the pun here occurring in the proverbial expression, ‘‘They need...something to look up to.’’), seems typical, and is of particular interest because it might be a sort of observation as can be supported not only by the somewhat equivocal use of a manifold of instances, but also by developing, as we shall later, an account which shows that there are systematic bases for puns occurring in proverbials. (Why we would suspect the possibility of such a basis being explicable I cannot briefly develop.) The observation does not suffice to motivate an attempt to work out whether indeed puns systematically occur in proverbials because such a locus, alone, has no apparent bearing on problems in the sequential organization of conversation. But the observation does suggest a direction: If we can at least partially distributionalize proverbials, in conversation sequential terms, we will, puns occurring systematically in proverbials, thereby distributionalize puns too in conversation sequential terms. Partially, of course. (I am quite aware that this is not a purely logical assertion. We shall be concerned that the proverbials we isolate be such as can contain the puns we are targetting.)

While it might be said about proverbials too, that they occur here, there, and anywhere in conversation, the proverbial here (‘‘They need...something to look up to.’’) occurs on a story completion, and is done by a story recipient. From a great deal of prior work we know that the talk of a recipient on a story completion is: (1) a structural position in conversation; (2) a structural position that is part of a serially
ordered set of positions involved in the organization of story telling in conversation; a position that accommodates a variety of forms, many of which have their instances used to do a particular ‘interactional’ job, i.e. ‘exhibiting understanding’ of the story they succeed, a job which story recipients do perform; (4) that job is one which proverbials are distinctly, and in some ways distinctively, apt for, and; (5) furthermore, proverbials are very commonly used at that position.

Our second observation is then: (Sometimes) proverbials occur on story completions. When done there by a story recipient they are at least partially occupied interactionally with exhibiting understanding of the story they succeed.

3. I proceed now to show that the ‘weak’ results we were initially interested in obtaining quite directly follow from these two observations. As of now, it may be noted, we have a tentative class of pun-potential positions: In proverbials, produced on a story completion, by a story recipient. We show first that this is non-trivially a class of pun-potential positions. Consider some quite familiar knowledge about the understanding of proverbials. First, commonly though not exclusively, they contain empirical or concrete materials; in some fashion they say things about clouds, rolling stones and the like. Because of this, they are, as sentences or sentence parts, subject to being understood in a usual way sentences containing concrete materials are understood, i.e. as saying various things about their subjects. In contrast, however, with the usual run of sentences that are similarly concrete, proverbials admit to being understood idiomatically or metaphorically, i.e. as talking about something other than their concrete materials. Indeed, detecting that a sentence containing concrete materials is or contains a proverbial yields that it is to be understood idiomatically, not concretely. The foregoing sentence contains what we call a ‘preference rule’ for understandings: Given the detection of a proverbial in a sentence, Prefer to use idiomatic over concrete understanding of it. It is by reference to the existence of such a preference rule that a motive occurs for finding what is after all but one of the possible facets of a sentence, that it is or contains a proverbial. With this we by no means exhaust the consequentiality of such a rule.

Given the use of a proverbial after a story (i.e. some sort of report of some sort of particular occurrences), and given the presence in the proverbial of concrete materials, and a preferred understanding of the proverbial idiomatically, then the systematic possibility for puns resides in the potential for a congruence between the concrete materials of the proverbial and the concrete materials of the story. Some such congruences directly yield a pun, as in our initial fragment and such a one as the following (in which Ellie, a divorcee, is being called by Lee, whom she knows to be married, for a date; the ‘Vi’ referred to by him being his wife):
Lee: No I'm et the sta(h)ge right now frankly that ehh y'know of course Vi en I are getting along fine, but god damn it I think maybe I've just got some wild oats to sow-- I've been-- --we hadda, well a, not a tragedy, well yeah it was a tragic death thot uh a very dear friend'v ours got killed en iz wife uh wz ar matron of honor at ar wedding.

Ellie: Yeah,
Lee: E:n I've been takin her out quite a bit y'know,
Ellie: Yea//h,
Lee: A:nd uh I think it's done me some good 'n then shit I thought--god damn it I thought I got in love with this broad y'know,
Ellie: Yahm.
Lee: So that shook the old (h)house(h)hold up fer a(h)whi(h)le heh=
Ellie: Oh yes I c'n imagine.
Lee: Y'know, a:nd uh I think Vi's, --realized thet hell maybe it's good f'me t'go ou:t. Y'know, --a:nd uh: I'm not so sure it isn' either.
Ellie: hhh Well I think thet y'know s- a lott'v times these situ-
ations Lee cn very easily reach a stalemate. hhh

A few comments are in order about the preceding discussion.
It is plain that at least one sense of how puns work involves the presence of a word, phrase, or other construction of more than one meaning, one meaning being used in understanding the construction in its conversational locus, while the other meaning(s) are also fitted to the locus, although in different ways. Such a formulation suffers not only from roughness, but also from its failure to specify the conversa-
tional locus involved. The foregoing discussion is intended to specify where in conversation a structural basis for this sort of operation may be found, and why. The preference rule for understanding sentences that can be conceived of as or as containing, proverbials (i.e. prefer their idiomatic sense), when applied to proverbials produced on a story completion by a story recipient (a placement itself having a structural basis), shows that to be a class of utterances with the potentiality of puns. We happened to isolate that class out of an interest in puns, but the class exists independent of any consideration of puns insofar as pro-
verbials systematically occur on story completions.

That 'on story completion' is a systematic position for proverbials can now be addressed directly, albeit more discursively than the state of the research requires. First, by reference to a general rule that provides that one should not tell one's co-participants what one takes it they already know, stories can be peculiar tellables. That is, one can tell a story whose import one takes it is 'a known' one's recipients already possess, the telling being occasioned by virtue of the story events being a bit of news that happened to the teller. One can, that is,
report a commonplace event that just happened to one, where one wouldn’t otherwise assert that such a thing happens, it being of course known to one’s co-participants that it happens and what it means when it happens. Furthermore, to tell recipients such a commonplace event may put them in a doubly peculiar situation. For they too should not tell what they know their co-participants know, and therefore they shouldn’t offer, in the place provided for exhibiting understanding of a story, some form of knowledge which could be heard as merely a preferred explanation or other understanding of the prior story, if, that is, such a piece of knowledge is presumably known to the teller too. What they need to do is to exhibit understanding without either suggesting that what the story told was news to them as knowledge, or seeming to be pejorative by treating the teller as not knowing what the story events mean. This they can do by using some form of talk which indicates: I understand, and already had that understanding before; and, furthermore, I take it that you had the understanding too. A detectable proverbial is precisely that sort of known. Its operation in this regard however turns on the use of the preference rule requiring idiomatic interpretation: Hearing the proverbial idiomatically is part of how one hears that its utterance understands, or is on topic with, the story it succeeds. It is, then, built for proverbial interpretation, though as a sentence or sentence part it is amenable to concrete interpretation. Thus, a user will have been seen to have selected an item of knowledge that he already had, and one that he supposes his recipient also to possess, and thereby to do the particular job, in his exhibit of understanding, of saying in effect: “I understand this, its point being the known I am reciting.” The import of the preceding is that there is a systematic basis for the occurrence of proverbials as the utterance of a story recipient upon story completions. And because there is a systematic basis for puns occurring in proverbials, there is, therefore, a systematic basis for puns occurring in that slot.

4. Having argued that proverbials can be systematically positioned at story completion points, and that they apply to the story just completed, then the positioning of the proverbial serves to locate some particular prior talk, the story or some part of it, as relevant to its understanding. If the punned-on is in that story we might have a rule relating pun and punned-on. Perhaps the proverbial ‘carries’ the pun to such materials as contain the punned-on. The rule might then be: A pun, occurring in a proverbial positioned on a story completion, will have its punned-on in the story the proverbial’s utterance understands, the story which needs to be used to understand the proverbial itself, on this occasion of its use. Note that this rule is consistent with the foregoing instances of story post-positioned, located in proverbials, puns. For neither of those need we be satisfied with saying, as might have been imagined, that the punned-on occurs ‘somewhere’ prior to the pun.
Note too that the rule might be generalizable to (or turn out to be a specification of) a rule that makes no mention of proverbials or stories: A pun finds its punned-on via any rule that finds the talk that the utterance containing the pun uses to have that utterance understood. Or, in other words: An utterance may require for its understanding attention to other utterances, but not any other utterances. Any rule or procedure which locates those other utterances needed for the understanding of some current one, will locate the materials punned-on, if the current utterance contains a pun. That utterance may itself be so constructed as to mark (extend) the range of prior talk to which the current utterance, with its pun, is directed. The generalization, when applied to story post-positioned utterances (whether or not they can contain proverbials), engaged in exhibiting understanding of a preceding story, nicely handles such further materials as the following fragment, which is from another session of the same group therapy. The pun-containing utterance, while following a dirty dittie, is built to assert without explanation an understanding of talk for which the dittie is but a last in a sequence which has included dirty jokes and other such things. The fragment runs partially as follows:

Al: [((pitched one-half step higher)) Down in Nagasaki where the women chew tobakki
Ken: hhh hhh
Roger: heh heh heh! hhh hhh
Al: hh hh hh
Roger: [heh heh heh
Al: ([half sung]) The men- when the men go wikki wakki woo.

( / )

Ken: hh
Al: Where the-
Ken: [Where the women-
Roger: [O.K. I must know a funny story youh I mea(hh)n hehh
Al: [((sung)) Ba::rney // Google
Roger: [Outta my colorful life hahh // hehh
Ken: [hehh
Al: [((sung)) Ba::rney Google with his goo-goo-googuley eye::s.
Ba::rney Google hadda wife three times his size,
Ken: ehheh
Al: [((sung)) She sued Barney for deevorce,
Now he's living with his ho::::::rse
Ken: heh heh hh
Al: [((sung)) Ba::rney // Google-
Ken: [heh heh

( / )
Roger: Did he buy the horse before he got divorced?
Dan: Well so far, all of you skirted around the subject, that see(hh)ms to b(h)e predominantly uh on your minds at any rate,
( ): ((clears throat))
Ken: heh heh
Roger: Hmnyeh well we’re at that sta(hh)ge.
Ken: ehh // heh hehh
Roger: [hehh hehhh hehh // hh hehh hh
Dan: Yeah?
(Al:) yeah.
Dan: What stage is that,
( /+) 
Roger: Awareness.
Ken: heh heh
Roger: hehh hehh hh hh
Ken: [Hey wha’ does that look like to // you?
Dan: ha ha ha ha ha ha ha ha // hhh
Al: (That got ’im off it, ) (I din’ know)
( / / / ) 
Ken: heh heh heh
Al: I- don’t know.
( / / / ) 
Al: My mind is not nasty anymore.
Ken: heh heh heh heh
Roger: Yer never nasty when yer talkin about sex,
Ken: hh
Al: It’s wonderful.

We are interested in Dan’s pun-containing utterance, “Well so far, all of you skirted around the subject, that seems to be predominantly uh on your minds at any rate.” Note that it too might be seen to skirt around the topic it locates as having been being skirted around. It exhibits its understanding without saying what the topic is, though the pun of “skirted around” employs that topic; and, what he says they are alluding to, they acknowledge they are alluding to.

The position Dan uses for this utterance is one that he could use, without marking his utterance specially, to say something like what he does say about Al’s last dittie. Now, it happens to be the case that ‘utterance front words’ are commonly used to position an utterance in more complex ways than the sheer placing of an appropriate sort of utterance brings off, a matter that we cannot here go into detail about. With his use of “Well so far, all of you,” Dan can extend the sequence his remark is intendedly after, it happening that the consistency of Al’s talk with much of what preceded it permits Dan’s indications of a
perceivable similarity to be used to find both the talk it applies to, and an appreciation of what the similarities are. As Dan so extends the sequence his talk understands, so, the pun in it does apply to the extended sequence, and not just to Al’s last dittie.

The aim of the foregoing discussion was to show, retaining for purposes of control materials which involve exhibits of understanding that are story post-positioned, but which do not involve proverbials or which involve understanding more than one prior story, that perhaps the generalization of the rule relating pun and punned-on will nonetheless preserve that what carries the pun, carries it to the punned-on via a rule that locates what the utterance that carries the pun applies to, or positions itself after. Note well that throughout this paper we have used ‘positioned after’ in terms of located structures. Sheer subsequence is not the issue. Were that what we were pointing to then the utterances we talk of as ‘after’ something might equally well be said to be after the last word or the last phrase, or the last hour that preceded them. We use ‘positioned after’ only for sorts of objects as have some characterizable analytic relation between them.

Given the initial version of the rule and the generalization, we are now in a position to note: What is of particular interest is that the rule(s) may turn out to be derivatives of a rule that itself says nothing at all about proverbials or stories or puns either, but that provides, for some to-be-specified classes of features of utterances, the relations between features of an utterance and features of some prior talk are made available by speakers to hearers, through speakers’ ways of using a collection of positioning techniques to locate the talk to be used to understand their current utterance. Whereas puns, as we noted at the beginning, are of no special theoretical importance, what this rule talks to is a central area in the analysis of the sequential organization of conversation.

NOTES

1All transcripts used in this paper were produced by Gail Jefferson. I am, as usual, indebted to my colleague Emanuel Schegloff for a great many clarifications of what I must have had in mind.


3While I am not here considering ‘obscene puns’ because they need a special treatment, I might note that they surely do occur in just such a position as we have been isolating. On an occasion I was witness to, a psychiatrist was reporting on a patient of his, a promiscuous homosexual. On a recent occasion, he said, the patient had come in to his
office beaming, and had reported that he was now in love, involved with just this one fellow. The psychiatrist reported himself to have responded, intending to forewarn about a possible failure, “Don’t put all your eggs into one basket”, a proverbial which in its obscene pun is distinctly apposite to the homosexual situation.

Note, with regard to our proposal about the use of story postposition for an exhibit of understanding, that quoted materials, as here, are particularly nice evidence, for the quoter apparently takes it that the proverbial will be seen by the recipient of the quoted sequence as such an understanding, which involves the recipient in using the reported positioning in the way we claim recipients of so-positioned proverbials use newly occurring ones to see that they intend an exhibit of understanding of the preceding story.
HISTORICAL DIMENSIONS
IN THE SOCIOLOGY OF LANGUAGE

JOSHUA A. FISHMAN

Yeshiva University

My basic proposition today is that since the sociology of language is ultimately an attempt to augment our understanding of and impact upon one important sector of social behavior, it might well benefit from closer contact with history. This proposition follows from a simple argument, namely: if societal dimensions are needed—as I believe they are—to productively understand the sociolinguistic facts-of-life, then surely historical dimensions are needed to productively understand the sociolinguistic facts-of-life. The contribution of history to knowledge is not my invention. It has been obvious to ever so many others before me, and, indeed 'knowing', 'to know' is the basic etymology (or history) of the word history [Gk. historia, from histor or istór, knowing; from the root eidenai 'to know']. Thus, if I stress an obvious point today, it is because it has not received the attention it deserves in our fast-growing field.

There are all sorts of quips about history (pro: not to know history means to be condemned to repeat its mistakes; con: the only thing that history teaches us is that it is not possible to learn anything from history), but I would like to urge upon you a non-quip in arriving at 'some sense of what sets history off from other social sciences. The contribution of history is perspective. This is no small matter' (Landes and Tilly 1971:2).

Since our discussion here is very far from being conducted in a vacuum, since we are very much aware that the world round about us is seething and writhing in the grip of all manner of problems, and that language issues are related to other issues in very complicated and crucial ways, my concern for history—rather than being diminished—
is all the greater. Many of us are often more concerned today than we were a decade ago with having an impact on social problems, with contributing toward their solution, just as we are more concerned with the applications of knowledge more generally. It is in this very context that I wish to concur with the recent ‘Survey of history as a social science’ (jointly sponsored by the National Academy of Sciences – National Research Council and the Social Science Research Council) that ‘never is the perspective of history so valuable as when men try to shape their destiny, that is, try to change history. Then, if ever, man has to know how he came to this pass; otherwise he is condemned to repeat his [own] errors, or, at best, to blunder through one difficulty only to arrive at another’ (Landes and Tully 1971:2).

However the basic blessings of history, I would maintain, are intellectual and esthetic, rather than activist ones, per se, and it is with this view that I approach history in the remarks that follow. As a long-standing fellow of the Society for the Psychological Study of Social Issues (and as former editor of its Journal of Social Issues), on the one hand, and as a firm believer in the applied sociology of language, on the other hand, I nevertheless frankly give primacy to understanding and to appreciating as I view the constant interaction and interstimulation between the cognitive, affective, and conative aspects of social reality. Perhaps this very bias, a cognitive one, leads me to suggest that there are three historical dimensions (rather than just one) that must concern us in the maturing of the sociology of language.

1. Sensitivity to the dimension of historical depth. The first of these, clearly enough, is the dimension of historical depth per se, i.e. the time perspective that deepens our understanding of and appreciation for any particular sociolinguistic topic. Our discipline has produced many excellent examples of studies that have profited from the addition of historical depths to their synchronic emphases. Without in any way seeking either to make invidious comparisons or to be exhaustively complimentary let me name a few such to illustrate what I mean.

Einar Haugen’s work comes to my mind first of all, perhaps because I used his Norwegian Language in America (1953) as the required text for the first course that I offered in the sociology of language in 1958. Haugen’s productive penchant for history is equally evident in his Language Conflict and Language Planning (1966) and I am confident that it will be there, clear and strong, in his forthcoming treatment of the Scandinavian languages as an interacting family. I do not think there is another scholar in the entire field of sociolinguistic concern who has given history its due as much as he has. He has frankly been my reference model in this respect, in my own still maturing efforts to recognize the living historical dimensions in those topics to which I have given recurring attention (1968, 1971, 1972).
But Haugen has not been alone of course. Every single one of my colleagues at this session has savoured the fruits of history. W. H. Whiteley has done so repeatedly in his treatments of East African language-in-society topics. However, I envy him most for his little volume entitled *Swahili: The Rise of a National Language* (1969), just as I envy those of you who have not yet read it but who will now do so, for you have a great and rare treat ahead of you. The same is true of the work of Heinz Kloss. Indeed, I have seriously recommended to serious students that they learn German if only in order to read his *Die Entwicklung neuer germanischer Kultursprachen von 1800 bis 1950* (1952), and I hereby nominate it as among the non-English books which the sociology of language needs most to have translated into English. I also want to stress the historical component of Prof. Das Gupta’s admirable treatment of *Language Conflict and National Development* (1970). Rarely does one become as impressed (or depressed) by the endless complexity that history forces upon current language problems as when one reads his treatment of Indian issues, with their centuries and even millenia of emotional and intellectual challenges to every current act.

I should really also mention Ferguson at this point; but I will save him for later (and perhaps more fitting) consideration. I should also mention many others, but since I make no attempt to be exhaustive I will mention no more.

Let me now sketch a few operational characteristics of what I consider to be truly exemplary use of the first historical dimension in sociolinguistic research:

(1) The dimension of historical depth is important even for (or precisely in) studies that are not basically historical per se. The live hand of the past is constantly with those who are on the center of the stage today. Our current protagonists live with history, or think they do, or want to, or object to doing so (and thereby merely reveal all the more the true grip of history upon their emotions and rationalizations and behaviors). We need not be historians or social historians to appreciate this fact (however much we might wish we were in order to grasp it more fully); but grasping it, we must either incorporate it in our research design or deal with only a pale shadow of the social reality that we seek to understand. The interpenetration between language behavior and other societal behavior, when understood most illuminatingly and provocatively, touches upon language- (or variety-) boundary behaviors, group- (or network-) boundary behaviors, overt and subjective loyalty and opposition to cultural values, legitimization and rejection of established intergroup relations—and all of these involve the incorporation and interpretation of history—not merely on our parts, as researchers, but frequently also on the parts of the protagonists of the dramas that we are trying simultaneously to portray, to analyze, and to explain.
It is true, of course, that when viewed as such, history like any other dimension of human behavior, is present differentially. It is expressed or reflected in the behavior of some networks all of the time and in the behavior of all networks some of the time. Even more, it is expressed or reflected with different weights in the various behaviors of those very networks in which it is recognizable at any particular time. ‘History is perspective’, which means that it is primarily social-interpretational, social-evaluational, social-rationalizational behavior, and, of course, not all social behavior is of that kind. However, much important social behavior is precisely of that kind and not to tap it is a great pity; for it makes our subjects out to be decidedly more one-dimensional, mechanical and over-determined by immediate forces than they either are or believe themselves to be.

(2) If my above stricture boils down to the view that real social behavior is frequently informed by historical perspective and therefore, social research must also be similarly informed, then my second stricture, to which I now turn, boils down to the view that historical perspective in the sociology of language must itself be informed by social research considerations. Social research (and here I am thinking primarily of empirical and, if possible, replicable and, if possible, quantitative social research) on participants in the social here-and-now must have its own dimensions, its own parameters, its own hypotheses, and its own methodological alternatives. The sociology of language cannot afford being reduced to history and to history alone (not even to social history). Certainly, it cannot afford being reduced to any single historical dimension such as chronology pure and simple.

If there is anything that marks intellectually sound and stimulating social research it is its toughness with respect to (a) the definition of the problem or phenomenon to be explained, as well as with respect to (b) the empirical orthogonality and theoretical coherence of the most parsimonious set of simple dimensions required for such explanations. These two characteristics indeed, make good social research very much like good research of any other kind. As a result of its own ground-rules (such as the foregoing) social research can fail as well as succeed, it can conclude that it has been barking up the wrong tree rather than necessarily be a self-congratulatory enterprise, it can attempt to pass public scrutiny before a panel of doubting Thomases rather than merely satisfy the corroboratory instincts of investigators who have lived with their data so long that they ‘just know’ that they (and it) are right.

Thus, while I agree that ‘life is history and history is life’ I also insist that history itself must be interpreted and understood, and that the current social behavioral validity of our interpretations and understanding must not merely use history (i.e. the investigators’ and the actors’ interpretations of history as an aspect of living social reality),
but it must test its use of history, it must revise its own views of the extent and nature of the 'live hand of the past', it must dimensionalize chronology and operationalize, empiricize and quantify the historically embedded dimensions tentatively favored at any particular stage of its ongoing explorations.

Historical depth in the sociology of language thus promises to be more difficult than the writing of history per se, and that, of course, is difficult enough. Good history has always been more than accurate and exhaustive chronology (with its discomfitting ability to tell us rather more and yet rather less than we would like to know and understand). Good history consists of ideas, their interrelationship, and their claimed validity (or face validity) in the light of all of the evidence out of which these ideas have been extracted. However, if we are dealing, as we frequently must be in the sociology of language, with current social behavior and with the current validity of history in that behavior, then our historically embedded dimensions must not only be extracted from the past but they must also be cross-validated in the present.

Does American immigrant history ‘teach us’ (i.e. imply, suggest, lead us to hypothesize) that without the maintenance of a relatively clear-cut functional diversification of codes, i.e. without overt and conceptual diglossia, language shift is likely to occur, to the end that the language associated with more powerful roles will displace the language associated with less powerful ones for those functions in which both are employed? In that case the sociology of language today, here and now, can test this ‘teaching’, can modify it, can set its limits. Does the history of the modernization of Hebrew ‘teach us’ (i.e. imply, suggest, lead us to hypothesize) that planned lexical elaboration succeeds in being accepted by ultimate target populations only when the (re)distribution of scarce social rewards is controlled by authorities who engage in such (re)distribution on the express basis of demonstrated utilization of newly elaborated lexicons? In that case, the sociology of language today, here and now, should not only be able to test this ‘teaching’, somewhere, but should be able to tell us whether such factors as age, sex, education, income, and ideology are in any important sense modifiers of the general teaching, (first in a particular historical context and, ultimately, more generally than that).

All in all, therefore, I conclude that the sociology of language cannot and should not escape from history, but, rather, that it can and should improve on whatever it is that history purportedly teaches us, by testing it, refining, precising and contextualizing it. In order to do so the sociology of language must go ‘on beyond chronology’, into the empirical data of current social behavior, and must do so via a variety of methods not all of which are historical by any means.

2. Sensitivity to the dimension of historical breadth. There is more to history, however, than I have mentioned thus far. History is
not merely diachronic, that is comparative across instances of time, but it is also often comparative across instances of place, of culture, of population, etc. Indeed, the comparative method as such is the basic device of historical research and theory, and the greatest historians from Herodotus to Toynbee have been frankly and eagerly comparative. I certainly consider comparativeness to be a further historically relevant dimension of considerable sociolinguistic interest and importance.

Once again, many of my friends here today are themselves first rate advocates and practitioners of the art and science of comparative sociolinguistic research. In addition to the names that I have already mentioned before, mi lanu gadol miGumperz, miGrimshaw u’miHymes? However, it is precisely Ferguson whom I would like to single out in this connection because, to my mind, no one represents quite as diversified an array of comparative sociolinguistic interests as he does. His historical examples are drawn from Christendom to Islam; within Christendom from its Western to its Eastern and African branches; within Islam from its Magreb, Near Eastern, and South Asian branches; from literate, literizing, and preliterate societies; from adult, adult-child, and child societies; from pre-modern, modern, and post-modern societies; from huge to tiny speech communities; from technological to religious development; and in each and every instance his observations with respect to language in society are innovative, intriguing, and, not infrequently, inspiring. How fortunate for all of us and for the sociology of language as a whole, that he has been concerned with this field and that our lives and minds could interact with his.

I need not go on at length about the comparative historical dimension within the sociology of language, because much of what I have said about history as depth also applies to history as breadth. It (history as breadth) informs our work and our understanding and does so bountifully, almost intoxicatingly. On the other hand, it too calls for verification, specification, and delimitation by the other types of data, as well as by the other methods of data collection and analysis, that are available to a broadly gauged sociology of language. Indeed, if I am to add anything to my former remarks in turning briefly to the issue of historical breadth in the sociology of language it must be to stress how meager and halting has been our progress along this path. The comparative sociolinguistic history of mankind still lies before us, virtually untouched, to mine, to map, and to magnify! Dare we take the plunge? Are we too young, too immature to try...or does the magnitude of the challenge enoble the challenger?

I know of only one sociologist of language today with a wicked enough gleam in his eyes to openly tackle something of such enormity as the comparative sociolinguistic history of the Western World and of Western Europe in particular. ‘Thank God there is only one!’ do I hear some of you say? But I say, ‘Thank God there is at least one, and may he
prosper!’ I am referring to the Catalan scholar, Lluis V. Aracil (now visiting Stanford for a year, on an ACLS grant, after having visited Yeshiva for a year during 1970-71), and, most immediately, to his paper ‘The (pre-)history of sociolinguistics’ presented at the September 1971 13th International Congress of Romance Linguistics and Philology (at Laval University, Quebec). It is clear even from his brief paper that the comparative social history of the successively attested and enthroned national vernaculars and supra-national languages of Westerndom not merely fascinates him (as it has and does so many others) but that he has worked upon the problem quite intensively. He defines his field as the history of Western sociolinguistic-thought-and-practice; his method is that of the sociology of knowledge; his goal: to relate sociolinguistic thought to sociolinguistic practice in all walks of Western endeavor: political, educational, literary, etc., from Charlemagne on. May he be blessed with the necessary long life and great strength, to match his wisdom and enthusiasm, in order to bring his labors in this topic to a successful (i.e. to an integrative and stimulating) conclusion.

However nearly all other great comparative sociolinguistic topics with historical sweep also remain to be tackled: The successive Aramaization, Hellenization, and Arabization of various (but not all) near eastern populations, on the one hand, and the successive Latinization and de-Latinization of Western European populations with respect to diverse spoken and written functions, on the other hand, may both be substantially beyond reconstruction; but, were they not (and perhaps they are not entirely so), I would be thrilled to view the Russianization of various Soviet populations (some with and many without major written traditions of their own), the Anglification of various North American populations (some of whom could have treasured religious and literary traditions of world-wide repute, instead of just coke and rice-crispies), and the Hispanization of various Latin American populations (whose between-group diversification is yet to be fully appreciated) in terms of a single, powerful theoretical-historical perspective spanning more than two millennia. Grandiose? Of course. Impossible? Perhaps. Foredoomed to failure. Not really. Indeed, I would argue that any such attempt—provided it does come down to current behavioral reality in terms of empirical data stringently collected, analyzed, and interpreted—is assured of being an immortal classic, both in conceptualization as well as in execution. It will, of course, be ‘ripped to pieces’, but so are most petty works, and for better reason, as well as with less envy and more malice on the part of the rippers.

Perhaps I am a hopeless romantic after all, but when I read Labov on language change I see beyond Martha’s Vineyard, beyond the Lower East Side, beyond Spanish Harlem, together with most of you, to other places and other times. I am not sure what times and places occur to those of you with historical sensitivity, but I see the situational
(contextual) and demographic variation of Latin in ancient Gaul, as well as the situational and demographic variation of any one of a large repertoire of old German or Old Slavic varieties in ancient Central and Eastern Europe. Most clearly of all, I see the contextual and demographic variation of Germanic speech among formerly Loaz-(Romance) speaking and always Hebrew-Aramaic praying Jews in the Lorraine region (which they called Loter) in the first centuries of the current millennium when what we now call Yiddish was born. I wish I knew in which kinds of networks contextual variation was greater than demographic variation and in which it was the other way round. Some of the variation was doubtlessly due to pre-Loter speech habits, so it was presumably most available to those most recently from 'the Old country', those who visited it most frequently for business or family reasons, or those who were visited from there. Additional variation derived from differential contact with Loter's non-Jewish society (as well as with that of other nearby regions), including its upper strata. However, although the linguistic variables that marked off the speech of Jews from the speech of non-Jews all had their originally demographic correlates they also came to have within-group contextual implications as soon as particular cooccurrences came to be associated with the enduring social-psychological dimensions that are always extracted from demographic differences: powerlessness vs. powerlessness, formality vs. informality, distance vs. intimacy, role-differential stressing vs. role-differential masking, etc. When and among whom did which kind of variation develop most fully and most rapidly? From my readings about this period I developed hypotheses concerning the time when Loaz was still far more widespread in Loter (and, therefore, probably revested both demographic and contextual variation) while the newly acquired Lotertaytsh was still fairly restricted (and, therefore, revested primarily demographic variation). I tested these hypotheses concerning societal 'bilingualization' on a Puerto Rican population in New York (Fishman and Herasimchuk 1969) and was much gratified by their confirmation, both substantively and procedurally.

I see the comparative study of languages-in-contact in Labov's work. I see pidginization and creolization. I see language maintenance and language shift. I see the establishment of societal bilingualism. Frankly, I see all of the topics in which I am interested—but I see them simultaneously in historical depth and in comparative breadth perspective, as well as in terms of testable hypotheses among current populations. Granted: If my hypotheses are confirmed 'today' I cannot as conclusively demonstrate that they were also valid 'then'. I can only use the here and now to hypothetically illuminate the past. Until I reconstruct H. G. Well's time-machine, that is certainly little enough of a quid-pro-quo given the wealth of currently testable hypotheses and proto-theories that the past has provided for me. I also make no
assumption in mining history (or comparative history) of linearity of societal development. I am quite aware, e.g. that secondary modernization is quite different from primary modernization, both in rate, in dislocation, and in prospects (Hoerning 1969/70). But even discontinuity hypotheses derive from historical perspective and sensitivity and imagination—and these are qualities from which the sociology of language (and sociology as a whole, and linguistics as a whole) could profit immensely.

3. Intra-disciplinary sensitivity to history. Disciplines are not merely 'establishments' (i.e. protected ways of viewing, doing, and interpreting) but, as a result of being establishments, they also have their decided blind spots (i.e. favorite ways of not doing, not viewing, etc.) and hangups as well. For us, in the sociology of language, there are special problems concerning doing or using history, and it is to these that I would like to turn, in closing. Since I have little time left, I will characterize these problems as claiming, on the one hand, that history should not be engaged because it is non-objective, i.e. under-demanding vis-a-vis scientific pursuits, and, on the other hand, that history should not be engaged because it is over-complicated, i.e. over-demanding. I am willing to grant both of these charges. Indeed history is very much like the rest of life in both of these respects, and I am very much in favor of going on living and historicizing.

To the charge that history is underdemanding (non-objective, non-scientific) I would answer that good history is not uniquely more or less so than are the other social sciences more generally. Every social science—yes, even linguistics—indeed, every human pursuit—has been put to subjective ends. As a result, every social science—yes, even linguistics when it is a social science—must face the problem of verification and evaluation of evidence and interpretations. Independent verification and multi-method verification by researchers of various moods and temperaments are ultimately our only safeguards. We typically work with probability statements concerning the power of particular social-behavioral dimensions as explanatory dimensions, and if viewed in this light, good history does not at all strike me as qualitatively different from sociology, anthropology, or political science. Be this as it may, however, I am essentially interested in mining history rather than in doing history, in deriving perspective from history, alternative explanatory parameters, comparative insight. For these purposes the argument that history is insufficiently objective or scientific is particularly unconvincing, because in addition to not being true it is also not to the point.

The second contra-history argument ostensibly takes a completely opposite course. It claims that history is too complex, too multiply-determined, too unique to fathom in and of itself, let alone to fathom
in terms of generalizable principles. However, I see nothing unsurmountable in this criticism of history that does not apply equally tellingly to ethnography, ethnomethodology, or to any other clinical or ideographic research, and the sociology of language has certainly benefited from each of them. They, like history, hold that their complexity is a good rather than an evil, that their dependence on individual insight, immersion and sensitivity are not merely inescapable but desirable if the goal is 'to see things whole'. But this is exactly what Landes and Tilly say about history. 'Insofar as history attempts to see things whole', they say, 'it is more dependent than other disciplines on individual perceptions. Interpretation and understanding are never routine; there are too many variables to reduce the analysis to some kind of procedure. Hence it is important that each scholar dig down to bedrock...immersing...[himself] in a particular time and place until...[he] absorbs its ethos, its rules of action, its everyday routines' (Landes and Tilly 1971:2-3). Certainly this is 'ethnography at a distance' and certainly it can be good.

Both sociology and linguistics have been burned or bruised in the past by prematurely grandiose historical endeavors, and az me brit zikh op mit heysen, blozt men af kaltm (or, once burned twice cautious). However, one can and should be cautious without being traumatized, particularly if the possible gains are in any way comparable to the possible risks. In our case the possible gains are great indeed. The sociology of language, in our day and age, has been very largely an American discipline, particularly insofar as most of its American participants are concerned. As such it is particularly exposed to the risks of historical shallowness and of comparative innocence. There is a world of time beyond our barely two hundred years and a world of language in society beyond that found from 'sea to shining sea'. History can help bring these worlds to the sociology of language. 'The contribution of history is perspective. This is no small matter'. Certainly it is no small matter for the sociology of language on the occasion of its being alive and well in Georgetown...and, essentially, all of ten years old.

NOTES

1On leave, 1970-72, as Project Codirector, International Research Project on Language Planning Processes (and, simultaneously, Project Coordinator Israeli Section) and Visiting Professor, Hebrew University, Jerusalem.

2There are three reasons why I consciously refer to 'The sociology of language' (although I do use the adjectival form 'sociolinguistic'), rather than to 'sociolinguistics', as I have at times in the past: (a) to draw to it greater attention from sociologists, (b) to make linguists
realize that they should beware of their ignorance of the science of social behavior, and (c) to stress that the total enterprise is not for the purpose of enriching or reforming or revolutionizing linguistics, but, for the purpose of understanding and influencing language-in-society as such.

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Language planning refers to a set of deliberate activities systematically designed to organize and develop the language resources of the community in an ordered schedule of time. If the community feels that the task of language planning is of sufficient salience to demand the authoritative attention of the political system, then it becomes a matter of public policy. The manner in which communities intimate public authorities regarding the necessity to undertake the planning function is usually too diffuse to permit the planners to grasp the specifics. At best one can say that this normally does not amount to anything more than a broad intimation of preferred terminal goals. The task of decomposing the terminal goals into a sequential set of realizable objectives and the ordering of priorities among these objectives in terms of a balance of resources and capability calculated by the authorities, is generally assumed to be the primary burden of a plan.

Planning, however, implies more than a composition of a plan. It refers to a continuous process involving the sequences of plan formulation, conducting the planned set of activities necessary for the realization of the objectives in a programmed time schedule, and preparing the planning system to steer itself according to the information fed back from the addressee of the plan output. Assuming that the context of the plan is the whole society, we are then considering the national political authority, which normally attends to the general concerns of the society, as the overarching coordinator of the planning process. By virtue of this assumption, the arena of language planning within the universe of national planning can be seen as a competing area of planning impinging on the national resources having alternative allocative claims.
Public policy

It is in this context of relative sharing and interdependency of the multiple sectors of planning that the proper place of language planning in a particular society can be understood either in configurative or comparative terms. Once this is granted, it will be easier to follow the process of language planning. We will further assume that the process itself can be decomposed into a series of specific episodes. Any act of planning can be broken down into a series of policy episodes just as each episode itself can then be studied in terms of its constituents and relations. The obvious advantage of treating the process of language planning in this manner lies in the fact that the translation of planning in public policy terms brings us nearer to the actual practice of language planning and at the same time it sensitizes us to the string of constraints that impinge on it. In addition, this manner of analysis will help us distinguish the properties of public planning of language from non-public efforts to plan language.

Public policy can be studied in many ways. One can choose to focus exclusively on its content or one can study the policy process emphasizing the manner in which a policy is formed and implemented. In real life, it would be difficult to treat content and process separately. If there is a choice of focus, it is more in the sense of relative emphasis for analytical convenience. For our purpose, we will choose the process alternative partly because of space constraint and partly due to our interest in highlighting the course of salient policy episodes and the events constituting those episodes, such that they yield a dynamic outline of the practice of language planning in a particular society.

Policy process in India

Policy formation: Language has been a subject of public policy long before India's independent status as a modernizing political community. But language policy making and implementation of the kind discussed above is obviously a post-independence phenomenon. In this sense policy process pertaining to language belongs to an innovative class of policies rather than the inherited class. The properties of the innovative class were defined by the uniqueness of the developmental responsibility and the national scale of planning involved in discharging this responsibility. Again, within this class of policies, language policy issues claimed a special uniqueness due to two factors: the normal difficulty for political authorities to intervene in a cultural realm, and the intense association of one dimension of the language question with the issue of national integration. In other words, unlike the issue of economic policy, here was a case where the experience and skill of the policy-makers were of relatively lesser utility in defining their role
and delineating their task with the degree of confidence that they normally bring to bear on more familiar grounds.

At the initial stage of policy formation, policy was crudely communicated in the form of nationalist intents. Its negative intent was clearer than its positive content. English was to be replaced by Hindi. During the national movement several ambiguities clouded this objective. In part, this ambiguity was functional for the preservation of the unity of the movement. As an anti-colonial move it was of no great import if some thought of this objective as merely symbolic, some assumed it was nothing less than a quest for a common language to unify the nation, and others inferred that Hindi will be a language of official transaction at the federal level of administration. However, as the movement came closer to the seat of national political power, the range of choices was successively limited so that the last option came to prevail. A settlement was reached on the floor of the Constituent Assembly through a process of struggle, bargaining, and compromise.

Constitutional policy formation was limited to a statement of choice of the federal official language and its script, a phased time schedule for the change over to the official language, a declaration of freedom to the constituent states of the federation to choose their own state official language, and announcement of the federal government's responsibility to promote the spread of Hindi, and a declaration of a general guideline for the development of Hindi. At this level policy goals were stated with a greater degree of clarity than before and an outline of the instrumentalities was suggested for the implementation of the objectives. What is equally important is the fact that the responsibility for developing Hindi was assigned the federal government and a guideline of development was adopted in the Constitution of 1950.

Policy elaboration for implementation

The function of the constitutional declaration can be seen as a record of consensus on a body of general language goals, enumeration of public responsibility for achieving these goals, and an implicit incentive to language planning for the coordination of the explicit language tasks under the auspices of the central political authority. Each of these steps needed subsequent elaboration through relevant policy structures so that proper authorities could be created to handle the problems raised by the general declarations—thus specifying general goals into specific objectives. Instrumental sub-goals needed to be defined, offices and roles were to be devised, lines of action were to be laid down, and control systems were to be instituted.

All these functions presuppose the willingness to recognize the existence of a body of information pertaining to the problems of official language policy with special reference to the available language resource
and the proper manner of developing this resource. The responsibility of preparing this body of information was assigned to an advisory committee of experts composed of scholars and reformers. The Report of the Official Language Commission of 1956 was considered by Parliament and in 1958 a Committee of Parliament examined and revised the policy recommendations of the Commission. On the basis of this review, the Presidential Order of 1960 gave directions for the implementation of the recommendations.

What is interesting from the perspective of the policy process is that despite the fifteen year deadline there was no noticeable hurry in implementing the constitutional proposals. If the constitutional document gave the impression of urgency, neither the legislative nor the executive authorities seemed to share that sense of urgency. In the order of national policy priorities, there were other pressing issues on the agenda which gained the major share of attention. Also within the legislature, the support for Hindi policy was less extensive than what was expected before Independence. Anyway, the level of intensity was lower than expected too.

The low salience of the federal official language issue for the national policy makers in the fifties was also, in part, due to the high salience of the politics related to the issue of reorganization of the states within the federation along linguistic lines. Language politics during this decade was confined to specific regions where the basic conflict was intra-regional rather than national. Fortunately for the national political authority, the timing of these regional conflicts was staggered throughout the decade and not cumulatively reinforced at one point of time. But these conflicts did demand an important part of the national authority's attention. The share of attention gained by these demands perhaps accounted for the relative loss of policy energy for the promotion and development of Hindi.

Policy structure

By the beginning of the sixties, the linguistic reorganization of states was largely accomplished and the policy planners at the national level were once again able to concentrate attention on the Hindi question. The Presidential Order of 1960 acted as a formal incentive for the implementation of the official language policy. Under the Indian system of policy-making, an order of the President implies an initiative of the cabinet which combines both executive and legislative leadership in one body. Constitutional directives are implemented by the parliamentary system through the cabinet led by the Prime Minister. The cabinet members are invested with the executive responsibility of managing their administrative departments and the cabinet as a body is collectively responsible to Parliament. Being in charge of overall policy
planning for the federal government, it is the cabinet that formally de-
cides which sector of policy deserves what measure of priority and
allocation of resources. As for the expected mode of operation, it is
assumed that, given the limited volume of national resources and alter-
native demands on them emerging from competing policy sectors, the
policy planners normally tend to arrange their scale of priority in such
a manner that it satisfies their norm of ‘good enough’ solution. Thus
the demands of the language sector are sought to be treated in the con-
text of other sectors just as within the language sector the state of
competition of rival demands tends to influence the relative share of re-
sources commanded by the policy planners. By resources, we mean
not merely the physical resources but also the political resources of
influence, authority and institutional capability accessible to the policy
planners in a given time and situation.

By 1960 the policy planners realized that only five more years re-
mained out of the fifteen year deadline for changeover to Hindi as the
official language and that nothing much had been done in the fifties in
this direction, so they resolved to increase the pace of organized public
effort for Hindi promotion and development. From the very beginning
the Ministry of Education within the national cabinet was assigned the
major task of implementing the objectives of language planning for Hindi.
However, during the fifties the Ministry of Education, due to the con-
straints discussed before and due to the personal lack of warmth for
the cause of Hindi on the part of the Minister, mainly relied on allocat-
ing grants to voluntary associations working for Hindi. Organizationally,
the Ministry of Education itself remained content with the creation of a
small unit of its secretariat called the Hindi Section which was subse-
quently raised to the status of the Hindi Division. The strategy pursued
in the fifties was one of routine coordination of the work assigned to non-
governmental agencies and the Section’s own work. Most of this work
was directed to preparation of scientific and technical terminology and
promotion of Hindi. The Hindi Division occupied a minor position in the
Ministry’s work and its funding represented a minor share of the Minis-
try’s budget.

A series of protests emerging from the Hindi voluntary associations,
increasing pressure from the pro-Hindi legislators and the plain fact of
inadequate preparation for the change-over made the national planners
aware of the need to create proper institutional structures for stepping
up the pace of work. In 1960 the Central Hindi Directorate was created
under the auspices of the Ministry and in 1961, the Commission for
Scientific and Technical Terminology was added as a separate but re-
lated organization. The working budgets of these organizations are
much larger than their predecessors’ and they enjoy a larger degree of
independence within the administrative setting of the Ministry of
Education.
The functional division between the Directorate and the Commission is relatively clear. The Directorate is responsible for the general task of Hindi promotion. It initiates and controls programs for development and propagation in several ways: translation of books, coordination of book programs including dictionary and encyclopaedias, translation of administrative manuals and documents; coordination of Hindi teaching and examination systems, standardization of keyboards for typewriter, teleprinter, conducting information centers, incentive systems for authors, publishers, journalists, students, and a variety of other related tasks.

The Commission, on the other hand, is exclusively devoted to the production and control of scientific and technical terminology. Each organization has its own publication program and both work in close cooperation with the specifically educationally oriented organizations like the Hindi institutes, university cells, training centers, etc.

Proliferation of organizations and expansion of work in each of the organizations often create a complex problem of coordination and this overall responsibility of coordination and control is vested in the Language Division of the Ministry of Education. However, important sectors of Hindi promotion and development work lie outside the control of this ministry since they are under the auspices of the Ministries of Home (interior), Law, and Information and Broadcasting. Inter-Ministry coordination systems have been instituted. Substantial work is done in the Hindi states and control systems of the central cabinet are not always sufficient to coordinate work at the state level. Periodic consultation and informal coordination seek to take care of the problems at that level of operation.

Organizational problems

A large part of the work outlined above is of a kind which the organizational resources of a routinized bureaucracy in a modern state may find rather hard to cope with. In the modernizing organizational context of India the organization task is still harder. The organizational structure operative in the two decades after Independence is basically an inherited structure. The colonial inheritance implies that it was created and sustained by a set of norms congruent with the purpose of colonial administration. These norms put a higher premium on regulation for the preservation of the status quo than on productive development for planned innovation according to these norms, since the outcome of innovative measures is less predictable than that of routine behavior. Apart from the continuity of norm, the administrative structure of India is manned by a personnel whose training, socialization, and gratification expectations appear to favor behavioral continuity rather than innovation involving adventure and developmental enterprise.
It is not surprising that the novelty of the task of language planning caught the administrative system unprepared and puzzled. The response was typical. The initial mood was one of delay and delegation to outsiders as far as possible. When direct responsibility was thrust on the system, the response was organizational proliferation in an incremental manner. As the number and size of the language related organizations increased, recruitment of personnel posed another problem. The available generalists were unsuited for the specialized function of guiding the work of language planning. Men with professional expertise and commitment were sought to be recruited through a process of nomination, bypassing the normal system of recruitment.

The higher echelons of the Directorate, the Commission, and the Institutes are staffed by people who by training, inclination, and commitment are more specialized and appropriate to the task of implementation of language policy. Most of them are confident of their expertise, and information concerning the language sector. Some of them have participated in the past in the voluntary language associations and many of them have a high degree of moral commitment for the job they are doing. As the number and status of such personnel keep increasing, they resent the higher status and power of their superiors in the Ministries whom they view as generalists incapable of leading the process of language planning. Due to differential organizational perception, morale, and personnel between these new and old structures involved in language policy process organizational conflicts are generated. In this conflict, as the last decades' experience in India indicates, the Hindi-related organizations suffer from one limitation. Above and below them are personnel who are parts of the regular administration and thus owe loyalty to the established structure.

The Language Division of the Education Ministry enjoys a higher rank in the control system pertaining to language policy planning than the Hindi agencies of the Ministry. It is the Division that evaluates the work of the Hindi agencies and their personnel. To the extent that the Hindi agencies are divided within and among themselves, to that extent they also fail to put pressure on the Division, or, what is more, fail to work out a winning coalition with the Language Division against the other Divisions within the Ministry of Education.

Policy analysis

The above outline of the processes of policy formation, levels of policy operation, and the problems related to the organizational environment of policy implementation raise many important questions concerning analysis of public policy related to language planning. For a configurative analysis of the Indian policy process related to Hindi planning, it is important to note that Hindi planning is a part of the
order task of language planning undertaken by the national political authority. In this case the relative share of Hindi planning within the context of Indian language planning and the relative share of the latter in the context of national developmental planning in general should be taken into account. For a comparative study, this aspect can be subsumed under the question of span of language planning and will help us in differentiating the Indian plural process from the ones where language planning is more unitary. Secondly, our outline indicates that the general policy system and the decision rules affect the outcome of language planning. An analysis of the systemic components and their relations governing the policy process may be helpful in understanding the present and the prospective shape of language planning. Thirdly, the policy process can be understood better if we distinguish between classes of policies and then subdivide classes into types. In the Indian case we noted the rationale for dividing the policy realm into routine and innovative developmental classes, and saw how the one impinges on the other or how they remain in a conflicted relationship. Within each class policies may be distinguished by their predicated functions typified as regulatory, productive, distributive, or in other pertinent ways. The usefulness of these constructs will be apparent when one seeks to order his information on the complex policy process in a manner that will help him to explain the impact of this process on various components of language planning.

Some theories of language planning are cast in the form of normative proposals of ideal planning, some in the form of an unarticulated growth model where any conscious input injected in the language scene directed towards developing or promoting a language is assigned the name of planning. From the vantage point of social analysis these trends do not help us in understanding the process of change of the language scene. If, on the other hand, we study language planning as a delimited set of policy processes, then we would be in a better position to analyze the sector of planned change as compared to the sector of unplanned language change and development.

NOTE

I have avoided citing source materials and relevant literature since this paper is primarily an outline. The basis of my Indian material is drawn from my field work in India in 1964, 1967, 1968, 1969, and 1970-71. One part of my early work is presented in my Language conflict and national development, Berkeley, University of California Press, 1970. From 1968 I have been involved in the Comparative Language Planning Processes Project with Charles Ferguson, Joshua Fishman, Bjorn Jermudd, and Joan Rubin. This paper freely draws on what I have learned from my project colleagues. For this particular paper I
am specially indebted to Bjorn Jermudd, and Charles Ferguson at Stanford University, and Ralph Retzlaff and Ramashray Roy at the University of California.
I should like to introduce my paper with two quotations from recent articles (1966) by Charles Ferguson. The first is as follows:

The fact remains that the availability of accurate, reliable, information on the language situation of a country can be influential in making policy decisions and is of tremendous value in planning and carrying out the implementation of the policies.

and the second:

It is assumed here that a full-scale description of the language situation in a given country constitutes a useful and important body of data for social scientists of various interests.

These constitute two kinds of justification for carrying out a sociolinguistic survey at the national level; they may, of course, be regarded as—and perhaps ideally are—complementary, but they need not be, and since they may require different kinds of research-workers for their implementation, more usually probably are not. It is evident that the first quotation embodies a view of sociolinguistics which underlies much recent work (Shuy and Fasold 1971) and which is probably also influenced by political considerations. My own experience of such surveys is limited to the recently completed Survey of Language Use and Language Teaching in Eastern Africa, which carried out surveys at this level in Uganda, Kenya, Ethiopia, Tanzania, and Zambia, and
involved the deployment in each country of two full-time research workers, a team leader, and various part-time research workers, for periods of up to one year. As a result of my participation in the Survey in Kenya I should like, first, to consider very briefly to what extent the first of the above objectives can be realized in the present socio-political climate, and secondly, and at greater length, to raise some of the problems that are involved in making statements about the language situation of a country at the national level, assuming that a 'full-scale' description of this kind subsumes a comprehensive study of language behaviour.

It must be recognized at the outset that there was little relevant experience on which the Survey could draw in the field of language-planning as far as Africa was concerned, though the experience of planners in Norway, India, and Turkey might have given us cause for scepticism. The newly independent states of Africa might lean heavily on expatriate economists to create, modify, and recreate their National Development plans; but they had not hitherto invoked the expertise of linguists, and, as was suggested on more than one occasion at Survey Council meetings, the view that '...the finding of suitable answers to language questions... is of crucial importance in their economic, political, and social development' (Ferguson 1966:1) was one which was largely formulated--however imaginatively--in expatriate terms.

So the objective embodied in the first quotation was written into the proposal for the Survey and remained, for many associated with the project, an important goal. Yet, allowing for the facts that language choices may not 'set quickly and decisively' and that they are not determined 'simply on the lines of rational analysis' it has still to be remarked that even by the time the Survey was being planned all the nations involved had been operating a set of language choices for some years, whether accompanied by planning or not, and it was noticeable that co-operation between the Survey teams and Governments was most fruitful in precisely those cases where choices had been most explicitly made, and where the Survey teams were able to provide the service of documenting and evaluating the implementation already achieved. Einar Haugen once commented (1966) that in the history of the world all the successful examples of language planning had had the participation of linguists, and some years earlier the Leverhulme Conference on Universities and the Language Problems of Tropical Africa (1963) had recommended that linguists should participate in fields where information was needed on which practical decisions concerning language could be taken. So much can be accepted, but when the decisions were made, the opinions of linguists were not invoked. Of course, history may subsequently appear to substantiate Haugen's claim, but it will be a difficult case to prove. Linguists may identify problems within particular communities yet fail to persuade the members of their relevance from their point of view.
The decisions were taken on political grounds and in conformity with particular political ideologies, but coloured inevitably by particular configurations of historical forces. Tanzania's choice of Swahili as a national language was in part a gesture of independence, in part a reflection of her socialism and her concern that all levels of the population should be involved and able to understand the objective of Party policy, given that the great majority of Tanzanians could hope for no more than primary schooling. In Kenya, where socialism is differently conceived, the de facto adoption of English as an official language in many sectors is in conformity with an essentially elitist ideology, where the country is administered by a professional Civil Service, not very different from that inherited from the Colonial Government, and clearly separated from the Party. The whole question of the place given to making decision on language choices within the total scheme of political decision-making demands attention. As Colin Leys has recently pointed out '... speaking generally, political leaders do not want one particular goal beyond all others. Like the ordinary people whose affairs they try to manage, they have a multiplicity of goals they want to achieve and most of the time they are pursuing them all more or less simultaneously'. (Leys 1971:110) There have to be priorities, of course, and in different cases it is evident that the political returns on deciding to use a particular language in a particular sector may not be commensurate with the risks involved. Thus a decision to use English at one level may need to be offset by a decision to encourage, for example, the teaching of the vernacular at the University. Even where the main lines of a policy are clear, there are still opportunities for decisions to be made on political grounds to trim the approach, e.g. the establishment of a National Swahili Council in Tanzania in 1967-68 in opposition to the University based Institute of Swahili Research. At this level the presence of a political scientist in a survey team would be welcome, always assuming that the socio-political climate were not positively hostile to this.

The Survey proposal listed three important spheres in which decisions relating to language questions must be taken: national unity and identity, access to modern technology, and international communication. To these one might add a fourth, internal communication. The second and third of these pose no great problem at the moment; all the East African states having opted, formally or informally, to continue using English. The first, however, is another matter, not least because the term 'national' is variously understood and realized. For lack of suitable alternatives Uganda's Ministers and former President, Milton Obote, used English on national occasions; in Kenya some Ministers use English and some, like the President, use Swahili, which he has referred to on at least one recent occasion as the national language; in Tanzania Swahili is used on all national occasions. So far as internal communication is concerned, while it may be possible for a bureaucracy using
Swahili to function efficiently in Tanzania, in Kenya it is English that is used as the medium for official communication from the centre through the Province, down to the District level. At the intra-district level between officials and between central government officials and people the local vernacular is used. In Uganda a similar situation obtains though Swahili is a less efficient articulator between the lines of communication coming down from the centre and the situation at the periphery. (Criper and Ladefoged 1971) In all these spheres the major decisions had been taken.

To sum up, I do not believe that in the present socio-political climate it is realistic to imagine that sociolinguistic surveys are likely to be influential in making policy decisions. Indeed I think it could be argued that it would be presumptuous to entertain such an expectation, bearing in mind that the kind of problems posed by these multilingual nations, require that special training be given to potential language planners of a kind not hitherto available. On the other hand there are no such objections to describing the language situation, and I should now like to turn to a consideration of the extent to which this is a realistic objective at the national level.

Here again, it must be recognized that there were rather few precedents for such an enterprise, and little basic data available, e.g. in Kenya very little language data is provided by the Population Censuses of 1962, 1969. As the first Survey team went into Uganda, Joshua Fishman was finishing off his two-year study of a Puerto Rican neighbourhood in New York and the experience and insights therefrom benefited us all; but Fishman was at pains not to minimize the difficulties of handling even so small a universe as a neighbourhood of 400-odd persons, and left no doubt that these would be magnified as one’s universe expanded. Whether it would be possible, as he envisaged, ‘...to generalize from individuals to entire neighbourhoods or countries...’ (Fishman 1968: I, 9) in half the time, had to be discovered.

As an introduction to the discussion I present a sociolinguistic profile of Kenya (Table 1), obtained by applying the criteria set out by Stewart (1968). This is not intended as a criticism of Stewart’s approach but rather as a device to throw into relief some of the problems that the survey of a multilingual country like Kenya entails. The profile of necessity simplifies to a considerable extent, thus:

1. Language-type. The problem of a ‘standard’ form for Swahili. While a standardized form of Swahili was used as a model in education and elsewhere during Colonial times, both in Kenya and Tanzania, there has been a tendency during the past twenty years to assert and develop a local standard in each country. In Tanzania this has mainly been the result of language planning in the period since independence. In Kenya it has largely arisen as a reaction to other models which co-existed, of which the most important were the following:
TABLE 1. Sociolinguistic profile of Kenya (After Stewart 1968)

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The population of Kenya was c. 11,000,000 in 1969.

Classes represent percentage of users within the polity. Upper case letters refer to language types, thus S = Standard; V = Vernacular; C = Classical; P = Pidgin. Lower case letters refer to language function, thus: o = official; p = provincial; c = capital; g = group; r = religion; w = wider communication; l = literary.

For a fuller discussion of the symbols see W. A. Stewart 'A linguistic typology for describing national multilingualism'. In: Readings in the sociology of language, ed. by J. A. Fishman, The Hague, Mouton, 1968.

(a) The coastal varieties of the language with their literary and historical traditions (Vgl), influenced by Arabic phonology and lexicon and by dialectal features (e.g. the dental/alveolar, aspirate/unaspirated oppositions in stops) characteristic of other Swahili dialects—even in Tanzania—but not generally elsewhere in Kenya.
(b) The very low-status varieties used between Asians/Africans; Europeans/Africans ($P_1$ and $P_2$) to mention the two most important varieties. The low status of the language as a whole in some areas is in part related to the recollections of the settings in which such varieties occurred and to present awareness that they still persist.

(c) The 'very difficult' variety of Swahili heard over Radio Tanzania. Recognizing that in rural Kenya children’s exposure to Swahili in the pre-school period will vary very widely, not only quantitatively but qualitatively and also that their subsequent use of the language is also likely to be very variable, the Kenya Institute of Education has been engaged over the past few years in producing Primary School Courses which aim at a somewhat simplified version of the former standard, which keeps Arabic influence to a minimum at both phonological and lexical levels and which seems to aim at a relatively modest level of competence. It is rather early yet to assess the impact of this new standard on usage.

Where children have access to a radio, this is likely to be their most persistent source of exposure to the language, except on the coast; but here too the models are very varied: from the announcers from the coast whose first language is Swahili to their colleagues from up-country whose Swahili sometimes appears to have been translated from English and bears an unmistakable intonational imprint thereof.

Thus in Stewart’s terms the standardization of Swahili may be said to be both polycentric (i.e. different sets of norms exist) and exonormative (i.e. based on foreign models of usage); but the situation is probably not susceptible to formulaic generalization at the national level. The important point is that the term Swahili covers a number of linguistic varieties which differ in social status and function. Whether the concept of a ‘continuum’ as developed by the Creolists is useful here is not certain, though the situation described by DeCamp (1961:61-84) for Jamaica, where individuals of different status in different parts of the country control different spans of the continuum, shows many similarities with the Swahili situation in Kenya, though the possibility of measuring the discreteness of the many varieties by some of the currently available techniques (Bailey 1971:341-48) remains to be carried out.

2. Language functions. The use of ‘o’ and ‘p’. It is not possible to assign ‘o’ to any language as ‘a legally appropriate language for all politically and culturally representative purposes on a nationwide basis’ unless one clarifies or refines the rubric somewhat. In broadcasting, for example, many of the major vernaculars are used to reach speakers across the whole nation, and this is inevitably so where social mobility has resulted in perhaps 25 percent of Kikuyu and some 12 percent of Luo being resident outside their home areas. Smaller numbers of other groups are similarly placed. Again, most of the major vernaculars are used at district and sub-district levels for some official purpose or other
depending upon the local situation, the nature of the occasion and the heterogeneity of the speakers. They are also used in the courts.

It must also be noted that the language situation in education is not revealed by such a profile. English, in more than half of the country’s schools, is the medium of instruction throughout the system. In the remaining schools it is the medium of instruction after the first year or two. Swahili is taught as a subject right through the system, including the University, except during the first year or two in rural schools. Some basic skills are still acquired in the vernacular in those schools which are not following the English-medium approach; but apart from this the vernaculars are not encountered again during the educational process until the University, where courses are planned in the major vernaculars along with the study of Linguistics.

3. Degree of use. The difficulty of using a national Class rating can be illustrated with reference to English and Swahili. To say, for example, that English is used by 30 percent of the population obscures the fact that this may represent a figure of say 80 percent of those living in towns, but only 25 percent of those living in rural areas. Nor is the very wide gap between the generations and the sexes revealed; thus in a sample of Kipsigis, 77 percent of the men under 30 claimed competence in their first language, Swahili and English, while only 4 percent of those over 30 made such a claim. On the other hand the incentives to acquire and to demonstrate competence in English are very great, since it is from this source that economic and social status are believed to flow. Many young Kenyans who are being encouraged to use Swahili by the Party are quick to point out that those who currently enjoy both wealth and power did not acquire it through the use of Swahili.

Again, the figure for Swahili conceals the fact that Kenya comprises an economically little-developed coastline with almost 100 percent Swahili speakers, including most of those who use it as a vernacular; an economically highly developed central area where perhaps 50 percent may be Swahili speakers; and large areas in the west and north-east of the country where fewer than 15 percent of the population may be Swahili speakers. Some consideration ought also to be given here to level of competence: a good deal may be inferred from an account of the settings in which particular languages are used, since for each setting there is probably a minimal ‘threshold’ below which effective communication cannot take place. On the other hand there are a number of settings in which it is not competence so much as social appropriateness which determines the choice of language. Gumperz has given many illuminating examples in his writings, and our experience in Kenya has provided ample confirmation of this, particularly in encounters between members of the public and government officials. Thus, a man wishing to see a government officer about renewing a licence, may state his request to the girl typist in Swahili as a suitably neutral language if he
does not know her. To start off in English would be unfortunate if she
did not know it, and on her goodwill depends his gaining access to au-
thority reasonably quickly. She may reply in Swahili, if she knows it
as well as he does and wishes to be co-operative; or in English if she
is busy and not anxious to be disturbed; or in the local language if she
recognizes him and wishes to reduce the level of formality. If he, in
return knows little English, he may be put off at her use of it and
decide to come back later; or, if he knows it well, he may demonstrate
his importance by insisting on an early interview and gain his objective
at the expense of the typist’s good will. The interview with the officer
may well follow a similar pattern, being shaped on the one hand by the
total repertoire available to each other, and on the other by their re-
spective positions in relation to the issue involved (Whiteley 1972, Ch.
12; Parkin 1972, Ch. 8). Yet it emerged from our sample surveys
that there were in any case a substantial number of people whose com-
petence in both English and Swahili was below the threshold for the set-
tings being considered and probably restricted to isolated words and
phrases. Even the settings themselves may prove an unreliable guide
to usage: typically, at District centre level and above, retail trade was
in the hands of Asians, and trading was carried on between them and
the local population in Swahili; below the District level, it might be
noted, numbers of Asian traders learned enough of the vernacular as
was necessary to carry on their trade. As the policy of Kenyanization
takes effect and Kenyans (especially Kikuyu) move into this sector, and,
moreover, as the educational system turns out an increasing number of
school-leavers with increasing competence in English, there is some
evidence that English is being favoured by the younger and Swahili by
the older generation.

Finally, the diglossia situation and the absence of it raises a number
of interesting points which cannot be generalized in terms of a single
national pattern:

(a) In some rural areas there appears to be a situation of stable di-
glossia with bilingualism, restricted usually to men, in which a ver-
nacular is L and English H. Where women are not monolingual, they
may use Swahili rather than English.

(b) In other areas, especially nearer the coast, the pattern for both
men and women is to use the vernacular as L and Swahili as H.

(c) Within the framework of Government service there is an interesting
complementarity of work-language where status differences are involved.
Here, while English is retained as H, Swahili is used as L.

(d) An increasingly common phenomenon among the younger genera-
tion, now that both English and Swahili are taught from the Primary
School upwards, is what might be termed triglossia, with Swahili occu-
pying an intermediate position between English and the vernacular. ¹

Thus English is used with agents of Government, especially those
senior to oneself, e.g. in hospitals, Police, Post-office, Administration, etc.; Swahili in trade, bars, and with agents of Government of equal or junior status to oneself; the Vernacular in and around the homestead and at church.

The incidence of monolingualism varies widely: from the higher than 35 percent reported in our survey from such diverse groups as Luo, Kipsigis, and Meru, to nil, reported for some Kikuyu and Luyia groups. However, we lacked material from the pastoral peoples of the north and north-east parts of the country where the incidence of monolingualism is reported to be high.

Two quite general comments remain to be made. 1. Within the context of a single national profile it is not possible to see the contrast between the urban and rural areas. While probably no more than 8 percent of the total population lives in towns, they include Nairobi, the capital, a city of more than half a million people, the seat of government and centre of prestige. It is linguistically heterogenous, where the rural areas are homogeneous; while the vernaculars dominate the rural areas, it is Swahili which is the common man's public language of the city. As Parkin has pointed out from his study of urban housing estates in Nairobi what emerges clearly is the persistence of Swahili as an important, growing medium of communication at all status and educational levels, apparently regardless of whether it has been taught at school or not. (Whiteley 1972:Introduction; Parkin 1972: Chapters 5-8).

2. Regional differences are such that it does not seem to be meaningful to describe the language situation except in terms which allow full weight to be given to them. The sort of factors I have in mind are the following:

(a) Demographic. Ethic homogeneity varies widely, in the rural areas, from virtually 100 percent among the larger groups to below 50 percent in some of the smaller; and it seems to be the case that—all other things being equal—competence in Swahili increases with heterogeneity. Area of occupation may also be significant. For example, the Somali and related groups who comprise only 3 percent of the total population, occupy almost half of the total land area of the country in the strategically important northern frontier region, with an average density of between 2-10 to the sq. mile. Monolingual to a high degree, intensely loyal to their language and very sensitive to events in the neighbouring Somali Republic, their importance far outweighs their numbers, and the provision of programmes in Somali together with broadsheets is an important aspect of Kenya's language policy. Finally, pressure of population may be important. This is undoubtedly one factor among many that has contributed to the steady emigration of Kikuyu into other parts of the country, so that substantial minorities are now being created in areas which had previously been very largely homogeneous, thus they numbered barely 1000 in Baringo in 1962, but over 12,000 by 1969.
(b) Socio-economic. Over the period of Colonial administration the different regions followed different lines of development. This was dictated by such factors as the siting of Missions, the alienation of land for European settlement or enterprise, the possibility of developing cash crops, and so on. Some areas became richer than others, acquired better communications and social services, including education. This resulted in differential opportunities for farming, employment, and so on, which in turn provided a range of incentives to add to verbal repertoires. Kipsigis who gained a reputation for service in the Army and the Police acquired skills in Swahili which were of little value to them after their return home; Luo who worked on the railway or in the port at Mombasa also acquired skills in Swahili which were quite uncharacteristic of the rural areas to which they subsequently returned where we noted one of the highest figures for (L1 - English) competence and one of the lowest claiming some competence in Swahili (c. 33 percent).

The above discussion has focused on the kinds of problems that remain obscured by a simple nation-based approach to the formulation of socio-linguistic profiles, and it is evident that any subsequent profiles will need to be multi-faceted if they are to take account of the range of variables encountered. Alternative approaches have, however, been adopted. Fishman and others have utilized the concept of 'speech community', which he has recently defined (1971) as one '...all of whose members share at least a single speech variety and the norms for its appropriate use'. Such a community may be as small as a 'single closed interaction network' or as large as a country. The largest speech community in Kenya by such criteria would be co-terminous with the ethnic group, and smaller communities could be set up in accordance with the purposes of the research under consideration. Fishman goes on to suggest that some speech varieties are referentially, rather than experientially acquired and reinforced and that 'nations' are likely to be speech communities of this kind. He goes on '...the standard variety of a language is most likely to be that variety that stands for the nation as a whole and for its most exalted institutions of government, education, and high culture in general. It is this variety which comes to be associated with the mission, glory, history, and uniqueness of an entire 'people'...'. As will be appreciated from points already raised, it is questionable whether this can be said of either of the two standards in Kenya, English or Swahili, at the present time. What could be said is that for many Kenyans each is associated with a syndrome of values in terms of which the other is delineated, thus those who hold that English is a mark of economic and social status, conferring a desirable form of modernity, and a sense of political unity, may also hold that Swahili is a symbol of lower achievement and status, which offers no sense of modernity and is less effective as a unifying force. In contrast, those who hold that Swahili is an important symbol
of authenticity which confers a sense of unity both within the nation and within the wider East African Community, may also hold that English as a Colonial heritage, has only a limited role at the national level. A third stance is also maintained, whereby both English and Swahili are held to confer national unity, and both are invested with some elements of modernity and authenticity. The difficulties of using this ‘speech-variety’-based definition of community arise when dealing with second and third language competence, and can be illustrated by taking the set of those who speak a variety of English in Nairobi—leaving aside here the question of deciding what constitutes a given variety. It would need to take account of the fact that a large number of the set also share a variety of Swahili, and a smaller, though still very substantial number a variety of Kikuyu. Similarly, account must also be taken of the numbers of Kamba, Luo, Luhya speakers, etc. Of the Kamba speakers a larger number speak Kikuyu than Kikuyu do Kamba, and a similar asymmetry characterizes the Luo/Luhya pattern of bilingualism. If one now suggests that this community might be enlarged to include the set of, say, rural Luo, who also share this variety of English, thus creating a larger community, one runs up against the difficulty that the two sets are underpinned by different qualitative factors and are thus not comparable in terms of both requirements.

Fishman himself stresses, later on in the same chapter, the fact that a basic definitional property of speech communities is ‘density of communication’ and thus approaches Gumperz’s early definition as ‘any human aggregate characterized by regular and frequent interaction over a significant time and set off from other such aggregates by differences in the frequency of interaction’ (Gumperz 1964:137). To base the definition of speech community squarely on sociological criteria leaves one free to examine verbal repertoires, both as properties of the community and of their constituent members. Thus, one might select for examination the repertoires for types of contrasting communities, e.g. urban/rural; pastoral/agricultural; rural work-groups/urban work-groups; manual workers/white collar workers, in different parts of the country and gradually build up a pattern for the nation on the basis of these parameters. At a lower level still, one might examine role-differentiation within the communities. These studies, however, are essentially directed at the relatively small communities, with a high level of internal interaction, from which Gumperz’s own illuminating material is drawn. More, recently, however, Gumperz has amended his definition of speech community thus, ‘... any human aggregate characterized by regular and frequent interaction by means of a shared body of verbal signs and set off from similar aggregates by significant differences in language usage’ (Gumperz 1968:219). This raises some problems. First, all 600,000 Gusii could be said to share a body of verbal signs; but they are not, as a body, characterized by
frequent or regular interaction; the Fishman definition fits better here. Second, if the body of verbal-signs is the defining characteristic of the community, it is difficult to accept the view that ‘...the totality of dialectal and superposed variants regularly employed within a community make up the verbal repertoire of that community’ (Gumperz 1968:230). If a group of employees constitute a speech community, X, by virtue of their interaction through code Y, any other codes employed by the members will be the markers of their membership in other communities, i.e. domestic, religious, etc. and thus individual not community repertoires. Clearly one may start either from a sociologically defined community, i.e. in terms of intensity of social interaction, or from a linguistically defined one, i.e. in terms of shared linguistic code. In the former case one might aim at a characterization of verbal-repertoires across a country for communities of comparably varying degrees of intensity of interaction. In the latter one might work towards establishing the members’ verbal-repertoires in terms of various social variables, e.g. age, sex, education, etc. In either case, it seems to me, such studies will need to proceed in close collaboration with detailed ethnographic studies. The survey has made a good start here with a number of intensive studies of Nairobi: from David Parkin’s work in African housing estates (1972: Chapters 5–8); to Janet Bujra’s study of the strongly islamized community of Pumwani (1972: Chapter 9) and Barbara Neale’s study of a Jain community (1972: Chapter 10). The sample surveys that were carried out in the rural areas were at the somewhat superficial level associated with rather short questionnaires and interviews; but they have revealed interesting patterns of linguistic competence and frequency in the rural areas, and have highlighted a number of interesting problems for future work: i.e. the extent to which the dominance of the vernacular in rural homesteads is being eroded by the younger generation’s competence in English, the position of Swahili in the domain of trade, whether communities in the rural settlement schemes break up into constituent ethnic groups each with its own vernacular or adopt Swahili as inter and intra-group language, whether there are recurrent patterns of language behaviour within Government service which transcend regional differences, and so on.

After a year in the field, and several months of writing up, one acknowledges a better understanding of the range and complexity of the problems entailed in making a sociolinguistic survey of a country such as Kenya. What has been achieved is certainly not a full-scale description of the linguistic situation; nor even a characterization of it in national terms; yet a good deal of reliable and accurate data will have been assembled and made available; some areas of darkness will have been illuminated, which are perhaps of more than local interest, and a base will have been set up from which future workers can explore.

One final point: if what has been achieved so far is not to remain of purely academic interest to a scholarly universe remote from that from
which it derives, it must be integrated into local institutions and used as a basis for further locally-based research. Only in this way will local scholars, operating from within the ideology of their particular governments, be able to influence the decision-making process in the way that Charles Ferguson envisaged in the first of the quotations with which I began this paper.

NOTES

I should like to express my thanks to Dr. Peter Hill of the Institute of Education, University of London; Dr. David Parkin of the University of Sussex; and Professor John Gumperz of the University of California, Berkeley, for reading and commenting on an earlier draft of this paper.

1. The situation seems to be not dissimilar to that described by Fishman (‘Bilingualism with and without diglossia; diglossia with and without bilingualism’, Journal of Social Issues, XXII/2, 1967, 31) for East European Jewish males who use Hebrew, Yiddish and, more recently, English in intra-group contact. In the East African context the importance of changes in the status of both English and Swahili for the overall sociolinguistic picture is such that the recognition of ‘triglossia’ seems useful.

REFERENCES


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[Text continues as provided in the image]
COMMENTS AT FRIDAY PM SESSION

EINAR HAUGEN

Harvard University

My first comments relate to Whiteley's paper: This is a richly faceted and thoughtful assessment of the problems involved in making a linguistic survey of a multilingual country. It would seem obvious that a realistic language policy can only be based on such research. But it is an extremely difficult and expensive kind of research, for it has to collect data not only on competence but also on the use of language and attitudes to language. It is characteristic that in a country like Norway, where language problems are a matter of public policy, no one has undertaken such a survey. Many loose statements are made on it, but no one has taken the trouble to find out how the N people really communicate. The first attempt was made by an American scholar, our colleague here, John Gumperz.

Having committed various indiscretions to paper, I am now in the flattering but sometimes embarrassing position of being open to misquotation. On the principle that even bad publicity is better than none, I proceed to comment in passing on a reference to me in Whiteley's paper (168). I did not claim that linguists had always shared in LP at the level of language choice (selection), though they have (as citizens) tried their hand at it from time to time. I only pointed out that they have always been called in or have volunteered as consultants in the codification process.

My model provides for four aspects of planning, which make up a matrix:

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<th>Form</th>
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<tr>
<td>Society</td>
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<td>(3) Elaboration/Development</td>
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<td>(4) Acceptance/Propagation</td>
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These are, respectively, (1) political, (2) linguistic, (3) technical, and (4) educational.

Das Gupta's very able and persuasive paper attacks a severely limited sector of the LP program. It is his privilege as a political scientist to treat this as an administrative problem, one of allocation of roles and resources. This is very instructive, but it is only a part of my conception of LP. The administrative problem probably could have been solved in India except for certain massive social realities: (1) the unifying value of the English language as an instrument of communication and the training of Indian officials in this language; (2) the economic and social discrimination inflicted at the national level on non-native speakers of Hindi. I find no mention in his paper of the plain linguistic fact of the tremendous language distance between Dravidian and I-E. These are realities which have helped to shape the nature of the administrative problems.

The background of colonialism which is part of the Indian situation is not limited to the third world. Within the very recent period since the French Revolution at least a dozen European nations have been freed from political dominance, and virtually every one of them has or had a language problem: Norway, Greece, Belgium, Iceland, Italy, Albania, Yugoslavia, Czechoslovakia, Latvia, Lithuania, Estonia, etc.

An interesting point was made by Das Gupta for India: Goals are diffuse as long as a movement seeks power, because it is important to preserve unity. The Liberal Party in Norway had the language issue as one of its platform planks in the 1870's and 1880's. But after achieving power in 1884 the reformers split into those who wished to cultivate the Danish-derived norm and bring its written form into line with Norwegian speech and those who wished to replace it with a purely native norm. Similarly in Ireland, the goals of Irish could be adopted by the independence movement and then sabotaged by half-hearted measures after independence, as we can read in John MacNamara's brilliant paper in the recent volume edited by Joan Rubin and Björn Jernudd, Can Language be Planned? (Hawaii 1971).

There seem to be two types of language policy, if one may oversimplify a bit: an overt, official, public one; another covert, unofficial, private. We may call them the French and the English policy—the French have a policy of official regulations, the English one of unofficial regulations. The results are evident in the former African colonies, where standard French is taught authoritatively and has been well learned, English permissively and less well. Indonesia as a Dutch colony was under the French model. India inherited the English hands-off policy ('Leave your language alone') which fitted badly with the real needs of a multilingual country.

Das Gupta comments on the lack of innovative ideas among the bureaucrats of India. In Norway the same problem exists, that the
bureaucracy was until recently recruited from the urban middle class, whose commitment was to the Danish-derived norm, and who therefore fought change with tooth and claw, and who sabotaged the language directives wherever possible. When they did follow them, they did so in a mechanical, counter-productive way.

Das Gupta notes that he has found 'the normal difficulty for political authorities to intervene in a cultural realm'. There is intense argument over whether LP is a political or a cultural problem: the innovators regard it as political, because they see it as a problem of educating the people to know their own good, and this can only be done through the schools, which are owned by the state. The power elite regard it as a cultural problem, because they see it as a question of cultivating and improving the norm as it is, which in effect means elaborating and refining its vocabulary and style to meet the demands of modern life. Both are right for what they wish to accomplish: norm change requires policy decision; norm preservation requires a change within the framework to keep the norm abreast of modern needs.

On Fishman's charming paper on the historical dimension, I have no comment other than to say that I find it hard to take any other stance on social problems than one which takes history into account. It is a matter of intense interest to me to observe that current linguistics, apparently without full realization of it, is finding its way back to history. I am not thinking of what is technically known as 'historical linguistics', but of the generative approach, in which morphophonemic alternations are treated not as patterns in a static system, but as ordered rules which look in most cases exactly like the internal reconstructions that have long been the tool of comparative and historical linguistics. This kind of synchronic history has given a new meaning to 'derivation' and has brought back the old meaning of 'etymological' which before the mid-19th century meant 'morphophonemic' when applied to spelling. So the dichotomy of Saussure has been resolved by a simple terminological fusion—or perhaps confusion.

And now, before I take my seat again, I have a pleasant though self-assumed duty to perform. We are gathered here for a conference under the title of 'sociolinguistics'. Many became aware of this term only when William Bright called a conference under this title at USLA in the spring of 1964. The term had been around; I suggested it in 1951 (in my LSA presidential address) as preferable to Trager's 'metalinguistics' or the anthropologists' 'ethnolinguistics' (Lg. 27. 213). Only from a footnote in Bright's published report (1966) did I learn that the term had been fleshed out in an article published a year after my suggestion. This was Haver C. Currie's 'A projection of sociolinguistics: the relationship of speech to social status', in the Southern Speech Journal for 1952. Last year I secured it through interlibrary
loan. The article struck me as most interesting and imaginative, and I wanted to get in touch with the author and ask him whether he had been following up his early lead. But he was nowhere to be found. Even Houston University where he taught English in 1952 had no knowledge of his whereabouts—they thought he had probably died. Well, just then I received a new book on American dialectology called _A Various Language_ (ed. Juanita V. Williamson and Virginia M. Burke, Holt, Rinehart and Winston 1971) where the article was reprinted (pp. 38-47), and through the editors of that volume I found him. I learned that he was well and alive and living in Austin, Texas. I wrote to him and found that he still entertained a lively interest in the topic. Yesterday a man walked up to me after the evening program and said—'I'm Haver Currie.' So, lo and behold, we have the opportunity here of meeting the first American to use and develop the term which sums up the activities of this conference. Let me quote a few sentences from the article to show that he did envisage what we are here concerned with: 'The present purpose is to suggest, by the citing of selected and salient studies, that social functions and significations of speech factors offer a prolific field for research. It is the intention in this connection to project, partly by means of identification, a field that may well be given the attentions of consciously directed research. This field is here designated socio-linguistics. Attention will be called to certain relevant research done or under way. Possibilities for further socio-linguistic research are, in fact, beyond estimation. Certain data gathered by linguists, sociologists, and specialists in speech call for coordination and mutual implementation.'

'Attention will be directed primarily to the relationship of speech to social status. This is not necessarily to delimit the projected field but to point up a desiderative approach revealed by the fact that in many linguistic studies and in a large array of popular and academic considerations the factor of social status with relation to speech has prominence either by clear implication or direct recognition.' After discussing some work by McDavid and Kurath he sums up, 'this paper has called attention to certain items of research and speculation salient to the consideration of the social significance of English as spoken in the United States. It has emphasized the persisting interest in the relationship of oral English and social status. Several works have been cited which have reflected this interest on the scholarly level, particularly with respect to speech and social class, regional dialects and social status, and national speech and social status. Specifically, a field for quite conscious study here called socio-linguistics has been envisioned, by warrant of work already done and possibilities hardly estimable. The coordination and mutual implementation of data presented by professional linguists, social
scientists, and speech specialists have been called for.’ I’ll ask Mr. Currie to get up and take a bow, and I hope you will give him the hand he deserves.
CROSS-MODAL COMMUNICATION: 
THE REPRESENTATIONAL CONTEXT 
OF SOCIOLINGUISTIC INFORMATION PROCESSING

AARON V. CICOUREL

University of California, San Diego

Sociolinguistics deals with the study of language acquisition and use in socially organized settings. Our claims to knowledge about sociolinguistic settings presuppose implicit and explicit methods we develop to decide what is to be called information when we act as observers or when we attribute meaning to materials obtained from respondents or subjects we research. As respondents and researchers our acquisition and use of language depend on the ways in which we are capable of recognizing, receiving, processing, and generating whatever we call informational particulars. Oral and written language indexes and imposes an immediate gloss over our thoughts and the various sensory modalities we use for processing and communicating information. The representation of information from our thoughts and several modalities by verbal communication stresses context-free expressions, yet everyday exchanges are invariably contingent productions embedded in emergent, context-sensitive informational environments. Everyday social interaction among hearing subjects is a creative activity that utilizes several communicational modalities.

Much of the creative activity of the everyday social interaction of hearing persons is hidden from us because it is tempered by selective attention, constrained by the sequential production of oral expressions, informed by and dependent on short- and long-term memory, and grammatical and conversational systems that organize information normatively. Examining the communicational strategies of the deaf, however, can help clarify and deepen this conception of information processing. The absence of a basic modality like hearing and normal
speech forces us to reexamine our understanding of how language reflects our thinking and memory, and how the latter processes alter any figural recognition or feature extraction that can be said to operate on cross-modal sensory inputs. Deafness in a culture that is verbally oriented means limited access to idealized 'native' oral language normative expressions for representing different sources of information. The context of communication becomes the focal point for generating and locating the meaning of everyday exchanges among the deaf.

The plan of this exploratory paper is to present selected materials from studies of hearing children and deaf adults to suggest how different modalities contribute to the production and understanding of everyday communication. The materials I will present are a small part of a larger corpus of data and do not reflect all of the complexities of the framework being developed, nor the subtleties that can be found in video tapes that are currently being analyzed. The general thesis of the paper is an old one: researchers and subjects face similar problems in representing their complex informational experiences because of their reliance on, and preoccupation with, verbal modes of representation that do not index adequately the interplay between normative social constructions and emergent settings.

Communication in the classroom

A study conducted with several former doctoral students (Cicourel et al. in press) underscored the importance of the interactional setting in the child's learning of classroom exercises and his performance on standardized tests. The verbal and nonverbal activities of the setting were central for understanding the child's grasp of the teacher's educational goals. A basic issue (Cicourel in press) was how the child was able to represent his knowledge of the immediate setting and his retrieval of past experiences in a context of selective attention. In this paper I focus on the common dilemma mentioned above whereby the researcher and the subjects studied rely on verbal representations to index an interactional context whose complexity as an information processing and generating system dramatically exceeds the verbal data base available.

The remarks that follow are based on a video tape of a hearing first-grade classroom lesson conducted by a teacher with five students. The lesson was intended to teach the children how to form subject-verb-object (SVO) sentences using locatives.

An initial problem we face is deciding the kind of stimulus field the children attend and understand and how the teacher orients herself to the children's responses. In examining the video tape I presume much more than is available from the tape itself because I know
something about the classroom independently of the tape. My memory provides background information that is imposed on the film and integrated with my viewing of the tape as I make observational claims that seem to be clear and obvious. Depending on what I think I see or hear and imagine the teacher and children to be doing in the setting captured by the perspectival view of the video camera, I selectively attend information and create judgments in a context that I hope represents what is happening on the video tape in some logical fashion. My description trades on terms I presume will be intelligible and convincing to the readers of the paper.

A brief examination of the dialogue will reveal that the organizing framework for the interaction is more than a sequence of exchanges between teacher and children where the teacher controls the right to speak by inviting responses from individual children. The children seem oriented to their particular circumstances despite the fact that the lesson is designed to be a coordinated venture whereby all children engage in the same task. The lesson begins with the following remark by the teacher.

(1) T: All right, let's take our green crayon and make a line at the bottom of our paper.

The teacher's opening remark is not prefaced by a statement outlining the intentions of the lesson. She does not say:

Children, today we will play a little game. The game will show you how to tell someone what you are doing. The game will also tell someone where different things are if they see you making something with a crayon or pencil or clay or sand or other things. The idea of the game is to make up something called 'sentences' to tell someone what you are doing. Let me give you some examples of what a sentence is like.

I could go on and provide less formal talk and examples and perhaps try to explain the purpose of the lesson more fully to the children. I am not sure if my brief attempt to introduce the lesson to a hypothetical audience is any more reasonable than what the teacher actually did with the lesson. I want to raise the issue of what it is that we should assume about the child's ability to receive, and recognize, and represent information when we attempt to teach him or her something, particularly when what we seek to teach is (like language) presumed to be known by the child implicitly. The teacher's strategy seems to be one of trying to explicate the nature of the lesson as different stages of it are presented to the child.
The classroom setting needs to be clarified further before proceeding. The teacher and children are seated around a circular table. The teacher's back is close to one wall and she faces the classroom. The camera is directed to the teacher but picks up all of the five children involved in the lesson. The teacher and students have each a sheet of paper in front of them and crayons are available. I cannot clearly see the children's papers but can catch occasional glimpses of the teacher's sheet as she uses the crayon. The teacher tries to monitor the other children distributed throughout the classroom while attending the smaller group of children.

As I review the tape over and over again, I find it difficult to describe what I think I 'see' and 'hear'. I think I 'understand' many kinesic-visual (Birdwhistell 1970; Ekman and Friesen 1969) and auditory nonverbal activities that are 'happening' but find it difficult to represent them verbally for the reader. As I notice the children communicating to one another with glances, one word statements, pointing gestures, nonverbal auditory outbursts, touching each other, and the like, I assume that various kinds of information are being exchanged but I cannot be explicit about the presumed content. The same would be true for the teacher's actions.

The teacher's opening line says to use the green crayon to make a line at the bottom of the page and her remark seems fairly obvious but the children did not automatically pick up their crayons and begin drawing the line. They looked at the teacher and at each other's paper (in some cases) before commencing the activity. Although the children did not seem to object to different terms used by the teacher like 'make a line' or 'bottom of your page', the ambiguity of the instruction was not clarified. Should the line be horizontal? vertical? and where is the bottom of the page? Mehan's (1971) analysis of this setting found this to be a problematic issue. I assume that the children were monitoring the teacher's movements to resolve ambiguities. The teacher assumes that the children not only possess various 'normal' conceptions about particular lexical items and adult syntax, but that the phonological forms she uses are obvious stimuli for the children. Yet her repeating of instructions and frequent glances at the children's papers suggest doubts about the clarity of her instructions. But the lesson was not the first exposure to an organized task nor was it the first day of class. We must allude to an unspecified notion of prior 'background' whereby long-term memory permits the children to attend this particular lesson as 'life as usual' despite some unfamiliarity with the specific task presented by the teacher. Everyone seems to be oriented to the setting as if it were unremarkable and the teacher's use of language assumes that everyone is capable of using some unspecified notion of standardized signals and coding rules for processing the information generated by the teacher. The teacher
repeats herself as the children carry out the instruction she gave them.

(2) T: Take a green crayon and make a line at the bottom.

It is difficult to assess the children’s ability to follow the teacher’s verbal instruction when it appears that they are utilizing kinesic-visual information as a basic frame for monitoring their own, the other children’s, and the teacher’s actions. The video tape is not clear enough for me to assert that the children’s faces reveal frowns that would contradict my claims that the children seem to understand the lesson. The teacher’s possible presumption that her actions are clear and do not require additional clarification is slightly compromised by her repeating most of the initial remark.

Ci, sitting directly to the left of the teacher with a good view of the teacher’s paper produces the following.

(3) Ci: Like this?

With her question Ci looks up to the teacher and seems to be asking for a confirming statement or a glance. The teacher’s next remark seems to be directed to the entire group as she glances around the table, but she also may be telling Ci that her green line is fine (‘yeah’).

(4) T: Okay, yeah, all right, now . . .

The teacher’s visual scan of the group seems to be quick and perfunctory. But notice how my interpretation of the scene I can only partially describe for the reader trades on my unspecified common sense conceptions of what is ‘perfunctory’ about a ‘glance’ and various other meanings that I presume are obvious because of my membership in the society, and my ability to go beyond the oral information given. But these unspecified common sense meanings attributed to the activities are difficult to objectify for the reader but are an integral part of the ‘data’.

(5) Ri: Now what are we going to do?

The remark by Ri seems to presume that the first instruction was obvious and executed properly. But we have no way of specifying precisely how obvious and context-free the teacher’s prior verbal instructions were for Ri or the extent to which kinesic-visual information was central to his actions.
(6) T: Now take your orange crayon and make an orange worm under the green line. We'll pretend that that's grass.

The teacher's instruction can be interpreted as signifying that any discrepancies between her understanding of what is happening and the children's understanding are irrelevant. She assumes that the term 'worm' is clear and thus does not describe what an orange worm should look like, and presumes that the children are aware that the phrase 'that that's grass' refers to the green line. The children do not seem to be responding to this request. The teacher glanced around the table when she gave this last instruction, and she continued to glance at the children as she said the following.

(7) T: [Looking at Ci] It's just a little wiggle. Here, let me show you on this one. An orange worm.

The teacher's remarks are accompanied by her producing a 'little wiggle' with an orange crayon on another sheet of paper rather than the one on which she had drawn the green line. This different sheet of paper seems marked by 'this one' and her comments presume that the kinesic-visual information available to the children is now sufficient for them to follow her instructions. She drew the 'worm' as she uttered 'orange worm'. Je, who was sitting to the right of the teacher, then makes the following remark.

(8) Je: Hey, can you make it on yours?

This remark presumes that Je observed what the teacher had done and ties in with the teacher's reference to 'this one' in (7). Thus, 'make it on yours' is asking the teacher to make the orange worm on the paper that contains the green line. Je pointed to the sheet with the green line. We can wonder if the other children were bothered by this discrepancy or were normalizing it, but the video tape does not help us explain their silence.

(9) T: No, I'm watch . . . watch . . . you make it on, [Je: 'over here?'] make it [pause] [Ci: (tapping on the teacher with her crayon) 'under?'] listen! I'll only say it once. Make an orange worm under the green line.

Can I now argue that the previous discussion of the dialogue whereby I assume that the teacher acts as if her instructions are obvious is now misleading? The teacher's remarks seem to imply that she is replying to Je's 'make it on yours' and Je's pointing ('over here') to the sheet with the green line, when she says 'I'm watch . . . watch . . . '
I assume the teacher is telling Je that she doesn't want to demonstrate each instruction on her own paper, but that she wants to watch the children carry out her instructions on their papers. Ci taps the teacher with the crayon while asking 'under' and I assume she is trying to remember if the orange worm is to be under the green line. The teacher seems a bit irritated when she says 'listen!', and her elbows are on the table as she raised her two arms as if to quiet Je and Ci who are on her right and left, respectively. The doubts expressed by Ci and Je about the teacher's actions may or may not be tied to the next remark by Do.

(10) Do: Like that?

I have no way of showing that the children are attending to the remarks by Ci and Je other than to say that they look up from their papers continually and could have easily seen the discrepancy between the teacher's making the green line on one sheet and the orange worm on a different sheet of paper.

(11) T: Beauuutiful! Okay.

The teacher appears to be looking toward Do when she makes this last remark but does not seem to be looking at Do's paper, but glancing around the entire room. We could assume that the children can all receive this remark as indicating that their efforts are satisfactory rather than inferring that the children are monitoring the exchange between the teacher and Do such that they realize that it is Do that is being told how appropriate her drawing is. There is difficulty here in deciding what the children are attending and how their glances provide informational particulars that might be presumed and indexed by the verbal materials. Perhaps the children are not paying any attention to such particulars. Yet how I direct the reader's attention to what I think I see as relevant particulars structures the kind of inferences that are made.

(12) Ri: I made two orange worms (laughs).

The remark by Ri reveals his understanding of the task at hand but is ignored by the teacher and the other children. Ci is tapping the teacher again here with her crayon and the teacher turns toward her and says.

(13) T: We're going to pretend that that green line is the grass, okay? Can you pretend that with me?
It is difficult to speculate on the teacher's motivation for being redundant about her remark pretending the green line is 'grass'. The teacher looks at Ci when she says 'Can you pretend . . . ' and I think Ci gives her a nodding acknowledgment and might have said 'yes'. There was too much background noise for me to feel certain. Without the video tape we would have to presume the remarks are directed to the entire group. The next remark by the teacher begins to address the purpose of the lesson; to encourage the children to create standardized SVO sentences using locatives.

(14) T: All right. Where is the orange worm, Do?

The teacher's 'all right' may be a signal that the important part of the lesson is about to begin, or it might be a way of telling Ci and Je that it is time to settle down and pay attention to the next remark. The teacher looks across the table at Do waiting for an answer. Here we are forced to assume that the teacher and children 'know' that they are expected to recognize the request as a formal demand for a verbal representation of the information. But the request also could be presumed to be curiously redundant given the fact that everyone has been drawing an 'orange worm' for some time. The teacher does not say what is expected in the way of a response.

(15) Do: Right there.

Both Do and Di seem to point to their papers as Do says 'right there'. The answer seems perfectly obvious within the context of the lesson and the video perspective available. But the teacher is not satisfied with the response but does not indicate reasons for not accepting it.

(16) T: Okay, tell me where he is.

The teacher refers to the 'worm' as 'he' as if this designation is clear, but does not explain that she wants a more elaborate response. Mehan (1971), in an earlier and independent analysis of this scene, notes that the teacher does not explain that the child's initial response is not a complete 'answer'. The implied elaboration suggested by the teacher can be made to appear 'obvious' from the point of view of an adult but we are hard pressed to justify any claims that would argue that the child understands the teacher's remark as a request for a more elaborate answer. But the child's response can retrospectively be seen as an elaboration despite its not being a complete SVO sentence of the form the teacher wanted to elicit.

(17) Do: Under the grass.
The child seems to have accepted the teacher’s previous request to view the green line as ‘grass’. The teacher’s ‘okay’ presumes unstated details are implied and thus allows Do’s response to pass as ‘correct’.

The sequence examined above presupposes but does not make visible ‘interpretive procedures’ presumably at work in the production of the dialogue. The children exhibited a ‘normal’ facility with language that makes it possible for them to represent the task at hand, but their truncated utterances traded on several modalities and presumed an ability to remember and integrate the relevance of key terms like ‘green’, ‘orange worm’, ‘under’, and so on. My interpretation of the dialogue, however, reveals a use of unexplicated interpretive procedures. I must continuously trade on what I think are obvious appearances and remarks while calling the reader’s attention to objects or expressions or movements. Meanwhile I think I recognize, yet fail to call into question, appearances that could challenge a normative or standardized account of what I think is happening. I continuously use immediate appearances available from the video screen to recall retrospectively other elements that have occurred or details available to me from my memory of the original setting. My attempts to use ‘obvious’ informational particulars from the video tape as well as descriptive accounts of information I experienced in the setting are not easily represented by standardized verbal constructions. My own difficulties of trying to find verbal expressions to describe the video tape underscore what I assume is central to the child’s task: to represent his thoughts and understanding of the setting and the task by the use of verbal constructions intended to displace and clarify his experiences. When Do says ‘right there’, the displacement is contextually clear, as would be a pointing operation without a verbal accompaniment. The child’s use of language is context-sensitive and presupposes the relevance of unstated information and reasoning. The lesson is designed to teach adult conceptions about the formation of context-free sentences. But the lesson presupposes the context-free, context-sensitive distinction it seeks to teach and test.

My description of the context is central to how the reader makes sense of my inferences, and my interpretations must presume information that is not available for examination. When we present the child with a task that is available kinesically and visually and indexed verbally and then request or demand that an adult representation of the solution be presented, we presume that tacit use of other modalities is involved, but our evaluation of the response does not make explicit how selective attention and contextual interference are integral features of the cross-modal integration and representation of information.
Representing manual sign language verbally

The hearing person has difficulty comprehending how manual signs are as 'natural' for the deaf as speaking is for hearing persons. It is difficult for the deaf to understand and use speech but their use of signs poses basic theoretical issues that highlight our understanding of cross-modal communication. Linguistically the problem is compounded by the fact that prior and recent studies underscore the lack of correspondence between the word order of oral language syntax and manual sign language as practiced among deaf persons whose first language is manual signs (Furth 1966; Cicourel and Boese 1972; Schlesinger n. d.; Bellugi and Siple 1971; Stokoe, Casterline, and Croneberg 1965). Every attempt to describe the 'syntax' of manual sign language must face the problem of having to utilize oral categories that are normative to oral language.

Sociologists use the term normative to signify tacit and explicit rules that are prescriptive and proscriptive for some group. The reference to such rules is similar to a linguist’s notion of grammatical rules; they are idealized instructions for recognizing or producing some state of affairs that others can implement or accept as 'normal' or 'correct' or 'appropriate'. Grammatical structures in oral languages are powerful but learned rules for representing cognitive activities necessary for attributing and creating order and meaning from everyday experiences. Normative categories are necessary for the assumption that intersubjective communication exists regardless of the differences in meaning, or distortions or assumed or imputed 'errors' that can be delineated by particular observers or participants of some communicational exchange. When linguists engage in semantic analysis, or when students of artificial intelligence construct programs for parsing sentences or for the semantic analysis of sentences, tacit use is made of various kinds of normative categories in the data base used to describe segments of speech. A similar tacit use is made of normative categories that index semantic information contained in a lexicon or dictionary. The cultural meanings employed by the researcher trade on his intuitive knowledge of some native language.

The role of memory and attention is central for any discussion of cross-modal communication, but it is not clear how we can represent their relationship to normative rule structures. We are unclear as to how thought and attention organize and reorganize what is recognized and stored as information and represented by oral outputs that participants treat as descriptions of the 'same' events they presume they have experienced. The verbal outputs that index a particular setting, and the complex interface between our thinking and possible selected sources of information, are difficult to disentangle because talk
indexes itself and our thought processes while displacing other sources of information simultaneously. The problem is also complicated when different sources of information and thought are indexed by manual signs and other bodily movements, but represented by verbal expressions in correspondence with oral language categories and rules. References to the importance of nonverbal communication among hearing persons do not deal with body movements associated with manual sign language usage. The significance of body movements for researchers has often been conceptualized narrowly by the use of a structural linguistics model and not seen as a problem in cross-modal communication.

The ethnographic context

My first task is to show how a hearing person, asking a deaf subject for the appropriate signs that correspond to oral language lexical items that the researcher organized into sentences, is able to record oral descriptive statements about the deaf person's signs. At a later date the researcher should be able to transcribe his descriptive statements and produce the appropriate signs that a third person can act out or can recognize as identical to the original written sentences. The procedure is straightforward and presumes considerable knowledge that a normal hearing, seeing person utilizes but does not recognize as relevant to the production of everyday communication.

Imagine the following scene: a living room occupied by a deaf woman, her deaf child of approximately 3 years of age, and a hearing researcher. The researcher writes out sentences on a sheet of paper in English and asks the woman to translate them into signs. Asking the woman to use signs involves making a kind of circular motion with the two hands (palms facing each other) while pointing to the written sentences. These movements were intended to signify that I was asking the woman to translate the sentences into appropriate signs. But the term 'appropriate' is not clear here. I had also written on the sheet of paper that I wanted her to translate the sentences into the kind of signs she used with her deaf friends, hoping she would give me 'natural' signs, or the signs that the deaf use among themselves. The 'natural' (or native) signs do not have a one-to-one correspondence with the meaning and syntax of oral language English, while another version of sign language (call it second language signing) is in correspondence with oral language English syntax and lexical items.

As I wrote each sentence on the sheet of paper the woman ignored my request for native sign order and instead gave me signs that had second language sign ordering. The word order of oral/written English was preserved, and there was a one-to-one correspondence between each English word and a signed or a finger-spelled version
of the English word if no sign was appropriate. Before presenting the sentences, a few words clarifying communication among the deaf are necessary.

Most deaf persons are trained to read lips and simulate speech and read texts of oral language. The creation of manual sign language has been a centuries-old response among persons born deaf for developing communication with each other. One way the reader can think about this problem is to imagine two deaf persons trying to communicate with oral language. An understanding of their simulated speech depends on how well they can articulate different lip movements with oral language terms. Reading lips is difficult if the person whose lips you are trying to read possesses a different dialect or is a stranger. Because the deaf do not have any auditory feedback from their own lip productions, they can only monitor their own lip movements by examining themselves in a mirror, or by their memory of appropriate articulatory or other activities associated with the pronunciation of oral lexical items.

When a hearing person attempts to comprehend a deaf person's speech he needs as much contextual information as possible to locate the sound patterns. This contextual information (particularly its acoustical features) is only partially available to the deaf person despite the availability of important visual cues. A similar problem exists for the hearing person trying to describe a deaf person's use of sign language. The deaf person may use native or second language signs and in each case embellish his use of signs with paralinguistic information that is further elaborated by contextual cues that are emergent in the setting but primarily of value to the deaf. When the deaf read lips it is difficult for them to sense that a person is trying to be friendly by his intonation patterns, or that a person's voice reveals irritation by what is being said about someone, etc. These elusive judgments of friendliness are represented by other modalities and influence the significance of the signs and paralinguistic information being received. Communication between deaf persons and hearing persons who do not know sign language is similar to man-machine exchanges; there is a sharp reduction in the forms of information that can be conveyed. Even the various gestures both parties use are linked to different language systems.

I attempted to describe the signs I observed, by speaking into the tape recorder and found it difficult to report simple, clear, meaningful movements as generated by the deaf subject. Part of the difficulty can be attributed to my own ineptness in trying to use a descriptive vocabulary that would delineate movements by the two hands, arms, head, and the upper-body area according to specifiable regions of the head, face, upper chest, lower chest, and abdominal region. I attempted to specify many additional details of the movements I
witnessed because they seemed significant at the time, but also because I could not be certain about what was insignificant.

A serious problem that emerged here had to do with the nature of the signs the present (and later) subject used, differences in what was felt to be the appropriate sign for a given oral lexical item. There were differences in what could be described as 'slang' and 'correct' signs. The female subject gave me signs that were translations of sentences written on a sheet of paper and involved difficulties associated with my deciding on the 'nativeness' of the signs. She gave me the same oral language word order I gave her, but her signs seemed to be informal or casual movements I could only describe as 'fluent'. The significance of this last remark should not be underestimated because one native signer can detect another native signer with apparent ease, and a native signer can also detect a second language signer who might be a proficient signer. Similar observations can be made about dialect differences among oral language users. If the signs are native, there will be considerable variation in where the hands, arms, shoulders, and head will move, making it difficult for a researcher inexperienced in sign language to produce descriptive statements about the signs. It becomes difficult to pinpoint the exact location of a movement, and especially difficult to specify when a sign begins and when it ends or where another sign begins.

Specifying the exact location of a sign depends on how well two interlocuters know each other's signing. If they are native signers, this becomes difficult for a researcher because the more native the sign, the more variability likely in its execution, and the more native intuition required to locate the sign in the existing or unfolding context of tacit but multiple sources of information available to a native deaf signer. Good second language signers might miss many of the variations and subtleties generated. Hence, it becomes especially difficult for a novice like myself to pinpoint what I think I am observing because different pauses, hesitations, facial expressions, body movements, all seem important, yet I cannot describe all of them. I observe several sources of information simultaneously, but my ability to describe what I think I see is limited by the sequential production of speech I am capable of emitting.

The subject was given the following instructions on January 12, 1971. The interviewing was done in the subject's home. The instructions were written out.

'Shirley, I need your help in how to translate something into signs that you would use with good friends at the Deaf Club in Oxford'. I then presented the following sentence.

(1) The bear gives the monkey to the man.
I then gave additional instructions: 'Please do not follow the English as it is taught in school, but try and tell me the signs as you would do it for a friend who does not read or write or speak well. Otherwise I will not learn to sign as the deaf do it'. I then gave her the following sentences.

(2) The man saw the boy.
(3) The man is easy to see.
(4) The man is eager to speak.
   (anxious) (he wants to very much)
(5) To see the man is easy.
(6) The man told the boy to leave.
(7) The man persuaded the boy to leave.
   (convinced--talked him into)
(8) The man ordered the boy to leave.

After giving the subject the first sentence ('The bear gives the monkey to the man'), my verbatim remarks to the tape recorder begin as follows:

'Bear, is the two hands underneath the armpits like, alright, circular motion, that's bear.

Bear . . . bear, bear, ah bear!

I'm sorry this is bear, bear is the two fists closed and crossed in front of the chest.

Bear, okay, bear . . . okay, gives, and the two hands go out, gives, the, ah, but look, uh, monkey, ah monkey is the two hands underneath the arms, the two hands underneath the two armpits. The monkey, to, she finger-spelled t-h-e and t-o, to-the-man, and man is the fist [right fist] up against the chin.

[The tape is suddenly silent as I wrote out the instructions to myself about how to sign.

Okay, I'm telling her (in writing) try and tell me the signs as you would do it for a friend who uh, does not read or write, or speak well.]

She's signing to herself what I've just written, alright, to see what I, what I mean.

Signing to herself.
Because, uh (long pause) otherwise I would not be able to sign as the deaf do, is what I just wrote.

Okay.

So now we start over again, okay, now, the first sentence: The bear gives the monkey to the man.

Okay.

[The first sentence was shown to her again.]

Bear gives the monkey to (finger-spells t-o) the man.

Okay, alright, uh, now, uh . . . now here's uh another one, let's see, a second sentence I'll put down will be, uh, let's see; 'The man . . . saw the boy.'

She finger-spells t-h-e, man, and then she did the sign for, then she finger-spells uh uh t-h-e, the-man-saw, she put the . . . right index finger to the eye and made it out, saw [the finger goes away from the eye, but not down, though slightly at the end, as I now recall.]

And then she finger-spelled t-h-e, and made the sign for boy with the right index finger rubbing parallel uh to the chin, not parallel, but yah, uh at right angles to the chin, I should say.

Okay?

[Now she's giving me the correct English normative word order.]

This transcription should give the reader some idea of the difficulties of trying to record orally what is being perceived as various sources of information (including a reaction to my own talk) where an attempt is made to be literal in one's description of an unusual activity. The difficulties I encountered are not due merely to deficiencies in my ability to use English precisely, but to the fact that such description of visual experiences has no clear correspondence with oral language usage. An additional problem has to do with the signer's precision in presenting the signs to me. I am assuming that a 'relaxed' informal atmosphere existed in the subject's home setting, but the reader has no way of checking this out. Because of the relaxed atmosphere I assume existed, I further assumed that the signs were not carefully designed to satisfy some idealized version of manual
signs (accompanied by pictures) often presented in books or dictionaries. The relaxed nature of the signs means that there were discrepancies between the ideal display of a series of pictures and the practical execution of the actual movements. A machine graphics program would be expected to present an idealized manual sign system. If a machine could 'read' video tapes of native signing, the pattern recognition problem becomes difficult in the same sense that it was for me or would be for any human. It is difficult for anyone to recognize sound patterns in a foreign language he knows moderately well when used by natives in relaxed, informal settings. But the deaf pose a more difficult problem because we are dealing with a kinesic-visual system that does not readily exhibit the kinds of systematic structures claimed for oral languages.

Language without a clearly identified syntax

The issue of language without a clearly identified syntax is perhaps strange to a reader whose thinking is accustomed to the ordering found in standardized language constructions. Sign language is based on kinesic and visual information that we force into a foreign oral accounting scheme. The oral accounting frame contains its own logical relationships that we cannot suspend even if we are aware of their constraints during usage.

As soon as we try to explain a language based on visual and kinesic properties said to be organized without an oral syntax conception of rules, we are caught in an interesting dilemma.

We need an organizing framework to describe what we think we see or hear. We employ a framework for oral language that is said to have universal structures (Greenberg 1966), and the system of manual signs does not seem to possess the syntactic structures that are basic to the idea of a universal linguistic framework. There is no difficulty demonstrating how this non-oral manual sign system works. We can make predictions about activities that will take place, indicate the signs necessary to communicate with deaf children without presupposing the child possesses any oral language structures, and then witness the activities carried out that only require us to assent by nodding our heads. Hearing persons can obviously engage in non-verbal activities but they invariably resort to oral accounting that must be described within a linguistic or conversational framework. The accounting process is highly idealized when model sentences are employed. Everyday conversational exchanges produce less linguistic idealization during communication. But model sentences and conversational exchanges miss much of the context indexed orally. Descriptions of manual sign language can clarify the centrality of context in cross-modal communication. Earlier, I briefly illustrated
the problem of producing clear descriptions of manual signs. I now present idealized versions to show how presumed 'obvious' context-free structures can be formed that obscure the informational base.

The different sentences I presented to the deaf subject, and the signed responses I received can be presented in idealized oral language as follows:

(1) The bear gives the monkey to the man.
Finger-spell t-h-e.
Close the two hands into fists and cross them in front of the chest. = bear.

Open the two hands as they are both extended in front of the chest with the arms parallel to the ground about the level of the lower chest. = give.

Finger-spell t-h-e.
The two hands, slightly cupped, underneath the armpits, with the fingers moving back and forth. = monkey.

Finger-spell t-o.
Finger-spell t-h-e.
The right fist underneath the chin. = man.

(2) The man saw the boy.
Finger-spell t-h-e.
Sign for man (see above).
Right index finger next to the right eye, slightly closed fist, with the hand then moving away from the eye directly down so that the forearm is almost parallel to the ground. = saw (actually see).

Finger-spell t-h-e.
Right index finger (but with fist closed) touching the bottom of the chin (but not underneath) and finger pointing horizontally to the left. = boy.
(3) The man is easy to see.
Finger-spell t-h-e.
Sign man (see above).
Finger-spell i-s.
Right fist closed with index finger extended, touching and slightly tapping the right cheek area. = easy.

Finger-spell t-o.
Sign see (see above).

(4) The man is eager to speak.
Finger-spell t-h-e.
Sign for man.
[The subject was puzzled by eager and I substituted 'anxious-like' in its place but this substitution also proved difficult so I tried 'he wants to very much' and this seemed to be meaningful as she proceeded to give me the sign for want.]
The right fist closed against the right side of the chest, moves down while opening the fist so that the palm is against the body as it crosses from the right side to the left (of the person doing the signing) moving down toward the stomach area on the left side. = want.

Finger-spell t-o.
The right index finger extended (with the rest of the hand made into a fist) is brought up to the mouth with the index finger almost touching the mouth and then a slight circular motion is made with the index finger, and the hand is dropped to the lower chest region. = speak.
(A certain amount of 'movement emphasis' can be added to want to signify want very much.)
(5) To see the man is easy.
Finger-spell t-o.
Sign see.
Finger-spell t-h-e.
Sign man.
Finger-spell i-s.
Sign easy.

(6) The man told the boy to leave.
Finger-spell t-h-e.
Sign man.
Right index finger (hand partially closed) placed close underneath the chin and moved away from chin area (away from the body) in a circular motion. = told or tell.
Finger-spell t-h-e.
Sign boy.
Finger-spell t-o.
Left and right palms close to the chest with the right palm covering the back of the left hand partially, both hands moving outward in a sweeping motion, palms up and forearms extended but elbows bent. [Or, as one informant told me, the right hand with thumb extended could be used as go in place of leave.] = leave.

(7) The man persuaded the boy to leave.
Finger-spell t-h-e.
Sign man.
[Subject did not understand persuaded; I then wrote talked him into and she signed talk in-to.]
Right index finger (hand closed) extended is placed near the mouth (points to the mouth) and then moved directly away from the face and body with the forearm ending up parallel to the ground. = talk.
[There are no established signs for pronouns and pronominal reference (except to finger-spell the word). A general strategy for dealing with pronominal reference is to use a pointing gesture in a context to signify someone present and referring to him after he leaves by pointing to the space or location where he stood or sat. Thus pronominal reference can be represented by location in a context when the participants have been talking about someone who was present earlier or who is known by where he sits or some characteristic activity. A simple sweeping motion is used that points to the location or in the general direction of a nearby room that implies someone not present but understood as known to the signers.] Right hand (open but slightly cupped) is put underneath the left hand, both hands palms down, but the right hand moves through the underside of the left hand to signify going in something. = in.

Finger-spell t-o.  
Finger-spell t-h-e.  
Sign boy.  
Finger-spell t-o.  
Sign leave.  
[The sentence signed is also represented in (7A).]  

(7A) The man talked him into, the boy, to leave.
The final sentence (8) is a variation of (7) and involves another complication because of the lack of native signs for the oral language lexical items.
(8) The man ordered the boy to leave.
   Finger-spell t-h-e.
   Sign man.
   Extended index finger of the right hand (fist closed, knuckles on the right side) up against the chin with an up and down motion and then away from the chin area.  [But this is not a correctly defined sign that would be unambiguously understood by native or second-language signers.]
   = ordered.
   Finger-spell t-h-e.
   Sign boy.
   Finger-spell t-o.
   Sign leave.

The reader will have noticed that my attempts to render the oral descriptions of the signs as edited glosses are not always very convincing because the movements implied are often ambiguous. It is not merely the case that I was unfamiliar with British sign language at the time the above materials were obtained, but that oral descriptions (even with time for extensive editing) of visual-kinesic information always presuppose unstated assumptions and meanings that cannot be clearly objectified for someone who has not experienced the setting. But even experiencing the setting does not guarantee consensus or complete agreement, but a tacit arrangement to claim that the 'same' object or event was witnessed. The representation problem is basically a negotiated accomplishment that trades on idealized, context-free rules or norms for creating an important (but nevertheless 'real') illusion of 'similarity' or 'sameness' or 'consensus'. These 'illusions' are socially organized cognitive constructions. Their construction can be partially clarified by reference to the previous materials where the terms speak, told-tell, persuaded-talked him into, talk into-talk, talked-ordered, are used. These terms presumably index related activities for which slight variations in the base signs exist but the base movements can be embellished in several ways by the deaf to communicate other connotations. Or, we can see the variations as a function of my inexperience with signs, particularly how signs are to be described. The materials are listed in Table 1.

My interpretation of what I think I saw produces several variations of what I took the signing to represent as movements. We can attribute this to my noticing different particulars despite an opportunity to edit my descriptions, or we can say that the subject produced natural
### TABLE 1.

<table>
<thead>
<tr>
<th>Sign</th>
<th>Description</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>speak-</strong> (4)</td>
<td>Right index finger extended (with the rest of the hand made into a fist) is brought up to the mouth with the index finger almost touching the mouth as a slight circular motion is made with the index finger, and the hand is then dropped to the lower chest region.</td>
</tr>
<tr>
<td><strong>told-tell-</strong> (6)</td>
<td>Right index finger (hand partially closed) placed close underneath the chin and moved away from the chin area (away from the body) in a circular motion.</td>
</tr>
<tr>
<td><strong>persuaded-talked him into-talk into- talk-</strong> (7)</td>
<td>The right index finger (hand closed) extended is placed near the mouth (points to the mouth) and then moved directly away from the face and body with the forearm ending up parallel to the ground.</td>
</tr>
<tr>
<td><strong>talked-ordered-</strong> (7A, 8)</td>
<td>Extended index finger of the right hand (fist closed, knuckles on the right side) up against the chin with an up and down motion and then away from the chin area. (Ambiguous or incorrect sign.)</td>
</tr>
</tbody>
</table>

Variations because of the colloquial way a native signer communicates. The edited versions given in Table 1 are derived from my rambling and verbatim transcription of the tape. As I formalize the descriptions of what I think I witnessed the terms truncate the several sources of information. These sources of information can be called overlapping variations or they can be made to appear as meaningful differences. The right index finger and hand are central to my descriptions and the ambiguity centers around the location of **speak** or **talk** or **tell**. The index finger pointing to the mouth and then away from the mouth in a circular motion indicates the origin of the activity, but the variations I give include an extended area surrounding the chin. There are additional subtleties and ambiguities such as the configuration of the right hand and the role of the left hand, as well as...
the location of the forearm. The kernel movement of the right index finger pointing first to the mouth area and then away in a circular motion seems clear enough but if the reader tries it, he or she will see that a certain amount of hesitancy is involved, and until you can observe a native signer do it while you try to imitate the movement, you will not feel comfortable with your own efforts. This hesitancy was an integral part of my efforts for several months while trying to learn British sign language. The difficulties I experienced underscore the problem of trying to describe unfamiliar activities (that presuppose multiple sources of information) with categories that require considerable elaboration for their comprehension. I found myself trying to use crisp declarative sentences to describe subtleties that I could 'see' and understand but not articulate clearly. This is precisely the phenomenon of interest in any study of sociolinguistic activities; the standardized, context-free methods we employ for claiming objectivity in the study of everyday social behavior can only index information we process from thought and sensory modalities (and their interaction) by a necessary reliance on our memory and a tacit understanding of what we imagine or feel we experience.

Adequate description is always tied to the practical circumstances of the occasion for reporting or accounting for experiences or observations. My attempt to objectify the classroom setting or the sign language context means subverting my experiences with several sources of information to produce an apparent logical sequence of descriptive statements. I cannot attach a footnote to every statement indicating the subtle kinds of information I intend. Hence my attempts to create 'appropriate' glosses of the descriptions I provide of the signing are similar to the child's truncated expressions when asked by the teacher to 'tell her where the orange worm is'. The child uses a truncated expression that presumes other sources of information are relevant and indeed operative for understanding the significance of the utterance employed. When I attempt to describe the movements of my deaf subject, I create normative categories that I assume are context-free under the assumption that I and others will be able to read or hear my descriptions and reproduce the signs (or carry out some action) initially produced by the deaf subject. The inadequacy of my description is evident from the literal transcription of the tape and from my attempts to reproduce the signs from the transcript. I had to recall my visual experiences of the original setting to communicate my intentions to another signer at a later date, and, as will be seen below, my signing was not entirely successful in producing the written statements I intended when I returned several weeks later and signed most of the sentences to the deaf subject again. Hence all sociolinguistic data are compromised by this representational problem
and require explicit theoretical explication and innovative methodologi-
cal strategies if we are to avoid the dangers of reifying the speech
acts we view as basic data.

The issue of how cognitive systems become mapped into linguistic
representations is not known, but may be partially clarified by noting
the difference in word order between sign languages and oral languages
(Cicourel and Boese 1972; Cicourel in press; Schlesinger n.d.). Some
evidence for word order difference can be seen in Table 2. The written

**TABLE 2: Written representations of native signed sentences by a
female subject born deaf.**

<table>
<thead>
<tr>
<th>Sentences signed by researcher</th>
<th>Subject’s written representations</th>
</tr>
</thead>
<tbody>
<tr>
<td>(1) boy give book man</td>
<td>(1) boy give book to man</td>
</tr>
<tr>
<td>(2) man boy see</td>
<td>(2) boy man see</td>
</tr>
<tr>
<td>(3) easy see man</td>
<td>(3) --- see man</td>
</tr>
<tr>
<td>(4) man see eager</td>
<td>(4) man see lady</td>
</tr>
<tr>
<td>(5) man see easy</td>
<td>(5) man see girl</td>
</tr>
<tr>
<td>(6) man told boy leave</td>
<td>(6) boy saw man left</td>
</tr>
<tr>
<td>(7) man persuaded [pushing,</td>
<td>(7) man --- boy left</td>
</tr>
<tr>
<td>pleading motion] please</td>
<td></td>
</tr>
<tr>
<td>and pray boy go (leave)</td>
<td></td>
</tr>
<tr>
<td>(8) man tell help boy go</td>
<td>(8) man see help boy left</td>
</tr>
<tr>
<td>(leave) (advised)</td>
<td></td>
</tr>
</tbody>
</table>

sentences were given to the subject on 12 January 1971. On 24 March
1971 I presented the subject with sentences now represented as native
sign equivalents. The reader will notice that the first sentence is dif-
ferent from the one presented earlier but retains the same word order
and is consistent with the other sentences given in January. I asked
the subject to write out the verbal equivalents of the signed sentences
I presented to her.

Except for adding to in sentence (1), the deaf subject provided
apparent lexical equivalents to my native signs. The word order of
(2) and (6) contains a subject-object reversal, while (3) deletes a
The signs I used for easy and eager were not clear to my subject, and in (4) and (5) were taken to signify lady and girl. The sign for lady or girl can be made on the same right cheek area as easy and could have been confused with my unclear signs for eager and easy. Clumsy signing on my part could account for these discrepancies.

The discrepancies in (7) and (8) point to problems of using standard SVO utterances or model sentences to establish semantic equivalences between two systems of communication. The equivalences ostensibly established in the sentences in Table 2 are misleading because I forced an oral language framework on the research setting at the outset. Hence the way I structured the exchange forced the deaf subject to orient to an oral conception of language. The expressiveness of sign language is distorted by insisting on a correspondence between bounded signs and oral language lexical items. The multiple sources of contextual information essential for native signing are not captured by single frame utterances. The use of words like persuaded or advised or promised in native signing requires contextual ‘build-up’ by participants that rely on creating the necessary meanings during the occasion (Cicourel 1970b). To speak of their equivalents in sign language ignores the difficulties of claiming knowledge about a different system of communication when the conception of language and thought used is conceived initially in terms of context-free oral language grammars.

I presented the sentences initially in written form and asked for sign equivalents that would be used with ‘friends’ in the hope of obtaining native signing. I received signed sentences in correspondence with oral language syntax (second language signing). The sentences signed in Table 2 attempted to simulate native signing and the subject responded with written equivalents that were ‘native-like’ and did not reorganize my signs into second language signing. It is like two separate systems that can trade on oral language grammar to represent differently organized semantic domains. Hence a bilingual speaker–signer would be able to keep the semantic domains separated. But the use of similar oral language syntactic representations does not signify equivalent semantic structures or thought processes.

My reaction to the deaf subject’s signing reveals how difficult it was for me to represent verbally several types of information I received simultaneously (including a reaction to my own talk that I perceived as inadequate). The diverse information and parallel processing that occurs is represented by sequential speech productions. In sociolinguistic research we are not clear how speech acts represent the activities of the setting experienced by participants. I cannot say to what extent the deaf subject’s experiences were represented by her verbal expressions. Hence our use of sociolinguistic speech data that
are divorced from occasions of use makes it difficult to understand to what extent the talk represents the activities of the setting and the experience of the participants.

The discrepancies between the signing I performed and the written forms produced by the deaf subject cannot be resolved by a simple reference to my ignorance of British native sign language. The problem is complicated by the subject's conception of what I am likely to 'know' and is similar to the visual illusions created by Kolers and Pomerantz (1971). The issue is not the 'objective' facts of how contours are perceived or the features that must be extracted to achieve recognition of a sign or object known to the experimenter. The problem involves the ways in which we respond to and represent information. The subject presumably assumes that I 'think' as a hearing person but she must also respond to my signing. I cannot monitor the adequacy of my signing but must rely on what the subject writes down even while 'knowing' that this is an insoluble dilemma. Hence our reliance on and use of different modalities, especially our thinking, continually alters the context-free basis we use to 'objectify' a setting. How 'native' must we be to feel 'confident' about signing with persons born deaf who are native signers? How are we to represent the information selectively attended? The discrepancies of Table 2 suggest how difficult it is to understand languages that rely on different representational structures. My attempts to learn and study sign language simultaneously reveal the difficulties of such study while exhibiting features of a substantive nature. The subject tried to create coherence by supplying terms that would presumably be intelligible to me. Her signing is equivocal because of the test conditions, but normally we would try to establish contrast sets to pinpoint discrepancies and eliminate ambiguities. Our research procedures are designed to create idealized rule structures. I followed this strategy only in part to underscore the negotiated sense in which we create claims of semantic equivalence or contrast.

I hope it is clear that forming a data base by translating manual signs into verbal strings that are grammatically correct is not an easy task. It is even more difficult to translate verbal categories that have presumed clear normative significance for hearing persons into manual signs whose significance for the deaf have a generative cognitive coherence that is tied to the circumstances of the experienced interactional setting. This is especially difficult when it is recognized that no formal syntactic rules have been identified for reformulating intentions that emerge in the context of communication. In our enthusiasm for discovering the grammar of manual sign language we must be cautious in proposing rule structures for signs that presuppose social meanings marked by verbal normative categories in formal ways. Every attempt to claim rule structures in sign language
that are similar to oral language syntax requires the use of oral language syntax as a way of formulating the problem. The formalization imposes a structure with built-in meanings that is not easily described, much less defended, in settings where signs are context-sensitive.

The idea of a data base is very misleading because operations on a data base usually imply that the subjects and researcher or experimenter use similar or identical coding rules and signals of the languages involved. When we are dealing with hearing-seeing subjects and experimenters, the issue of how much native competence is presupposed is not addressed but traded on implicitly. When using oral language categories that can have various normative meanings attached to them, and manual signs whose meanings are usually tied to the settings in which they are created and/or used, we cannot take the sign-oral category correspondence for granted because we cannot be certain that we know how native signers translate oral language categories into signs and vice-versa.

Theoretical implications

Students of language have always recognized that the verbal constructions of the child or adult do not adequately index the kinds of information available from several sources. The importance of nonverbal information has been stressed repeatedly, but the displacement of everyday meanings expressed by different modalities is usually studied by examining normative or rule-governed verbal utterances as the central carrier of information. The study of nonverbal materials employs a verbal model. I have presumed the familiar idea that we express information in several ways simultaneously. I have explored this familiar theme by suggesting two possible strategies; the representational context of oral and sign language usage. Our reliance on sequential verbal materials and our implicit use of tacit social or cultural sources of information mask or truncate the complex information processing that seems to be effortless exercises for hearing-speaking subjects. The study of social interaction presupposes that participants possess some broad capabilities that I shall gloss as interactional competence. Interactional competence refers to the ability to recognize, receive, process, and generate communicational procedures (which are at the same time informational resources) while simultaneously integrating and elaborating our thinking and reaction to these activities in the act of production or comprehension. The data base or memory simultaneously become informational resources and communicational strategies or procedures (Winograd 1971; Becker 1970; Shank 1971). Sociolinguistic studies recognize but seldom study our ability to attend simultaneously to
multiple sources of information which can be processed selectively. Instead, our research focuses on data that follow the constraints of our speech organs and idealized cultural rules for generating sequential utterances. The normative ways in which we produce utterances and elicit information, and our formalized conceptions of language structure and its acquisition obscure the complex organization of information processing in everyday settings despite our reliance on everyday exchanges as the source of our data and elicitation strategies.

Representation of experience and interpretative procedures

In western societies the hearing child between the ages of 4 and 6 faces a major problem in moving from his initial reliance on kinesic and nonverbal auditory communication, and his acquisition of an oral language that is largely context-restricted, to his gradual partial or extensive mapping of these activities into a standardized oral and orthographic system that stresses context-free constructions. Standardized oral or orthographic systems are intended to displace and index visual, oral, and auditory nonverbal, kinesic, proprioceptive and tactile informational particulars. These standardized verbal systems are creatively learned extensions of developmentally acquired abilities. We are not very clear about what is innate about language acquisition or the resultant childhood social structures that are produced, but adults find sense in and accommodate to the child's activities by supplying adult conceptions of meaning. During this period between 4 and 6 years of age the child's cognitive ability to displace objects and events, and their temporal qualities, is limited by the organization of his memory and by his command of context-free communicational devices. Recalled previous experiences seem to be divorced from their real-time adult significance and are often indiscriminately articulated within the conditions of the immediate setting. Perhaps the child has equated several images of prior experiences because of the present context, but for the adult these experiences would be marked as independent and unrelated events.

Children between the ages of 4 and 6 do not always produce clearly bounded context-free sentences that conform to a subject-verb-object (SVO) construction when interacting with other children or their parents, but as several studies (Brown 1970; Menyuk 1969; Chomsky 1969; McNeill 1970; Slobin 1971; Bloom 1970) have noted, the sentences can be seen as approximating an adult SVO structure around 6-8 years of age. It is difficult, however, to pinpoint the child's heavy reliance on several modalities for communicating intentions and understanding the intentions of others. An important adult criterion of educational success is to observe the child's increasingly more sophisticated use of oral language for displacing information. The use of apparently
sophisticated (adult-like) oral expressions does not tell us the extent to which the child understands his own usage vis-à-vis adult conceptions of such expressions, nor are we clear about the child’s awareness of how the use of oral expressions truncates other modalities when it is seeking to communicate and understand immediate, past, or future activities.

The study of cross-modal communication and semantic information processing presumes knowledge about the roles of memory and attention (James 1890; Neisser 1967; Norman 1969); for example, how many things we can attend at once. But studies of memory and attention do not always address the ways in which interpretations of the stimulus field and the reflexive thinking of the subject presuppose ideal social conceptions of socially ‘appropriate’ informational particulars. The organism’s ability to execute many disconnected conceptual processes simultaneously hinges on the difficulty of the tasks involved, the number of events it can attend and follow, the ability to retain and retrieve information (Miller 1956), and the way experiences are socially organized. Access to storage depends on the ways in which information is experienced and organized by the organism’s normative accounting procedures. Real-time is a normatively organized accounting device for recognizing and receiving information. The socially defined temporal properties of an event or object depend on our ability to switch our perception from one event or object to another. Our selective attention and retention create different kinds of temporal orderings and thus a coherent organization of experience despite independent observation or evidence by other observers that would claim otherwise.

Sociolinguistic studies must go beyond the use of model sentences and conversational exchanges because these data bases obscure how different channels of information are selected or rejected. These channels of information are basic ingredients of all interaction settings. The study of how conversational exchanges get accomplished in everyday life assumes that we design our talk so that selection of different pieces or strings of utterances occurs in ways that are presumed to facilitate the conversation. But few students of conversational exchanges view the speaker or hearer’s cognitive structures as central processes. An interface is needed between ideal normative selection procedures (covering greetings, turns, chaining, nesting, closings of conversations, topicalizing, insulting, distrusting, etc.) and the cognitive organization of processing selective cross-modal information. Conversational meaning and rules are not obvious social facts, but contingent productions. The developmental acquisition of these conversational constraints by children is not clear, but their glossing and displacement of different modalities cannot be ignored if we are to understand sociolinguistic communication.
The ethnography (or ethnographic context) of speaking (Gumperz and Hymes 1964; Gumperz 1971) implies that the social setting is an integral part of semantic information processing, and that ideal-normative conversational rules (Sacks 1970; Schegloff 1969) exist whereby participants trade on implicit socialization experiences for deciding when and how the selection and generation of different lexical items or strings will occur. The selection of particular strings, accompanied by ‘appropriate’ pauses, presupposes considerable knowledge about how to convey social appearances and utterances and intonational impressions in socially defined and emergent settings.

The picture presented thus far for hearing children may be clarified further if we return to the use of deaf manual sign language. The idea of real-time, intonational subtleties that signal irony, annoyance, pleasure, etc. are not ‘natural’ acquisitions for the deaf child, but are assumed to be developmentally ordered for the hearing child. The selective retention of information and interference from other channels of communication for the deaf is not as clear as our current speculations about hearing adults seem to be. The deaf child must struggle to learn phrase structure rules and transformations that hearing children presumably learn ‘naturally’ or ‘intuitively’.

The manual signaling systems used by deaf children and adults have localized standardization vis-à-vis each other, but achieve context-free status when in correspondence with oral language systems that have standardized grammatical rules. How the deaf, using native sign language not in correspondence with oral language syntax, displace other modalities including their memory and emergent thought is not known. But our attempt to represent sign language orally creates problems we also face as hearing persons when we rely on oral representations to index several modalities and our emergent and past experiences and thought.

Studies of natural language seldom reveal how model sentences and conversational exchanges interface with other modalities. Our general strategy in the study of model sentences and conversational exchanges is to take output as a given and then construct rule structures that may be said to underlie the output. The cognitive activities that presumably order and organize selective memory and selective attention are not examined.

Routine everyday exchanges are cross-modal, self-embedding, self-modifying, and emergent in the act of their production. The outputs of social interaction are cross-modal informational particulars that also serve as ‘programs’ or instructions for all participants including the speaker or signer of manual sign language. Cognitive structures seem to possess a self-organizing quality while simultaneously generating rule-governed strings and utterance fragments, visual, kinesic, tactile, and nonverbal auditory information. These
processes occur within a socially defined setting that is being reinterpreted continuously. Sociolinguistic studies must ask how memory influences the selection of informational particulars at any point in the conversation. How do conversational utterances become constrained by the existence of information from several sources that emerge simultaneously? Semantic information processing requires a more general cognitive organization that goes beyond linguistic and psychological structures to explain the conditions faced by deaf native signers who find oral language syntactic rules 'strange'. I have called these socially relevant cognitive properties 'interpretive procedures' (Cf. Cicourel 1970a; 1970b for a more elaborate statement and various references). The interpretive procedures are an extension of cognitive structures and facilitate cross-modal communication and understanding. The child must acquire a facility with the following properties to achieve adult interactional competence.

1. Reflexive thinking about informational particulars selectively available from multiple sources (including the speaker or signer’s own activities in the setting) in an emergent context provides participants with a basis for creating continuous instructions for programming their activities in socially acceptable ways.

2. Despite cultural differences and different spatial arrangements in the setting, participants must behave as if they share the same social setting and are receiving and processing the same information. Various appearances and utterances (signs) must be treated as 'obvious' despite the possibility that the participants are aware that differences exist and are being communicated in subtle ways.

3. In addition to assuming tacitly they are oriented to the 'same' environment of objects and thoughts despite cultural differences, and the use of a particular dialect or standardized (oral or sign) language, the participants must also be familiar with normative constraints about who can speak first, or next, what topics are considered socially relevant and acceptable, how to terminate an exchange, when someone's talk (or signs) is being insulting or distrustful or 'odd' and the like (Sacks 1970, Schegloff 1969). We do not have comparable data on the deaf here. We do not know if the deaf are constrained by sequential ordering or chaining rules because several signers can allow their signing to overlap continuously and several types of information can be communicated simultaneously that fall under the general notion of kinesic-visual communication.

4. Participants expect each other to possess 'normal form' repertoires of possible appearances, behaviors, and utterances (signs) that can be expressed or 'understood' when emergent in contextually organized settings. Participants also assume that each will normalize discrepancies to sustain the social interaction. Thus 'strange' appearances or thoughts about the setting can be handled
routinely as ‘life as usual’ because of selective attention or by the negotiated employment of a common and standardized system of signals and coding rules that are presumed to be reciprocally available to all participants.

5. The previous points imply that the participants must be able to go beyond the information given to recognize appearances, behaviors, or utterances or gestures as meaningful activities, while filling in appropriate information where relevant, by linking present informational resources to prior sources and future possibilities.

6. The ability to go beyond the information given and thus retrospectively and prospectively link immediate information to past and possible future objects or events or thoughts is central for the articulation of idealized normative (signs) rules (like conversational rules or linguistic rules as applied to model sentences) with contingent social settings. Linking stored information about rules and general background to immediate settings highlights the idea of interactional competence.

7. Participants must be capable of articulating immediate settings with idealized rules and general informational particulars of a substantive nature under the assumption that this is a routine feature of the interactional setting, yet simultaneously may or may not recognize that much of what transpires may not be accountable in standardized or colloquial expressions. What is not accountable may be viewed as comprehensible or remarkable, and yet because there is no necessary explicit marking for something attached to the setting we may fail to acknowledge its essential presence in everyday exchanges.

The above elements of interpretive procedures seem to be minimally relevant for the kind of interactional competence necessary for the production of everyday social structures. They provide a link between sociolinguistic activities and cognitive processes.

Embedded or nested signs or conversational sequences presume parallel information processing. With interference such embedding can be altered drastically. Hence in multi-party conversations the ‘cocktail party’ problem (Cherry 1953; Norman 1969), whereby different channels carrying information are selected or rejected, becomes a central issue in assessing the impact of particular channels and how short-term and long-term memory create the circumstances for embedded sequences or the cooperative construction of signs or utterances. When a subject creates a written text the problem of selective attention and competing channels becomes one of available stored information that serves as data and rule systems for satisfying a variety of considerations such as appropriate syntactic organization, possible audiences, masking thoughts, emphasizing certain points, and the like. A central issue is the extent to which the subject has been exposed to different kinds of information and rule systems. For hearing
(and deaf) children the representation of thoughts and intentions or their understanding of something is especially difficult.

Our interpretation of a child's classroom activities provides materials that highlight the kind of displacement and social reasoning attributed to the child when we try to articulate the child's performance with the teacher's claimed understanding of the outcome. Attempts to represent manual signs by verbal descriptions underscore the problem of integrating cross-modal information particulars and call our attention to the dilemma of trying to understand cognitive processes independently of communicational modalities. The problems are further complicated by the explicit use of context-free descriptions that presuppose tacit context-sensitive information.

The native competence of deaf manual signers is central to the present discussion because persons born deaf have great difficulty ever becoming native speakers or writers of oral languages. Hence, cross-modal communication involves the difficult problem of translation and equivalence of information from different modalities and the fact that the normative categories used in each language are not in correspondence despite the assumption that the psychological and social realities experienced involve standardizing the 'same' environment.

When I gave my English deaf subject model sentences for translation into sign language, I received signs that contained the same word order despite her problems of finding the appropriate signs for the verbal lexical items. The deaf subject was responding to my stimulus sentences under the assumption that the syntactic rules of oral English were the appropriate data base. The difficulty of finding the appropriate sign translations obscures the lack of correspondence between the possible semantic systems being utilized and also obscures the more central problem of how cognitive systems are mapped into linguistic representations.

Teachers of hearing and deaf children make context-free attributions of competence by the evaluation of performance in context-restricted classroom settings. In each case (referring to hearing or deaf children) the teacher makes judgments about the child's competence by examining the child's ability to represent himself or herself vis-à-vis adult performance and competence. Thus a teacher may say that a hearing child has difficulty distinguishing between particular sound patterns and associating them with idealized phonemic representations. A teacher of the deaf with some knowledge of manual sign language may say that a deaf child has written a particular sentence in a way that is not grammatical for oral English because anecdotal evidence suggests that the deaf often produce signs to represent experiences in the order of their emotional significance for the subject. In the classroom setting the (deaf and hearing) child must
contend with a system of representation that is different from the ways in which they have learned to process received or recognized information. The child's interactional competence is continually being modified by the ways in which he or she learns or is taught to represent himself or herself in family settings, peer group interaction, and in the school. The ability of the teacher or researcher to infer this competence is influenced by the adult's use of standardized SVO or model sentences to teach and elicit information, as well as to evaluate the child's performance. In the case of deaf children an unexplicated adult oral model is imposed or lurking implicitly in the background even when we claim to be studying 'natural' stages of sign language development.

NOTE

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While in search of examples in education illustrating the unity of living and learning, I became intrigued with accounts of the life of cigar-workers in Tampa, Florida, and in Puerto Rico. My interest was sparked by Jose Yglesias' recent novel, *The Truth About Them* (1971). He described the way in which the men of the cigar factories endured their long hours: they raptly listened to a reader while folding tobacco leaves. In the morning, the 'lectore', chosen for his sonorous voice by the workers and paid by them, read news-items and editorials from a variety of papers. In the afternoons, he enacted the pages of novels, some of which were world classics. Workers liked to campaign for their favorite author as the next choice for the readings, a process which entailed a vote. When the factory owners fired the readers, the workers struck.

The determination of adults to learn facts as well as to stimulate their imagination while working, is a worthwhile precedent to ponder in the midst of debates on education. The cigar-workers and their readers chose an informative as well as an enthralling method to educate themselves. Theirs is an example of relevance, of which we speak glibly and desperately while attempting to reconstruct schools. The new breed of educators, who are straining to overcome the artificial barriers between structured learning in classrooms and the spontaneous learning which takes place wherever humans congregate, are committed to relevance in education. The impact of sociolinguistics is best apprehended with their endeavors as focus; the new views of what are the settings of educational growth, and what are
meaningful role-relationships between learners and teachers. These concerns are of eminent interest to the ethnographers of language.

A. The study of diversity and the learning process

Sociolinguists are familiar with some of the controversies in education as they are expressed in approaches to language learning, language attitudes, and bilingualism. As field-workers they need not be convinced that the educational process, including the acquisition of speech and its derivatives, cannot be confined to the walled-in classroom. The ignorance of the many market-places of learning, such as the school-yard, the plaza, the chapter-houses, the streets and homes of ghettoes, has contributed to pedagogical impasses; but these are the very settings where ethnographers learn of the learning of speech. However, the broader relationships between sociolinguistic theory and practice, and the crisis of contemporary education may not be always evident--this paper is an attempt to explore these issues.

The dissidents in the educational professions have focused on two major issues, one of which is the structure of educational institutions, the other relates to models of learning and teaching.

In discussing the former, Lillian Weber’s (1971) analysis seems particularly pertinent. From the very start of compulsory education, she argues, factory practices and a commitment to efficiency were influential in shaping the nature of schooling. ‘Far more decisive than the spread of a vague Deweyism was the much wider spread of administrative adjustment to correlate with business practices’ (page 234).

Institutions which resemble factories, in the standardization of plants and the preoccupation with products (such as reading scores), contribute to functional preferences to fit such settings. Paolo Freire speaks of ‘banking education’ as illustrative of the kind of classroom practices which fit these settings. In a long list of descriptive statements, he ends up by summarizing the prevalent approach to education: ‘the teacher is the Subject of the learning process, while the pupils are mere objects’ (1972).

The best-known example of teaching practices in the field of language pedagogy, which illustrate the education Freire decries, is the Distar program, an adaptation of the original Bereiter-Engelman method of ‘enriching’ low-income, Black children. Though this method of intervention has been criticized widely, it is still a popular and widespread effort. In it the child is treated as an empty vessel, and Freire’s words are applicable, once more: ‘The teacher thinks and the students are thought about’, and ‘the teacher chooses the program content, and the students (who were not consulted) adapt to it’ (1972:59).
The assumptions regarding a predominantly passive learner, who is challenged and programmed to acquire certain institutionally useful skills, goes counter to the linguist's oft-stated conception: 'that all normal human beings are gifted with a capacity of language, a capacity which is fundamentally equivalent in all, a capacity that has a complexity and potentiality for creative use beyond our scientific ability as yet to assess fully' (Hymes 1970:12).

The contradiction between a universal language capacity, and the performance of great numbers of children of the oppressed, has been accounted for in a variety of ways: One explanation is offered by eight Italian schoolboys, the authors of a Letter to the Teacher, who see it as follows: 'Languages are created by the poor, who then go renewing them forever. The rich crystalize them in order to put on the spot anybody who speaks in a different way. Or in order to fail him in exams' (Schoolboys of Barbiara 1970:12). The students of Barbiara look at forms of language which are stilted and academic, (and are used in many classrooms by the teachers) as one more aspect of the silencing of the poor. They express their insights powerfully, illustrating Labov's (1970) contention, that children raised in low-income homes speak with clarity on issues of import to them. These are qualities they are supposed to have in short supply.

The opposite view of genetically-linked deficiencies in abilities, (particularly among Blacks) has been resurrected lately. Jensen's position has received an unprecendented popular exposure in the mass media during the last two years. However, a refutation of the arguments concerning limited abstract skills among the poor need not be replicated here; the job has been done well elsewhere. One aspect of the controversy relates to how, and where and from whom children learn speech, and in what contexts they use that which they know. These aspects of verbal growth are of concern to educators as well as to sociolinguists.

Students of language have been strongly influenced by a theory of acquisition which emphasizes universal, biologically-based processes. Behaviorists, on the other hand, have chosen highly specific aspects of the environment, (i.e. contingencies of reinforcement) as crucial to their conceptualizations of learning. I would like to argue that both of these stances toward growth, however contradictory to each other, capitalize upon fragmentary aspects of a total and complex process. The accurate depiction of the learning of language, in concrete settings and during definable historical periods, requires observational methods and approaches to reflection which are currently marginal in education, and in the social sciences. One of these, as yet marginal methods, is the ethnographic study of communication. The emphasis it represents is upon settings; those in which the child learns to speak or to remain silent, the variety of language he chooses
to use, and other such considerations. These are of great importance to theories of language acquisition as well as to the more general accounts of human learning.

Sociolinguistic studies are hard to summarize. I have chosen one investigation, familiar to many of the readers of the Georgetown Round Table Monographs. In addition, I will mention a couple of studies in progress, which are as yet fragmentary. My choices reflect my concern for relating empirical findings collected in a broad range of settings to theoretical questions.

The study of speech-use among Warm Springs Indian children in Oregon offers a dramatic illustration of Freire's concept of 'banking education'. The ethnographer Susan Philips writes: . . . 'there is the explicit and implicit assumption that the teacher controls all activity taking place in the classroom and the students accept and are obedient to her authority. She determines the sociospatial arrangement of all interactions; she decrees when and where movement takes place in the classroom. And most important for our present concern with communication, she determines who will talk and when they will talk' (1970:82).

Young Indian children find it difficult to adjust to such a structure in their classrooms. They are eager to engage their peers in talk, while non-Indian children of the same age, observed by Philips, are vying for the attention of their teachers. The children of Warm Springs, like most other tribal young ones, are reluctant to speak in front of large groups. The 'participant' structure they prefer is small, run by the students themselves, with the teacher as a resource person. But they have little opportunity to form such work groups, particularly in the lower grades.

The teacher confronts them with her firm belief that verbal performance in front of a group is a necessary aspect of the teaching-learning process. The validity of this assumption is now challenged in many schools which have opted for informal education. The force of cultural misunderstanding, however, impels those who see themselves as socializing agents to implement teaching practices which have been formative in their own development. Precisely because these teachers have difficulty interpreting their students' 'silence', they choose verbal recitation as a method of assessment.

Learning, or failure of learning in the classroom, is contrasted in this study of Oregon Indians with the styles of informal study by children in their homes. In Warm Springs, as in many other rural communities, youngsters spend many hours observing adults in the midst of productive activities. In the recollection of older members of the tribe, the importance of long periods of observation is highlighted as a critical first stage in the acquisition of complex skills, such as
tanning or healing. After children have spent some time observing adults in a task, reports Philips, they are invited to carry out a small segment of it by themselves. The invitation carries no pressure with it. The child discovers in private whether he has learned enough to share in the total activity. Once satisfied, he joins in.

Cole, Jay Glick and Sharp (1971) have also found instances of learning by watching on the part of Kpalle children in Liberia. They, too, practice some of their newly acquired skills away from the view of adults, though not in private; they tend to stay in groups of children. From these descriptions, a way of learning emerges in which the child is an active participant in determining the rate at which he wants to learn. He makes judgments about his own competence. This is in sharp contrast to classroom methods of instruction described by Phillips, and similar efforts depicted by Dumont (in press) among Cherokee children. Public performance or failure, open criticism, and competition play a crucial role in the experiences of children in the latter school settings.

In the context of ethnographic accounts, observation as a method of learning is easy to take for granted. Upon reflection, however, it becomes apparent, that ‘learning by looking’ has no real role in the conceptualizations presented in contemporary theories of human development. Reinforced behavior is considered essential by behaviorists, and the role of active exploration is stressed by Piaget. Educators are uncomfortable with a curriculum which makes allowances for apparently aimless stretches of time conducive to children observing others. There is no strong theoretical justification offered to them for including these kinds of experiences as part of schooling.

Through a continued documentation of learning and communication styles, the sociolinguist can make an important contribution to the reassessment of existing theories. Only if theories were to become genuinely broadened could there be a modification of educational practice to include observational learning of the type described above.

I do not suggest that we modify our current approaches in favor of a one-sided emphasis upon learning by watching. Instead, theories which emphasize phases of acquisition seem to be called for. The examples in learning described by Philips illustrate distinct behavioral episodes. A new and complex theory might include a cognizance of such episodes or phases, where each phase might be characterized by a different principle of learning, including observation, reinforcement, and active exploration.

The challenge of extending and modifying theoretical notions when confronted with the impact of detailed observations has relevance to informal education as well as to those theories which form the backbone of ‘banking education’.
The major premise of open education is that the child is an active agent of his own learning; he constructs and reconstructs his own experience. Those who have proposed such a view of growth have opposed a mere charting of gains in a clinic or laboratory. Instead, they used their own versions of the field workers' methods; Nathan and Susan Isaacs, and Jean Piaget, the three main architects of the British Infant School, have chosen to engage in time-consuming and painstaking efforts of watching children in a variety of settings. Their theories of human development have been influential in Europe over a long period of time, and they are now gaining increasing respect in the United States. The many new approaches to informal education, based on their concepts, have given new confidence to those who have despaired of the possibility of change in the public schools.

The emphasis in most open classrooms is upon the child's individualized encounter with his physical and social world. Brearley likens this process to the play of the infant, which is solitary for a considerable length of time. She states 'the creative task is the individual's but the sources are many' (1970:10).

In an observational study in New York, Mrs. Adela Oliver is recording the physical and interactional settings children gather while they are reading (personal communication). In these open corridor classrooms, the children prefer trade books to basal readers. In groups of three, or four, they sit on a rug, or on chairs in quiet corners, occasionally pairs of children, or even five of them, get together while each is reading a book. These little groups of avid readers mix reading to themselves with animated conversation. They help each other with difficult words: one morning Mrs. Oliver recorded one child saying to his friend in trouble, 'If the teacher were here now, she would make you sound the words out!' The students do not hesitate to call upon their teachers when they cannot solve a problem in reading; even casual visitors might be consulted and asked for help.

Reading by these children is a social endeavor; this observation is something of a contrast to the notions of learning and growth proposed by the Isaacs and Piagets, which have been influential in the designing of these classrooms. The theoretical perspectives of these European scholars is welcomed by educators who find large classroom instruction, with its attending reliance upon choral speaking, constant disciplining, and public recitation futile as instructional techniques. The active thrust of the learner in the open classrooms is a welcome change from these traditional practices. But the opposite tendency, with its heavy emphasis upon the individual learner, might also limit our understanding of the full range of intellectual growth. For instance, the role of language, if viewed as a critical bridge between the fragmentary view of the world that a child can construct by his
isolated efforts, but which can be unified and expanded through dialogue, and a joint evaluation of what has been learned in discussion with peers and teachers, is minimized in schools which are primarily influenced by Piaget's ideas.

Theoretical debates on these points are empty exercises without the support rendered by data gathered in systematic observations conducted by psychologists and by the ethnographic approaches of the sociolinguists. These are enormously useful in depicting those aspects of learning which may be overlooked or minimized by developmental theorists with specific biases. The study of diversity in learning styles as revealed in research, such as the work of Susan Philips, thus has a direct and immediate role in sharpening a needed theoretical debate in the field of educational innovations. Its immediate impact is to offer a more thorough understanding of where and with whom children practice that which they learn, and how they share that which they know.

B. Sociolinguistic contributions to the problems of educational assessment

A recognition of learning styles is not particularly novel in the literature of psychology and education. But the range and variations in styles described in the extant literature are limited by the settings in which children are observed and tested. The narrow focus of those who work with children in accessible urban schools and laboratories is compounded with the application of measuring instruments which reflect various stages of culture blindness on the part of their constructors.

The frequently practiced assessment of children's linguistic performance by standardized approaches, and in atypical settings, is an example of such a focus. Language which is recorded in spontaneous settings reveals different aspects of communicative competence, particularly among the children of the dispossessed. Schrager (1971) has shown that children are capable of producing utterances of greater complexity than those they are taught by the Bereiter-Engelman (or more recently Distar) method. This peculiar asymmetry is hard to explain until we realize that the instructional techniques developed by these authors were based on their assessment of children's verbal know-how, measured by standard tests, and in standard test circumstances. These are limited and limiting settings.

The point has been made repeatedly; the aspects of performance selected for measurement reflect the values, the know-how of psychometricians, who in turn articulate the values of a competitive and overly technologized society. These measures are couched in the language variants of the dominant group, and are administered
according to the interaction styles of that dominant society. Consequently, children whose parents are not fully participating members of that strata of society are baffled and insecure when confronted by the communicative and linguistic demands of these tests (Hymes in press; Baratz 1969:889-901).

A sociolinguistic perspective has played a critical role in questioning the work of psychometricians. Labov's (1970) analysis of the linguistic and intellectual potential of dialect speakers is a case in point. We are all indebted to him for his clear and uncompromising attack on those, who by accepting standardized tests as valid measures, have contributed to the educational oppression of poor children. Though his work has been quoted and praised by those who read what linguists write, as yet his impact is limited upon most workers who evaluate and monitor the thousands of projects aimed at low-income children.

Cross-cultural research studies offer an exception to such a pessimistic statement; similarly, the impact of ethnographic research is slowly discernible among young workers embarking on their doctoral research. The stress upon language use as one aspect of communicative competence is better understood and explored by those researchers who are not bound by the rigid demands of contractual research.

One example of a promising research technique, aimed at assessing skills in language performance and knowledge of interactional strategies, is the two-person communication game. The task has a number of variants; at Harvard, Moore (1971) used arrays of abstract designs as well as pictures of children as stimuli. Each child is screened from his partner's view; he is asked to pick up a picture and describe it, so that the other participant in the communication game can choose the same picture from his differently arranged array.

In his intervention study, Moore's primary concern was in assessing whether children profit most from a 'patterning' approach to dialogue (in which they repeat as well as elaborate adults' models in speech). The other method of 'extension' consists in using the child's own, spontaneous verbalizations as the focus of subsequent interactions. It was found that children's own verbalizations contributed most to gains they made in the accuracy and complexity of their performance in the two-person communication game. In other words, methods of assessment which do not limit the child to a canned question-and-answer format reveal more subtle insights in the growth of language, as well as furnishing a more natural pull upon the child's linguistic repertoire. The exploration of these approaches in rather traditional research contexts reflects the slowly, but increasingly growing impact of sociolinguistic concerns.
The kind of dialogue which is elicited in a communication game bears some resemblance to ordinary communication, particularly when the participants are not in a face-to-face situation, but in situations such as telephone exchanges, or when people shout to each other across rooms. These aspects of realism of the game may be responsible for Gleason's finding that young children can produce, in a relatively short time, descriptive styles which fit the requirements of the communication game. Such rapid improvement is hard to demonstrate with standardized tasks in which the child's sole experience is being judged (unpublished MS).

Comparing the performance of children in a traditionally administered vocabulary test with performance in a two-person communication game, Gerald Hoffman (1972) is finding similar trends. Low-income children need many clues in order to produce the style of performance which is appropriate to the task. These clues or prompts were offered to them by the experimenter without his being aware of it. In the testing situation, Hoffman gave an equal number of prompts to both low-income and middle-income children. In the game situation, he prompted the low-income children three times as often as their controls and only after the data was collected did he discover this discrepancy. But the low-income children did show improvement in their behavior during the test, revealing once more that children who are outside of the mainstream can learn to approximate their 'competence' when removed from the constraints of standard test conditions. Whenever testers are reporting above average test scores for low-income children, a similar context for performance has been noticed. They help the child to choose appropriate styles of performance from his existing repertoire, without giving him clues about content (Mendez 1972; Mallory n.d.).

Once the adaptive value of any particular process of learning is recognized, racial and cross-cultural comparisons which ascribe variations to genetic endowment, or which focus on isolated aspects of behavior for comparisons, emerge as useless. While the ethnographer of speaking and learning is aware of context, the student of education and psychology still examines diversity with the aid of the bell-shaped curve. In order to rank individuals quantitatively, the strategy of isolating features of behavior becomes necessary. In a setting where a range of behaviors is tapped, and which approximates the use of skills in experientially familiar circumstances, children may display that which they know.

The promise of such an approach should not be made without some warning of the complexity of research conducted from a sociolinguistic perspective. The work of Mallory in Albuquerque illustrates this concern. She has collected some ethnographic observations concerning language choice among diglossic Spanish–English speakers in
a small barrio of the city. Attitude tests, the use of talking pictures, failed to reflect language choice reality; however, a role simulation method with children was successful. She concluded, 'Role-simulation with children was the lone successful attempt. The adults did not perform their diglossic competence. While the information sought still could be significant for bilingual programs, it seems doubtful that it will be obtained except by careful, time-consuming, ethnographic, participant-observer work of the type exemplified by John Gumperz' (Mallory 1971:72).

C. The role of context in theories of growth: Piaget and Chomsky

A preference is shared by many whose concern is the understanding of human capacities; namely, that observation instead of measurement should be the foundation on which theories are built.

A recognition that observation is a legitimate, and at this stage of our knowledge, a necessary concern did not come easily to students of humankind. The freedom to look at children for long periods of time is considered a luxury by some, and frivolous by others. However, this is the very method used by Piaget for the development of his conceptualizations. The spontaneous activity of the infant, and the experienced results of such activity, are ever-present themes in the flow of development captured by him. 'To know an object is to act on it. To know is to modify, to transform the object, and to understand the process of transformation . . .' (1964:8). In this theory, the link between external and internal worlds is based upon the active engagement of the child with his surrounds. The roles of perception, memory, dramatic play, and feelings are stressed by those who are fashioning an educational practice based on Piaget's monumental work in the genetic study of development.

The center of gravity of this theory is biological man. The great Swiss psychologist does not neglect the impact of reality upon the growing child, but he views the outcome of mutuality as a universal theory of stages.

The child is an active agent in his learning while he displays sensory-motor intelligence, or as he views the world pre-operationally. He constructs and reconstructs his development while he masters formal logic. The thrust which impels him is part of his humanity, a humanity which is as general as his body is akin to all other human bodies.

There is a prideful reassertion of the wholeness of the species in these concepts, a resistance to mechanistic notions so popular in a mechanistic age. But it is intriguing to realize that Piaget's theories are of particular effectiveness in explaining the growth of children's thinking toward the model of scientific thought. The clarity and
Students of language are apt to ask questions at this point. The sociolinguistic perspective represents change in its contextualized garb; the child who speaks is fluent in a language, that of his speech community. He learns a set of expectations, a view of himself as a communicator in his discourse with others who sometimes listen, and at other times question, reject, or challenge his meaning.

The notion of universals, whether in experiential growth or in biologically layered stages, is at odds with the methodology Piaget and the sociolinguists share, namely, the study of the specific learner in specific settings. But similarities between children who may thus have been observed are not drawn in such a manner; a commonality is presupposed between their inner structures, their inner schemas. The assumption prevalent in Piaget's theory, that the inadequacy of schemas during the early stages of learning is due to immaturity and will reach an equilibrium later, is particularly distressing to me.

I prefer Ernst Fisher's view of modern men and women, whom he sees as incompleted beings conscious of their incompletion (1963). It is through art, language, shared action, and active dialogues, engaged in by peers at particular times and places, that true growth emerges. No scientific theory is accepted without first passing the challenge brought to it by an opponent. Similarly, at all stages of the growth of children their active engagement with their surrounds includes a social elaboration and testing of the reflected consequences of their acts. Action divorced from reflection remains isolated. Reflection is not only the schematic inner change of which Piaget speaks. Reflection is the use of man's socially perfected tool, the heritage of meanings, through which new experiences are sifted and evaluated.

Even objects through which the child tests his construction of reality are reminders of the irrevocable fact that human reality in every one of its manifestations is socially fashioned. The growing child in Piaget's books is something of a loner, a proper figure, if we remember that his intellectual birth took place in Geneva, the home of Calvinism.

These criticisms of Piaget bear some resemblance to those of Chomsky, as expressed by Dell Hymes: 'I do not think that we can abandon some conception of a generic human nature (human essence), as the thesis might be taken as saying; but the man for whom Chomsky's competence and theory is a model is indeed an isolated man in the abstract. There is nothing to be said about men and women' (Hymes 1970:75).

As applied to the acquisition of language, the transformational theory highlights the active role the child plays in the development of his language; he uses creatively a set of complex rules. This
conception is stated, once more, as a universal. And, again we are torn. A theory is proposed which claims the democratic oneness of man whose skills of language are not a pitiful imitation of sounds, rules, and meanings to be repeated as a consequence of reinforcement. What he hears is a small, but necessary part of what he produces. His 'input' forms the basis for the testing of hypotheses of regularities in the syntax. He has the creative capacity to produce an infinite variety of rule-governed, and frequently novel, utterances. The gift of language is part of man's make-up.

Piaget and Chomsky share a focus upon biologically rooted growth, though to the grammarian, environment is of lesser consequence than to the psychologist. Hymes questions the abstractness of Chomsky's representation. 'The creative aspect of language, like "competence", offers more than it contains. It is analyzed in terms of the possibility of producing an infinite number of sentences, free of immediate stimulus control, that are yet appropriate. But a sentence may be new, free of stimulus control, and bizarre. Appropriateness entails a relation to a situation' (1970:75).

The thread which hopefully unites this paper is in its emphasis upon situationally defined, elaborated, and enhanced human behavior. The criticisms of Piaget and Chomsky, briefly sketched above, are approached from this vantage point. The word situation or context, as used in this paper, is but a shorthand for a more complex notion; the setting in which learning takes place is a reflection of social reality, symbolized in the many features of time, place, and people. They are, in their interactions and impact upon the child, a reflection of his and his society's history. He, in turn, in his actions upon them, creates his and their joint future history.

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SPEECH STYLE AND SCHOLASTIC SUCCESS:
THE TENTATIVE RELATIONSHIPS AND POSSIBLE
IMPLICATIONS FOR LOWER SOCIAL CLASS
CHILDREN

ROBERT FRENDER AND WALLACE E. LAMBERT
Harvard University and McGill University

... One of the Co-heads (of the Truant Department) ... characterized all truant children as being 'less intelli-
gent, less well-scrubbed, crude, careless, and with
pungent speech'. (Excerpt from The way we go to

Children from lower social classes, or from minority group back-
grounds typically achieve less well in school than their middle class peers. To those who want to foster equality of educational opportunity within their society, this long-standing fact continues to be a source of concern. Because opportunities to share in many of the benefits and enrichments of modern society are increasingly dependent upon achieve-
ment in school, 'the futures of these children are comparatively unprom-
ising. At the individual level this means that the young people who most
need their full potential to understand and cope with the challenges of life
are kept ignorant of what their lives might be. At the societal level, it
means a misuse of valuable human resources and that the social system
itself is unfair.

The past decade has seen the problem come to national attention in
Europe as well as in the United States. For example, in France a power-
ful political party, the Radical Socialist Party of Jean-Jacques Servan-
Schreiber, has based its platform on (1) a reduction of social distances
between social classes, (2) an abolition of hereditary private wealth
and (3) a redistribution of public power (see article in l'Express 2-8,
février, 1970). In the party's manifesto, Servan-Schreiber places
great emphasis on the plight of the socio-economically disadvantaged child in the public education system.

In the United States, steps have been taken to improve the educational achievements of those now known as the 'disadvantaged'. Numerous intervention projects have been funded by government agencies such as the Office of Education and the Office of Economic Opportunity, most prominent of which have been the preschool compensatory education programs of the Head Start variety. The rationale behind these programs was to provide the disadvantaged child a summer, later increased to a year, of preschool instruction in the hope that skills necessary to scholastic achievement could be adequately developed as the child entered the public school system.

Smith and Bissell (1970) reviewed the successes and failures of these programs and the evidence now available is far from conclusive. The findings of early workers (such as Weikart, Bereiter, and Klaus and Grey) gave reason for optimism and high expectations since it seemed that carefully designed and implemented programs did increase the cognitive performance of disadvantaged children. More recent reports have sparked controversy by suggesting that compensatory education programs have not promoted the intellectual development of the target children (Jensen 1969, Cicirelli et al. 1969). According to Smith and Bissell, the major findings of the Westinghouse-Ohio National Evaluation of Head Start were these: (1) Summer Head Start programs do not produce cognitive or 'affective' gains that persist into the early elementary grades, and (2) Full-year Head Start programs do not appear to influence affective development, although they have marginal effects on cognitive development which can still be detected in grades one, two, and three (Smith and Bissell 1970:52).

Smith and Bissell criticized the methodology and analysis of the study conducted by Cicirelli et al., and came to quite different conclusions on the effect of the full-year Head Start. In their view:

The Head Start group scored higher than the control group on the Metropolitan Readiness Test by a large enough margin for us to consider the differences 'educationally significant' (Smith and Bissell 1970:101).

They also suggest that the findings for the second grade sample argue for continued intervention into elementary school years. They make this very strong statement:

The second grade data from this study, as well as the data from the few careful longitudinal studies of intervention programs which have been undertaken over the last few years show the same pattern: when the participating children leave
the preschool intervention program and enter the public school system, a precipitous drop in their achievement occurs, although the participants continue to perform better than non-participants on cognitive measures through the end of third grade. This suggests that in order to prevent cumulative and continuous retardation on the part of disadvantaged children, a policy of continued intervention during the elementary school years must supplement preschool intervention programs (Smith and Bissell 1970:102).

This last sentence suggests that rather than compensatory education projects, it is the schools themselves that have failed to meet the challenge.

If it is true that intervention programs, or the schools themselves, have failed to meet the needs of the disadvantaged child, it seems worthwhile to examine briefly the theoretical bases of intervention programs as a first step toward understanding why they, as well as the schools, may not have achieved their original goal and how they might eventually do so.

In the early sixties J. McV. Hunt (1961) wrote an influential book which emphasized the importance of rich and varied sensory experiences in infancy for the development of intelligence. By providing the infant with an optimal environment one could enhance his mental growth. This notion led to another: that children from the lower social classes had been deprived of the necessary inputs for cognitive growth in early life, and that this deprivation was likely at the root of their inability to cope with the demands of school. Hunt drew on basic studies showing that animals raised in environments poor in sensory input were later retarded in perceptual development, and had difficulty on tests of 'animal intelligence' (e.g. Riesen 1950, Thompson and Heron 1954). Similarly, infants raised in orphanages with little sensory variety and little interaction with a caretaker were retarded in their mental development. When provided with more adequate care and experience, their depressed intelligence scores could be raised (Skeels and Dye 1939).

This line of thought led to the practical idea that if these children who came to be known as culturally 'deprived' or 'disadvantaged' could be reached early enough by programs that might compensate for their impoverished experiences, perhaps the fatal effects could be reversed. This was reinforced by the work of Bloom (1964) who demonstrated that 50% of the variance on IQ test performance at age eighteen could be explained by the scores on these tests as early as the age of five.

Another interpretation of the deprived child's failure in school is the 'linguistic deficiency' theory. Proponents of this view argued that one of the major handicaps of lower class children was their relatively poor linguistic skills. Basil Bernstein, a British sociologist, was the most
prominent theorist to give impetus to this notion. In describing the ways children from different social classes use language, he distinguished between the 'restricted' code of the lower social classes and the 'elaborated' code of the middle class. The fundamental difference seems to be that the restricted code relies more on implicit meanings and is context bound, whereas the elaborated code presents information in a more explicit manner. Put simply, this means that the restricted code can be effective only if the listener already knows what the speaker is talking about.

The characteristics of the restricted code as Bernstein (1961) describes them are: (1) Short, grammatically simple, often unfinished sentences with a poor syntactical form stressing the active voice. (2) Simple and repetitive use of conjunctions (e.g. so, then). (3) Little use of subordinate clauses to break down the initial categories of the dominant subject. (4) Inability to hold a formal subject through a speech sequence; thus a dislocated informational content is facilitated. (5) Rigid and limited use of adjectives and adverbs. (6) Infrequent use of impersonal pronouns as subjects of conditional clauses. (7) Frequent use of statements where the reason and conclusion are confounded to produce a categoric statement. (8) A large number of statements/phrases which signal a requirement for the previous speech sequence to be reinforced: Wouldn't it? You see? You know? etc. This process is termed 'sympathetic circularity'. (9) Individual selection from a group of schematic phrases or sequences will frequently occur. (10) The individual qualification is implicit in the sentence organization: it is a language of implicit meaning.

To Bernstein, the restricted code is both a cognitive and communicative handicap.

In the learning of this linguistic form (restricted code), the child is progressively oriented toward a relatively low level of conceptualization...In turn this will affect what is learned and how it is learned and so influence future learning. (Bernstein 1967:98)

Finally and of greatest importance it is a language of implicit meaning in which it becomes progressively more difficult to make explicit, and to elaborate subjective intent. (Bernstein 1967:99)

These and other writings by Bernstein had an important effect on the conceptualization of the lower class child's linguistic competence. Yet Bernstein (1970) recently argued that the distinction between restricted and elaborated code has 'sometimes led to the erroneous conception that a restricted code can be directly equated with linguistic deprivation,
linguistic deficiency, or being nonverbal.' If it is true that American researchers have been led to 'erroneous conceptions', it is not hard to see why if one reads the excerpts listed above.

Misinterpretation or not, the important fact is that lower class children came to be seen as possessing an inadequate mastery of linguistic skills. Consequently many of the early and prominent workers in the field of compensatory education began to stress the importance of providing children from lower classes an adequate mastery of language (Bereiter 1966, Deutsch 1963).

Not only did Bernstein introduce the concept of a restricted code, he also focused attention on the mother-child relationship as one of the antecedents in the development of the restricted code:

The linguistic relationship between the lower-working class mother and her child is such that little pressure is placed upon the child to verbalise in a way which signals and symbolises his unique experience (Bernstein 1967:92).

This idea was expanded by Hess and Shipman (1965) in a very influential paper. They contended that the effects of cultural deprivation stem in part from an insufficiency of cognitive meaning in the communication between mother and child. They believed that communication and language are shaped by society and family, and that in turn thought and cognitive styles of problem solving are shaped by language. In lower class families, they argue, the mother-child communication is more likely to be 'status-oriented' than 'person-oriented'. A person-oriented mother regulates the behavior of her child by discussing and explaining, taking into account the individual needs of the child. In the status-oriented family appeals are made to norms of behaviour, and imperative statements are used to control the child. A restricted language code on the part of the lower class mother is concomitant with her status-oriented regulation of the child, and consequently the lower class child acquires a restricted code himself. The restricted code which has been learned by the lower social class child then handicaps his modes of conceptualization.

To support their argument Hess and Shipman (1965) presented the following evidence obtained from a study of mothers and their four year old children from the various social class backgrounds. (1) There were differences in the quantity and quality of maternal language between the social classes. (2) Middle class mothers gave more person-oriented statements in protocols dealing with instructions they would give to their children in hypothetical school situations. (3) Both the lower class mothers and their children used less sophisticated modes of categorizing behaviour on sorting tasks. (4) The children of the lower social classes gave less verbal justifications for their sorting choices. (5) The
lower social class mothers were less effective in teaching tasks to their children.

Hess and Shipman directed attention to the mother–child relationship in the lower social classes, and paved the way for intervention studies where lower class mothers were trained to provide their children with presumably optimal experiences for cognitive growth (e.g. Gordon 1969). But what are optimal experiences, and what are the characteristics of a competent mother? Actually, investigators continue their search for the characteristics of mothers who produce competent children (e.g. White et al. 1969).

Thus during the sixties three important assumptions came to light: (1) Children from the lower social classes are deprived of the experiences necessary for cognitive development during infancy, (2) Children from the lower social classes are deficient with respect to linguistic skills, and (3) The quality of the mother–child relationship in the lower classes hampers the lower class children’s cognitive and linguistic development.

Most of the work based on these assumptions focused on the deficiencies of the lower class child and his culture as compared with middle class counterparts. Baratz and Baratz (1970) discerned an ethnocentrism among social scientists in their descriptions of the Negro deviations from the white middle class norm. Blacks and members of the lower class have come to be seen as deficient, not different. A social pathology model had been substituted for a genetic inferiority model and this directed attention away from schools and educational programs that may have failed to meet the needs of children who come to them with different backgrounds and experiences. It is only recently that the focus has drifted back to the possible shortcomings of the educational system, as well as the lack of preparation lower class children appear to have for the schools as they now operate. Researchers in various fields now are careful to examine the conceptions and the misconceptions in vogue about the ‘disadvantaged’ child. Hopefully, this critical but constructive approach will assist schools and preschools to achieve their goal: the improved educational performance of the children that have come to be known as the ‘deprived’. Baratz and Baratz see this issue clearly:

The educational problems of lower class culturally different Negro children, as of other groups of culturally different children, are not so much related to inappropriate educational goals as to inadequate means for meeting these goals. (Baratz and Baratz 1970:42)

A good example of this new approach to the study of the child from the black ghetto is the work of Labov (1970). He shows that these children
are not verbally deprived, nor do they speak a language that is deficient. Rather they speak a different dialect of English which is as syntactically sophisticated as standard English. Labov also rejects the idea that their language is a cognitive handicap, i.e. one which prevents them from achieving higher forms of abstract or logical thought. As a consequence of Labov’s work, some researchers have endorsed the idea of teaching the black child to read using books that are written in his own dialect (J. Baratz 1970), so that he can acquire the necessary educational skills and competence in the standard dialect of the society without eradicating his own dialect. The aim is to have him become bidialectal, or in the case of other minority groups, bilingual. This idea in turn has been given the backing of the American government in the form of the Bilingual Education Act.

Our purpose here is to introduce another set of ideas, perhaps best described as an aspect of the social psychology of education, which hopefully can contribute to our understanding of the difficulties that lower class and minority children face in school and in life. The basic notion underlying the work to be presented is that the speech styles of children affect their scholastic success. As used here speech style encompasses the voice characteristics of a speaker and how he articulates or delivers the message. It is not meant to include what he says, the content of his message, or such features as grammatical errors. Instead as we use it, speech style includes such characteristics as intonation, pronunciation, pitch, fluency, and how confident the speaker sounds.

Put briefly, our argument runs as follows: The expectations that a teacher develops about a child’s ability, potential, personality, and social class background will influence how she deals with him, and this will have an effect upon his scholastic achievements. One of the more prominent mediators of these expectations of the child’s potential and personality may be his style of using language and this, we contend, may affect the teacher’s evaluations. Thus, we would anticipate a relationship between speech style and performance in school to show itself in research. This hypothesis gains credence in the light of recent studies that show how members of different social classes differ with respect to certain salient aspects of speech style, and how characteristics of speakers from the lower social class lead to relatively unfavorable impressions on the part of listeners. In short, how a child ‘sounds’, as well as what he says and does, are believed to affect his performance in school. Since the ‘sound’ of lower class children is perceived less favorably in general, we may have one tangible factor (obviously not the only one) that helps explain why children from lower social class backgrounds do less well in school.

Teachers’ expectations can have an important influence on the subsequent performance of their students as Rosenthal and Jacobson (1968a, b) have shown. In their experiment the researchers designated an
arbitrarily chosen group of students as those expected to be ‘academic spurters’ on the basis of a test that had been given to them and their classmates. The actual performance of these children on the test did not differ from that of the control group. The contrived expectations were communicated to the teachers in a casual, off-handed manner. At the end of the year it was found that the group said to be ‘spurters’ showed greater gains on IQ tests than did their control counterparts. To balance the study would have been unethical because it would require equivalent conditions where unfavourable expectations for a group of children were passed on to teachers. Thus, one can only hypothesize about the effects of such expectations, but as Rosenthal and Jacobson suggest, teachers do expect children from the lower social classes to do less well in school, and it is not improbable that these expectations result in a ‘self-fulfilling prophecy’.

Thus there is evidence that teachers’ expectations can influence the subsequent performance of their pupils. Is there corresponding evidence to show that speech styles affect the perceptions or impressions listeners form about speakers? Research at McGill University has demonstrated that the dialect or language a speaker uses can have powerful effects on the reactions of listeners who are asked to evaluate personality characteristics of that speaker (Lambert, Hodgson, Gardner, and Fillenbaum 1960; Lambert, Anisfeld, and Yenikomshian 1965; Lambert 1967; Tucker and Lambert 1969). Labov (1966) has shown that mode of pronunciation of selected phonemes is related to one’s social class, and conversely that variations in phonemic production are used as a basis for making judgments about a speaker’s social class background. Recent work in Great Britain by Giles (1971) and Cheyne (1970) has shown the prominent role pronunciation and speech style play in impression formation in British settings. Similar research by Brown (1969) and Brown and Lambert (1972) has found that French-Canadians of various social class backgrounds differ in speech characteristics, and that these differences markedly affect the evaluations that French-Canadian listeners make about speakers from the different social classes. Brown found that upper class in contrast to lower class speakers use more intonation, are more articulate, sound more confident and self-assured, are more fluent, and have less of a Canadian and more of a Continental type of French accent. Apparently on the basis of these characteristics, the lower class speakers were evaluated decidedly less favorably. In the McGill studies and those in Britain each speaker read the same short passage (or a translated version) to control the content of the speech sample which the linguists evaluated objectively and that the listeners responded to subjectively.

The ‘nature’ of the evaluations made of lower versus higher status forms of a language is of relevance to our position. In both the Montreal
and the British studies, lower class or lower status speakers are perceived as being deficient in 'competence' (i.e. they are rated low on traits like intelligence, self-confidence, determination) in comparison with higher prestige speakers. But there is a perceptual selectivity involved; they are not in general judged as being less kind, less good-natured, or less humorous. In fact, lower status listeners show an 'accent loyalty' whereby they rate speakers who also have a regional, a social class, or an ethnic style as having more personal integrity and social attractiveness although they are clearly seen as less competent (see Giles 1970). This selective perception of class-marked speech styles suggests that the lower status speaker runs a greater risk of being taken as a good-natured clod or an honest peasant.

There is then a good deal of evidence that people of different social class backgrounds differ with respect to salient speech characteristics, and that these variations influence selectively the perceptions of a listener. Is there corresponding evidence to show that teachers are influenced by the dimensions of a child's speech when making assessments of his background?

Williams (1970) found that teachers when listening to samples of children's speech used two underlying dimensions to make judgments about the children's social status: 'confidence-eagerness', largely a measure of fluency, inversely related to incidence of silent pausing, and 'ethnicity-nonstandardness', sounding ethnic and nonstandard in one's use of English. The speech samples used in this study were excerpts from the children's responses to standard questions in an interview situation. Therefore content was not controlled, so that what was said, how the message was organized, and errors in syntax could also have influenced teachers' evaluations. Confidence-eagerness in speech was related to the perception of the child as reticent and unsure in the speech situation, an interview. Characteristics that underlie ethnicity are deviations from standard English such as nonstandard syntactic constructions (e.g. He be going), and nonstandard pronunciation of selected phonemes. Perceiving a child as unconfident-reticent and nonstandard in his English was associated with judging him to be of low social class or 'sounding disadvantaged'.

There does then seem to be a relationship between a child's speech characteristics and the impressions a teacher forms of him, even though in Williams' study the speech samples were not equated for content.

More recently Seligman, Tucker, and Lambert (1972) examined in more detail the ways in which teachers form attitudes towards students. Their research technique involved presenting teachers with selected information about each of a number of third-grade boys, including an original composition and an artistic drawing, a photograph and a taped sample of speech of each child. These three types of information were randomly combined and presented to teachers with the request that they
form evaluative impressions of each pupil. The results indicate that speech characteristics were particularly decisive in the formation of overall impressions. Thus a child with a good composition, a cute and alert face, but with a speech style characteristic of a lower social background would be seen as limited in intelligence and ability. In this study, speech characteristics and photographic cues both significantly affected judgments of pupils' intelligence and academic ability.

In summary, it has been found that: (1) Teachers' expectations can influence children's performances. (2) Speech characteristics can influence the evaluations that a listener makes about a speaker. (3) Teachers use speech styles as a basis for making judgments about a child's social class background. (4) Members of the various social classes differ with respect to several characteristics of speech; the speech characteristics of lower class speakers prompt relatively unfavorable perceptions of their overall competence.

One vital piece of evidence is still missing. Is there a relationship between children's speech styles and their actual performance in school? If so then the relationship between speech style and scholastic performance could help account for some of the difficulties that lower class children have in school, especially since we know that important social class differences are marked in speech. Of course if a correlation is found between styles of speech and scholastic success, this does not necessarily mean that the causal relationship postulated does in fact exist. By itself the correlation between the two variables would only suggest that how one speaks will have an effect upon his success in school. However, in conjunction with the other research findings reported above, the argument for the causal relationship is considerably strengthened.

The same reasoning can be applied to another feature of the research to be discussed. In these studies we try to eliminate or reduce individual differences in social class background. We do this because we know that speech style and social class covary, and since we want to determine whether there is a relationship between speech style and scholastic performance independent of social class background. If we do find such a relationship, the truth of the proposition that the speech characteristics of lower class children affect their school performance will not have been demonstrated directly. But such an inference can be made in light of the research discussed above.

The basic study which prompted the present investigation is reported elsewhere in detail (Frender, Brown, and Lambert 1970). In that study two groups of third grade French-Canadian boys, all from lower social class backgrounds, were compared in terms of their speech characteristics, their intelligence, and their scholastic standing. One group had relatively high grades in school, the other relatively low grades. The two groups, one of relatively good students, the other of relatively poor
students, were equated carefully with respect to a non-verbal measure of intelligence and social class standing of the family.

For each boy, a short reading sample was collected, using a common passage. The speech samples were then rated by a linguist who was unaware of the purpose of the study. She evaluated each boy's speech on the following scales: Pronunciation: Articulate----Inarticulate; Pronunciation: Accurate----Inaccurate; Accent: Continental-style-----Canadian-style; Speed of Speech: Quick-----Slow; Intonation: Much-----Little; Intonation: Appropriate-----Inappropriate; Pitch of Voice: High-----Low; Voice Quality: Soft-----Hoarse; Speaker Sounds: Confident-----Unsure; Speaker Is: Fluent-----Not Fluent.

It was found that the better students read more quickly, but not at an extreme, used more intonation and used it more appropriately, sounded more confident, and had voices that were not as low pitched as those of the students with lower school grades. But it was also found that the two groups differed on their verbal intelligence scores. Since it was possible that the differences in speech characteristics were merely a reflection of the differences in verbal intelligence, a covariance analysis was conducted using verbal intelligence, non-verbal intelligence, and age as the covariates. This left the picture somewhat modified. The differences in speed of speech and confidence were no longer significant, but the difference in voice quality became significant, the poorer students sounding more hoarse. Thus with verbal intelligence controlled, the better students used more intonation and used it more appropriately, had higher pitched and softer voices than the poorer students.

This evidence offered some support for the proposition that there is a relationship between speech style and scholastic success. In fact, the preliminary study was encouraging enough to launch the following more comprehensive investigation.

Method

Subjects. The subjects in this case were 151 fourth grade boys from four English Catholic schools in Montreal. At the time of the study, these particular schools were segregated with respect to sex. School A (1 class), school B (1 class), school C (2 classes) were all located in very low socioeconomic areas of Montreal. School D (2 classes) was in a working class area of the city and about two-thirds of the boys in attendance were of Italian descent and in many cases Italian was the major or only home language. However, all of the children could speak English.

The following data were collected for each subject.

School Grades. This was a percentage score based on an average for nine academic subjects. The scores for the two terms of that school year were averaged.
Intelligence Measures. The Henmon-Nelson Test of Mental Ability (grades 3-6) was administered to each subject. This test of intelligence has a heavy verbal emphasis. The school board also supplied each boy's score on the Lorge-Thorndike test, a relatively 'non-verbal' measure of intelligence.

Metropolitan Achievement Test. The Elementary Form of this standardized test was administered. It consists of the following seven subtests: word knowledge, word discrimination, reading, spelling, language usage, arithmetic computation, and problem-solving and concepts. An average of the seven standard scores on these subtests was computed.

A questionnaire was sent home to obtain the following information.

Parental Education. Two scores were used: number of years of schooling for the mother and number of years of schooling for the father.

Socioeconomic Status. The family was asked to describe the father's occupation. With this information a score on a seven point scale of socioeconomic status (SES) was obtained using the scale devised for Canadians by Blishen (1964).

Home Language Index. The parents were asked to answer four questions. (1) What language does the mother use most often when talking to the children? (2) What language does the father use most often when talking to the children? (3) What language do the children use most often when talking to each other? (4) What language do the parents use most often when talking to each other? For each question, a score of 4 was assigned if the language was English, 3 if English and another language were mentioned, and 2 if English was not mentioned. The total of these four scores was tabulated, and constituted the home language index.

Speech Samples. Two different speech samples were recorded for each subject. The first was a reading of a twelve sentence passage entitled 'My Little Brother' (see Appendix A). Since there were many reading errors in the last sections of the passage, only the first four sentences were analyzed by the linguists. They were: This is my little brother. His name is Timmy. He is three years old. Sometimes I take care of Timmy. This sample constituted about 15 seconds of speech.

The second sample, about 40 seconds long, was a recital of a series of numbers (presented in digital form), followed by the naming of a series of colours (see Appendix A). These samples of speech were used to determine through comparison, how much the 'reading' of a passage affected the relationship between speech and school grades. However, the two types of speech samples also differed with respect to meaningfulness. That is, it is very difficult to recite a series of numbers in a meaningful way, and this restriction will affect intonation ratings, for example.

Linguistic Ratings. Two linguists, B and J, analyzed and rated all of the speech samples. Each listened first to all of the 'My Little
Brother recordings, working with thirty voices in each of five sessions. Each block of thirty speakers included five boys from each school class. The same procedure was then repeated for the recitals of 'Numbers and Colors'. This work was completed over a period of several days, and the linguists did not do the ratings in the presence of one another.

The following seven-point rating scales were used to evaluate both speech samples. A score of 7 was assigned to the left-most positions of the scale as shown below. On the actual scales used by the linguists, the 7 position was randomly placed on the left and right ends. Also, there were three different scale orders, and these were randomized in each session of thirty speakers.

(1) There is ___ intonation: Much------Little
(2) The intonation is: Appropriate------Inappropriate
(3) Voice quality is: Non-hoarse-------Hoarse
(4) Speed of speech is: Quick--------Slow
(5) Pronunciation: Standard-------Non-standard
(6) The voice is: Pleasant--------Unpleasant
(7) The child sounds: Male-like-------Female-like
(8) Pronunciation: Articulate--------Inarticulate
(9) Pronunciation is: Accurate-------Inaccurate
(10) Pitch is: Low---------High
(11) The child sounds: Confident--------Unsure
(12) The child is: Highly fluent-------Highly disfluent
(13) The child seems: Eager to speak-------Reticent to speak

Data Analysis. The method of analysis used was multiple linear regression. The traditional predictors of scholastic achievement (IQ, SES, parental education, etc.), and the speech variables that were found to be correlated to school performance were entered into the regression. The aim was to determine if the speech variables could increase the amount of variance explained in school performance, over and above that explained by the more traditional predictors. A step-wise multiple linear regression was used. In this procedure the independent variables are entered into the regression equation in a systematic manner. The variable having the highest correlation with the dependent variable is entered first. Variables are then entered such that the one having the highest partial correlation with the dependent variable, the effects of all previously entered variables being partialled out, is entered next. Only variables whose beta coefficients were significantly different from zero were included in the final regression equation.

Two regression analyses were performed: one used school grades as the dependent variable, and the other used Metropolitan Achievement Test scores as the dependent variable.

The program used was from the Harvard Data-Text collection, which is able to handle missing data. There was a small amount of missing
data due to absences on testing days, unreturned questionnaires and partially completed questionnaires. Table 1 shows the size of the Ns for the correlations between each of the independent variables and the dependent variables.

Results

1. Speech Ratings: Inter-rater reliability. The first question of interest is the reliability of the speech ratings the linguists have made. Is there a high degree of correlation between the ratings of the two linguists? This is important to know for the following reason: if there is really a significant relationship between speech style and scholastic success, but the measures of speech style being used are not reliable, the relationships that exist may not be detected.

Table 1 provides the answer to this question. At best the speech ratings are moderately reliable. The median inter-rater reliability for the 'My Little Brother' passage is .42, while for the 'Numbers and Colors' sample it is .46. It should be noted that for a scale like pitch which taps an objective aspect of speech the reliabilities reach the levels of .67 and .69. The reliabilities for the speech ratings on the 'Numbers and Colors' passage are higher in all cases except one, and this probably reflects the fact that they were based on a much longer sample of speech. It probably would have been desirable to use a somewhat longer reading sample to improve the reliability of the ratings.

Considering the relatively low reliabilities, and the fact that this would tend to obscure relationships that might in fact exist between speech parameters and scholastic success, the detection of any significant relationships should be all the more notable.

Before having proceeded to subsequent analyses, an average of the two linguists' ratings was computed for both the 'My Little Brother' and 'Numbers and Colors' passages, with the aim of making them more reliable.

2. Speech Ratings: The relationship between speech ratings on the two samples. Are the speech samples obtained from the two different passages, the one involving reading, the other approximating a recital, tapping into the same dimensions, or are they measures of different things? One way of answering this question is to correlate the speech ratings obtained from the two passages. Table 2 presents these correlations for linguist B's ratings, linguist J's ratings, and the averaged ratings of the two linguists. These tend to be rather low, moderately high at best. The median correlations for linguist B, linguist J, and their average are .31, .34, and .43. But considering that the reliabilities for each of the speech samples themselves are rather low, it is not then surprising that the inter-task correlations are even lower.
TABLE 1. Correlations between linguist B’s and linguist J’s ratings: Inter-rater reliability (N = 144)

<table>
<thead>
<tr>
<th>Speech Sample:</th>
<th>‘My Little Brother’</th>
<th>‘Numbers and Colors’</th>
</tr>
</thead>
<tbody>
<tr>
<td>Amount of intonation</td>
<td>.36</td>
<td>.47</td>
</tr>
<tr>
<td>Appropriateness of intonation</td>
<td>.44</td>
<td>.45</td>
</tr>
<tr>
<td>Hoarseness of voice</td>
<td>.20</td>
<td>.37</td>
</tr>
<tr>
<td>Speed of speech</td>
<td>.51</td>
<td>.59</td>
</tr>
<tr>
<td>Standardness of pronunciation</td>
<td>.50</td>
<td>.60</td>
</tr>
<tr>
<td>Pleasance of voice</td>
<td>.15</td>
<td>.45</td>
</tr>
<tr>
<td>Masculinity of voice</td>
<td>.29</td>
<td>.45</td>
</tr>
<tr>
<td>Articulateness of pronunciation</td>
<td>.40</td>
<td>.43</td>
</tr>
<tr>
<td>Accuracy of pronunciation</td>
<td>.42</td>
<td>.72</td>
</tr>
<tr>
<td>Pitch of voice</td>
<td>.67</td>
<td>.69</td>
</tr>
<tr>
<td>Confidence of speaker</td>
<td>.56</td>
<td>.58</td>
</tr>
<tr>
<td>Fluency of speaker</td>
<td>.55</td>
<td>.46</td>
</tr>
<tr>
<td>Eagerness of speaker</td>
<td>.35</td>
<td>.44</td>
</tr>
</tbody>
</table>

However, it can be seen that the correlations are, in all cases except one, higher for the averaged ratings than for either of the two linguists ratings taken separately. This can be attributed to the fact that averaging the ratings has made them more reliable.

The absence of a strong correlation between the speech ratings from the two different samples may reflect either the fact that the reliabilities of the ratings are low, or that the two tasks are not equivalent. In the case of an objective scale such as pitch which has relatively high reliability, it can be seen that the cross task correlation for the averaged ratings is a reasonable .77.

It thus remains to be seen whether the speech ratings from both passages are equally effective in explaining some of the variance in school grades.

3. The Relationship Between Speech Ratings and School Grades, Achievement Tests. Table 3 presents the simple correlation coefficients between the independent variables, IQ, mother’s education, father’s education, SES, home language index and speech ratings, and the dependent variables, school grades, and metropolitan achievement test scores.

Not surprisingly performance on both types of IQ test is more strongly correlated with achievement test scores than school grades. And in both cases scores on the relatively more verbal Henmon–Nelson test are more strongly correlated with the dependent variables, school grades, and achievement test scores. Interestingly, maternal education is more strongly related than paternal education to the dependent variables.

Of particular interest is the fact that the relationships between SES and the two measures of scholastic performance are essentially zero.
TABLE 2. Correlations between speech ratings on the 'My Little Brother' passage and the 'Numbers and Colours' passage (N = 144).

<table>
<thead>
<tr>
<th>Ratings:</th>
<th>Linguist B's</th>
<th>Linguist J's</th>
<th>Average*</th>
</tr>
</thead>
<tbody>
<tr>
<td>Amount of intonation</td>
<td>.27</td>
<td>.11</td>
<td>.28</td>
</tr>
<tr>
<td>Appropriateness of intonation</td>
<td>.22</td>
<td>.17</td>
<td>.29</td>
</tr>
<tr>
<td>Hoarseness of voice</td>
<td>.11</td>
<td>.24</td>
<td>.29</td>
</tr>
<tr>
<td>Speed of speech</td>
<td>.39</td>
<td>.33</td>
<td>.44</td>
</tr>
<tr>
<td>Standardness of pronunciation</td>
<td>.42</td>
<td>.49</td>
<td>.59</td>
</tr>
<tr>
<td>Pleasance of voice</td>
<td>.34</td>
<td>.39</td>
<td>.43</td>
</tr>
<tr>
<td>Masculinity of voice</td>
<td>.25</td>
<td>.53</td>
<td>.52</td>
</tr>
<tr>
<td>Articulateness of pronunciation</td>
<td>.28</td>
<td>.25</td>
<td>.38</td>
</tr>
<tr>
<td>Accuracy of pronunciation</td>
<td>.49</td>
<td>.41</td>
<td>.52</td>
</tr>
<tr>
<td>Pitch of voice</td>
<td>.63</td>
<td>.69</td>
<td>.77</td>
</tr>
<tr>
<td>Confidence of speaker</td>
<td>.22</td>
<td>.28</td>
<td>.33</td>
</tr>
<tr>
<td>Fluency of speaker</td>
<td>.22</td>
<td>.21</td>
<td>.28</td>
</tr>
<tr>
<td>Eagerness of speaker</td>
<td>.27</td>
<td>.35</td>
<td>.46</td>
</tr>
</tbody>
</table>

*This is the average of the two linguists' ratings

This suggests that a reasonably good job was done in keeping the social class standing within our sample relatively homogeneous. However, another possibility is that a good measure of SES was not obtained, since the experimenter had to rely on questionnaires brought home to the parents by the children, and as can be seen from Table 3 only 120 of these were answered in a satisfactory manner.

The home language index is more strongly related to performance on the achievement test than school grades, the correlations being .30 and .10 respectively. This suggests that the Italian children and others whose home language is not English are doing relatively more poorly on the achievement tests than on the school grades being assigned by the teachers on the basis of class performance. The same pattern emerges when one looks at the correlations between both the accuracy of pronunciation and standardness of pronunciation scales and the two dependent variables. For example, using the standardness of pronunciation scale from 'My Little Brother', the correlations are .34 with school grades and .47 with achievement test scores. Thus if one takes the standardized achievement test as an objective measure of scholastic achievement, and since the Italian children are doing relatively more poorly on this measure than on their school grades, it would be hard to argue that there was any bias against these Italian children when it comes to grading by the teachers. One might argue that the correlations between these pronunciation variables and the dependent variables, all of which are significant and moderately strong, reflect the fact that the Italian children
are being subjected to biased treatment. However, it must be remembered that there is a correlation between IQ scores and pronunciation, and thus what must be demonstrated is whether pronunciation explains any additional variance in school grades over and above that explained by IQ. (See Appendix B for correlation between pronunciation ratings and IQ scores.) Finally, it is not necessarily the case that standardness-accuracy of pronunciation is a measure of one’s ethnic background. For the correlation between a combined scale of standardness-accuracy of pronunciation and home language index is .28 and .43 using the ‘My Little Brother’ and ‘Numbers and Colors’ ratings (Appendix B).

Inspecting the rest of Table 3 to see what the relationship is between the speech variables and the dependent variables, one is first struck by the fact that in almost all cases the correlations for the ‘Numbers and Colors’ ratings are smaller than those for the ‘My Little Brother’ ratings, and in many cases substantially smaller. The only case where the correlations are both significant and essentially the same for the two speech ratings are on the accuracy and standardness of pronunciation scales. It should be recalled that these two scales had relatively high cross-task correlations. (See Table 2). It can be seen that the two intonation scales, the confidence and fluency scales have relatively strong relationships with school grades, but this is also true of their relationship with achievement test scores. The question that remains to be answered then is whether these variables will explain any additional variance in the dependent variables over and above that accounted for by the traditional predictors of scholastic achievement. Ideally, from the perspective of confirming the hypothesis, one would like this to be the case for school grades, but not the case for the metropolitan achievement test scores. For recall, the argument that is being advanced is that speech variables affect teachers’ perceptions and evaluations of a child. Since the teacher does not directly affect scores on the achievement tests one would not expect these speech variables to be related to performance on these tests, unless one wished to advance the argument that the treatment accorded the child based on the teachers’ perceptions actually affected his level of achievement. This is certainly not an implausible hypothesis; the Rosenthal experiment produced this type of result, but we are trying to prove a more difficult case.

4. Intercorrelations Among the Speech Ratings. Before proceeding to the multiple regression analysis, one other matter need be dealt with. The reader has probably noticed that some of the speech scales seem to be measuring much the same thing, for example standardness of pronunciation and accuracy of pronunciation or pitch of voice and masculinity of voice. If these scales really are measuring the same dimension, then it would be best to combine them for this would increase reliability. Table 4 presents the intercorrelations among the thirteen speech scales
TABLE 3. Correlations of independent variables with school grades and metropolitan achievement test scores

<table>
<thead>
<tr>
<th>Variable</th>
<th>School Grades</th>
<th>Achievement Tests</th>
</tr>
</thead>
<tbody>
<tr>
<td>Lorge–Thorndike IQ (non-verbal) N = 149</td>
<td>.52***</td>
<td>.60***</td>
</tr>
<tr>
<td>Henmon-Nelson IQ (verbal) N = 144</td>
<td>.76***</td>
<td>.84***</td>
</tr>
<tr>
<td>Mother’s education N = 129</td>
<td>.34***</td>
<td>.36***</td>
</tr>
<tr>
<td>Father’s education N = 120</td>
<td>.24*</td>
<td>.24*</td>
</tr>
<tr>
<td>SES (based upon occupation) N = 120</td>
<td>.03</td>
<td>.04</td>
</tr>
<tr>
<td>Home Language Index N = 130</td>
<td>.10</td>
<td>.30***</td>
</tr>
<tr>
<td>Intonation, Amount:</td>
<td></td>
<td></td>
</tr>
<tr>
<td>‘My Little Brother’</td>
<td>.43***</td>
<td>.44***</td>
</tr>
<tr>
<td>‘Numbers and Colors’</td>
<td>.07</td>
<td>.18*</td>
</tr>
<tr>
<td>Intonation, Appropriate N = 144</td>
<td>.43***</td>
<td>.46***</td>
</tr>
<tr>
<td></td>
<td>.17*</td>
<td>.27**</td>
</tr>
<tr>
<td>Voice quality, Non-hoarse N = 144</td>
<td>.23**</td>
<td>.26**</td>
</tr>
<tr>
<td></td>
<td>.03</td>
<td>.05</td>
</tr>
<tr>
<td>Speed of speech, Quick N = 144</td>
<td>.25**</td>
<td>.24**</td>
</tr>
<tr>
<td></td>
<td>.04</td>
<td>.02</td>
</tr>
<tr>
<td>Pronunciation, Standard N = 144</td>
<td>.34***</td>
<td>.47***</td>
</tr>
<tr>
<td></td>
<td>.30***</td>
<td>.46***</td>
</tr>
<tr>
<td>Voice, Pleasant N = 144</td>
<td>.12</td>
<td>.18*</td>
</tr>
<tr>
<td></td>
<td>.12</td>
<td>.04</td>
</tr>
<tr>
<td>Child sounds, Male-like N = 144</td>
<td>-.15</td>
<td>-.30***</td>
</tr>
<tr>
<td></td>
<td>.11</td>
<td>-.05</td>
</tr>
<tr>
<td>Pronunciation, Articulate N = 144</td>
<td>.16</td>
<td>.30***</td>
</tr>
<tr>
<td></td>
<td>.19*</td>
<td>.27**</td>
</tr>
<tr>
<td>Pronunciation, Accurate N = 144</td>
<td>.31***</td>
<td>.44***</td>
</tr>
<tr>
<td></td>
<td>.28***</td>
<td>.43***</td>
</tr>
<tr>
<td>Pitch, Low N = 144</td>
<td>-.05</td>
<td>-.18*</td>
</tr>
<tr>
<td></td>
<td>.06</td>
<td>-.10</td>
</tr>
<tr>
<td>Child sounds, Confident N = 144</td>
<td>.44***</td>
<td>.40***</td>
</tr>
<tr>
<td></td>
<td>.26**</td>
<td>.28**</td>
</tr>
</tbody>
</table>
TABLE 3. Correlations of independent variables with school grades and metropolitan achievement test scores (continued)

<table>
<thead>
<tr>
<th></th>
<th>School Grades</th>
<th>Achievement Tests</th>
</tr>
</thead>
<tbody>
<tr>
<td>Child is, Highly fluent</td>
<td>N = 144</td>
<td></td>
</tr>
<tr>
<td></td>
<td>.54***</td>
<td>.52***</td>
</tr>
<tr>
<td></td>
<td>.30**</td>
<td>.31**</td>
</tr>
<tr>
<td>Child seems, Eager</td>
<td>N = 144</td>
<td></td>
</tr>
<tr>
<td></td>
<td>.34***</td>
<td>.31***</td>
</tr>
<tr>
<td></td>
<td>.09</td>
<td>.12</td>
</tr>
</tbody>
</table>

*** = significant at the .001 level of probability  
** = significant at the .01 level of probability  
* = significant at the .05 level of probability

The speech ratings for 'My Little Brother' passage and the 'Numbers and Colors' passage are averages based on the two linguists' ratings.

for both the 'My Little Brother' ratings and the 'Numbers and Colors' ones. In each case the averaged ratings of the two linguists were used. Three correlations of a very high nature stand out, and they are consistent for both of the speech samples; the correlations between standardness of pronunciation and accuracy of pronunciation, .81 for 'My Little Brother' and .82 for 'Numbers and Colors', the correlations between pitch of voice and masculinity of voice, .71 and .66, and the correlations between confidence of speaker and fluency of speaker, .83 and .68.

In each of these cases, for the regression analyses to follow, the two scales were combined by taking their average. If one wonders why such strong intercorrelations are possible when, as has already been pointed out, the reliabilities for each of the scales were at best moderately high, the answer appears to be this. The ratings were not made independently, but at the same time, and this would tend to inflate the correlations. From an inspection of the correlation table it can be seen that there is a marked pattern of intercorrelations among the 13 scales. In fact, a factor analysis for both the 'My Little Brother' and the 'Numbers and Colors' ratings reveals in the former case a first factor which accounts for 40% of the variance and on which 12 of the 13 scales have loadings of at least .40 or better, while in the latter case, a first factor which accounts for 34% of the variance and on which 10 scales have loadings of .40 or better. This could lend support to the notion that the speaker's composite speech style (one might be tempted to say gestalt) has an effect which influences perceptions and judgments about the individual, and that even the linguists in making their evaluations of certain parameters may have been subject to this influence. Of course some part or all of the observed relationships may be due to the fact that these parameters are actually correlated.
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<td>‘Numbers and Colors’ (Bottom row)</td>
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TABLE 4. Intercorrelations between ratings on speech scales (continued)

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<td>10. Pitch of voice</td>
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<td>- .29</td>
<td>- .46</td>
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<td>11. Confidence of speaker</td>
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<td>12. Fluency of speaker</td>
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<td>.63</td>
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<td>13. Eagerness of speaker</td>
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<tr>
<td>The ratings are the average of the two linguists' scorings.</td>
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5. Multiple Regression Analyses Using School Grades and Metropolitan Achievement Test Scores: The hypothesis revisited. Table 5 presents the summaries of four different multiple regressions that were performed. The first of these was a stepwise multiple linear regression in which the dependent variable was school grades and the independent variables were verbal IQ, non-verbal IQ, mother's education, father's education, SES, home language index, and the four following ratings from the 'My Little Brother' speech sample; amount of intonation (scale 1), standardness-accuracy of pronunciation (average of scales 5 and 9), pitch-masculinity of voice (average of scales 7 and 10), and fluency-confidence of speaker (average of scales 11 and 12). A preliminary analysis had shown that the remaining speech variables would make no significant contribution to a multiple regression equation.

Briefly, what stepwise regression does is this. It first selects the independent variable having the highest correlation with the dependent variable, $X_1$, and enters it into the regression equation. Thus $Y = b_1X_1$, where $Y$ is the dependent variable. The independent variable selected to enter next is the one having the highest partial correlation with the dependent variable, the effects of $X_1$ being partialled out. Thus, the equation

$$Y = b_1X_1 + b_2X_2$$

This process is repeated, in each case the next independent variable entered being the one with the highest partial correlation with the dependent variable, the effects of all the previously entered independent variables being partialled out. The process was stopped when the variable to be entered next would have given a beta-weight or standardized coefficient, $b$, that would not have been significantly different from 0. That is, the contribution that the variable would have made to prediction would not have been statistically significant.

In regression 1, verbal IQ is the first variable to enter, it having the highest correlation, .76, with school grades. The multiple correlation, $R$, is at this point also .76, and the variance explained, $R^2$, is .578. The next three variables to enter are, in order of succession, fluency-confidence, pitch-masculinity of voice, and amount of intonation. Each makes a small, but statistically significant, contribution to prediction of the dependent variable. With the addition of confidence-fluency, $R$ becomes .781, $R^2 .611$, and thus an additional 3.3% of variance in school grades is explained. The addition of pitch-masculinity explains an additional 2.5% of variance, while amount of intonation explains another 1.6%. However, this should not be interpreted to mean that this is the amount of variance that is uniquely due to each of these variables, since by reversing the order of entry the amount of variance added by each variable would be changed. A somewhat better indicator
of the contributions that each variable makes is found by examining the reduction in the amount of variance explained, were the variable in question to be deleted from the final regression equation. This is what is shown in the column labelled unique variance. For example, were pitch-masculinity to be dropped from the equation the amount of variance explained would drop from .652 to .622, thus it has a unique variance of 3%. The standardized coefficients shown in the table are those for the final equation using the four independent variables. The t-values test whether each of these is significantly different from 0.

The meaning of both the fluency-confidence and amount of intonation contributions seems relatively straightforward, there being a positive correlation between fluency of speaker and amount of intonation used and his school grades. However, the effect of sounding relatively more male-like needs to be elaborated. As the reader may recall, it was shown in Table 3 that both the pitch scale and the masculinity scale had what were essentially zero correlations with school grades, and these correlations were if anything in the negative direction. Why is it then that having a lower pitched voice, or sounding relatively more male-like, is now positively correlated with school grades? The reason is this. Although the correlation between the pitch-masculinity rating and school grades is -.09 (see Appendix B), its correlations with verbal IQ and fluency-confidence are -.25 and -.34 respectively, but both of these are positively correlated with school grades. Thus the effect of pitch-masculinity is suppressed in the simple correlation coefficient. Although the boys who sound more male-like have lower IQ scores and speak less fluently which would tend to lower their school grades, it appears that working in the opposite direction is the effect of sounding more male-like which is positively related to school grades. One possible interpretation of this finding could be that the teachers tend to form more favorable impressions of boys whose speech they perceive as being more sex-role appropriate.

The question that remains to be answered is whether these same speech variables will be of predictive value in explaining some of the variation in metropolitan achievement test scores. From the point of view of supporting the hypothesis advanced, they should not, since the teachers do not directly affect the scores achieved on these tests. To test whether this was the case another regression analysis was performed using the metropolitan achievement scores as the dependent variable, and the independent variables from regression equation 1. This time instead of a stepwise analysis, the order of entry of the independent variables was forced, replicating the previously obtained sequence. Thus we were attempting to see if the regression equation which fit the school grades data would also be applicable for the standardized achievement test scores.

The summary table for regression 2 clearly shows that of the three speech variables, only the standardized coefficient for amount of
## TABLE 5.

| Regression 1: Stepwise Multiple Linear Regression  
| Dependent Variable: School grades  
<p>| Independent Variables: Traditional predictors, Speech ratings ('My Little Brother') |</p>
<table>
<thead>
<tr>
<th>Variable name</th>
<th>Standardized coefficient</th>
<th>T value</th>
<th>R</th>
<th>R²</th>
<th>Increase in R²</th>
<th>Unique variance</th>
</tr>
</thead>
<tbody>
<tr>
<td>Verbal IQ</td>
<td>.658</td>
<td>11.66***</td>
<td>.760</td>
<td>.578</td>
<td>.578</td>
<td>.324</td>
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<tr>
<td>Fluency-confidence</td>
<td>.204</td>
<td>3.40***</td>
<td>.781</td>
<td>.611</td>
<td>.033</td>
<td>.028</td>
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<tr>
<td>Pitch-masculinity</td>
<td>.188</td>
<td>3.57***</td>
<td>.797</td>
<td>.636</td>
<td>.025</td>
<td>.030</td>
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<tr>
<td>Amount of intonation</td>
<td>.150</td>
<td>2.64**</td>
<td>.808</td>
<td>.652</td>
<td>.016</td>
<td>.017</td>
</tr>
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</table>

| Regression 2: Multiple Linear Regression, Order of Entry Fixed  
| Dependent Variable: Metropolitan achievement test scores  
| Independent Variables: Verbal IQ, Speech ratings ('My Little Brother') |
| Verbal IQ      | .763                    | 15.31***| .841 | .707 | .707 | .435           |
| Fluency-confidence | .075                  | 1.42    | .847 | .718 | .011 | .004           |
| Pitch-masculinity | .008                   | 0.17    | .847 | .718 | .000 | .000           |
| Amount of intonation | .121                      | 2.43*| .854 | .729 | .011 | .011           |

| Regression 3: Multiple Linear Regression, Order of Entry Fixed  
| Dependent Variable: School grades  
| Independent Variables: Verbal IQ, Speech ratings ('Numbers and Colors') |
| Verbal IQ      | .768                    | 14.03***| .760 | .578 | .578 | .511           |
| Fluency-confidence | .059                  | 1.03    | .761 | .579 | .001 | .003           |
| Pitch-masculinity | .216                   | 3.86***| .788 | .620 | .041 | .039           |
| Amount of intonation | .026                      | 0.45    | .788 | .621 | .001 | .001           |

| Regression 4: Multiple Linear Regression, Order of Entry Fixed  
| Dependent Variable: Metropolitan achievement test scores  
| Independent Variables: Verbal IQ, Speech ratings ('Numbers and Colors') |
intonation is significantly different from zero. Thus it appears that both the fluency-confidence variable and the pitch-masculinity one do not explain any additional variance in standardized achievement tests over and above that explained by verbal IQ.

To summarize, using the speech ratings obtained from the 'My Little Brother' sample, it has been demonstrated that there is a small, yet significant, relationship between grades received in school, and pitch-masculinity of voice and fluency-confidence of speaker, with the possibility that the amount of intonation used is also related.

Do these findings replicate using the ratings obtained from the 'Numbers and Colors' speech sample? To answer this the equation obtained from regression 1 was forced using school grades as the dependent variable and the 'Numbers and Colors' speech ratings as the independent variables. From the summary table for regression 3 it can be seen that this time only the pitch-masculinity variable seems to make a significant contribution, and it has a unique variance of 3.9%. As would be expected, and as shown in the summary for regression 4, none of the 'Numbers and Colors' speech variables explain any of the variance in metropolitan achievement test scores.

Why is it then that both the fluency-confidence and amount of intonation variables for the 'Numbers and Colors' ratings do not explain any additional variance in school grades, when these same ratings for the 'My Little Brother' speech sample did so? One possibility is that the findings from the first equation are due to chance. However, the standardized coefficients for these variables were significant at probability levels of .001 and .01, although it must be mentioned that the N was rather large, 146. Another possibility is that it is very difficult, as well as inappropriate, to invest any meaning into a recital of a series of numbers and colors. This should then have an effect on intonation ratings. Similarly, reciting unmeaningful material may not bring out
the normal speech patterns of an individual, although it is possible to level the same criticism at the reading of a passage. Thus it may be that the 'Numbers and Colors' speech sample does not capture some of the salient dimensions of speech style that are reflected in the reading of a passage. The fact that on the scales in question the cross task correlations (see Table 2) are rather low is consistent with this interpretation. However, as mentioned previously, this may be due mostly to the low reliabilities for these ratings.

Discussion

The results described above are consistent in demonstrating a relationship between pitch-masculinity of voice and school grades, but are somewhat more mixed in supporting the relationship of fluency-confidence and amount of intonation with school grades.

It is clear that some problems remain to be answered. Why is it that in this study having a relatively lower-pitched, more male-like voice is positively correlated with school grades, when in the previous study of Frender, Brown, and Lambert (1970) it was found that the students who were doing more poorly in school were the ones with the relatively lower pitched voices? This seems quite puzzling at first, and caused the authors some consternation. The most probable answer seems to be this: In the 1970 study, the students with the better grades also had higher scores on the test of verbal intelligence. In this study we find that verbal intelligence is positively correlated with having a relatively higher pitched voice. (See Appendix B.) Since verbal intelligence was probably the major determinant of the grades received, it is not then surprising that the group with better grades in that study also had higher pitched voices. Even though the effects of verbal IQ were subsequently controlled by using a covariance analysis, this is not the same as equating the groups in reality with respect to verbal IQ. Thus even if a relationship similar to the one found in the present study between pitch-masculinity and school grades existed in the previous study, the design of that study unfortunately would not have enabled us to detect it.

The reader may wonder (assuming the results reported in the present study prove to be reliable) why the relationships between speech parameters and school grades are relatively small. In designing the study it was not anticipated that these relationships would be large, for speech style is at best one of the many variables that determine a student's grades in school. Furthermore, by restricting the range of SES of the students sampled it is likely that the range of speech styles was restricted, and this by itself would tend to diminish the size of the correlations. However, in conceiving the study it seemed important to demonstrate that speech style had an effect independent of social class, otherwise it could
have been argued that some other aspect which was correlated with social class, and not speech style, would have been responsible for any correlation that might have been obtained.

Also it should be remembered that moderately high simple correlations were found between several aspects of speech style and scores on both IQ tests and standardized achievement tests. In explaining the variation in school grades, all of this covariation between speech style and IQ was attributed to IQ's share of variance explained in school grades. But what if students who speak in a manner that is perceived unfavorably are subsequently dealt with by the teacher in such a way that results in these children being less highly motivated or involved in their school work? This might affect their actual level of achievement as reflected by the IQ or standardized achievement tests. If this were the case, then our findings, if anything, would be underestimating the relationship between speech style and scholastic success.

Up to this point, only relationships between variations in selected speech characteristics and variations in school grades have been demonstrated. What about the implications for social class differences in scholastic attainment? It seems quite unlikely that there are social class differences in pitch of voice, and Brown (1969) did not find this with his French speaking adults. However, he found that both fluency-confidence and amount of intonation were correlated with social class. Thus if the same is true for children of the various social classes, and the findings reported here prove to be reliable, then it seems that there is a possibility that the speech styles of the lower social class children may be the source of an unfair bias against them.

In this regard it should be remembered that Williams (1970) found a relationship between dimensions that he labelled confidence-eagerness and ethnicity-nonstandardness and teachers' judgments of the 'disadvantagedness' of children. Strictly speaking the teachers are probably reflecting what is a real relationship between styles of speech and social class standing; like the rest of us, they are aware of some of the correlates of social class. But if they use an individual child's style of speech to make judgments about 'disadvantagedness', and then on that basis make inferences about his ability to learn, it is not unlikely that some children will suffer an injustice. What is especially dangerous is that there tends to be a moderate correlation between certain aspects of speech style and IQ. This may tend to reinforce a priori notions, and lead to their being overgeneralized.

One further aspect of the results needs to be mentioned. In this study, no relationship between standardness-accuracy of pronunciation and school grades was detected, once the effects of verbal intelligence had been controlled or partialled out. Thus in this particular sample it appears that there is no systematic bias reflected in the grades being given to the Italian children. This is reassuring. It is true that their
school grades were somewhat lower, but this trend was even more pronounced in their performance on the IQ and standardized achievement tests. Of course one might argue that their poorer performance on all three measures is a result of some bias, but after spending a considerable amount of time in the schools in question, the authors would not support this argument. A more likely explanation is that these children have not yet mastered the necessary English language skills to the same degree of proficiency as the English children.

The problem of replicating results of the sort reported here poses a problem. There is no reason why these findings should consistently turn up. Some teachers may not be affected by speech styles in dealing with children, or may have learned to compensate against tendencies to be influenced by irrelevant factors. Thus a failure to replicate in any particular instance may not be a disproof of the existence of the phenomenon being discussed.

It is customary for many research reports to end with the cautionary note that more research need be done. Although we will follow suit, it is with the intention of pointing out some of the problems of the present piece of research.

While relationships were found using the speech ratings from the 'My Little Brother' sample, some of these were not replicated when using the 'Numbers and Colors' ratings, and this may be due to the lack of meaning of this material. Klaus Scherer (who has studied the influence voice characteristics have on the attribution of personality traits) has pointed out to us that he found it was necessary to record meaningful samples of speech. It is also possible that the results found in our study for fluency-confidence and amount of intonation may not be associated so much with speech style in general, as with the ability to read orally. However, if speech style when speaking spontaneously is correlated with speech style when reading this would not constitute a problem. It would be desirable to replicate these findings using samples of spontaneous speech, but then content is left uncontrolled. We have no solution for this problem. Scherer (1970) solved the problem by first recording meaningful samples of speech, and then cutting up his tapes and splicing them together in a random manner. However, he was interested in the effects of voice characteristics that would survive this type of mutilation.

Future research should endeavour to produce speech ratings that demonstrate a higher degree of inter-rater reliability. Whether using longer samples of speech will do the trick remains to be answered. It is also not inconceivable that on some of the scales low reliabilities may be the result of the same speech style being perceived differently by different listeners. This should be true with regard to more subjective evaluations of speech style. But as was shown earlier there was a
relatively high degree of intercorrelation among the linguists ratings on the thirteen scales, so even the more objective ratings may be influenced by the linguist's overall perception of the speaker.

In summary, the work reported here was guided by the hypothesis that 'how a child sounds' may influence his success in school. For some it may be hard to believe that factors as insignificant as speech characteristics could have any relationship to ultimate performance in school. In this regard it seems useful to conclude with a brief mention of an illuminating piece of research conducted by Ray Rist (1970). Through observation of a kindergarten class, Rist provided evidence that the development of expectations by the teacher as to the differential academic potential and capability of any student was significantly determined by a series of subjectively interpreted attributes and characteristics of that student. Eight days after school had commenced, the children in the class being observed had been divided into three groups based on perceived similarity in expected performance, although no formal tests of cognitive potential had been administered. From Rists's point of view it appeared that attributes highly valued by middle class Americans were the basis for these divisions. These were: 'ease of interaction among adults; high degree of verbalization in Standard American English; the ability to become a leader; neat and clean appearance; and coming from a family that is educated, employed, living together and interested in the child; and ability to participate well as a member of a group' (Rist 1970:422). Rist then goes on to detail how these children were differentially dealt with, and how through the second grade the divisions instituted in the kindergarten class tended to remain relatively intact despite changes in teachers.

It is the hope of the writers that this work may sensitize teachers and others who deal with children to the tendency, inherent probably in all of us, to sometimes use idiosyncratic and irrelevant characteristics when evaluating other human beings.

APPENDIX A. 'My Little Brother' passage

This is my little brother.
The linguists listened
His name is Timmy. to this section of the
He is three years old. passage only.

Sometimes I take care of Timmy.
I hold his hand
when we cross the street.
He is too little
to watch for cars by himself.
Sometimes I help Timmy
dress in the morning.
I help him button the buttons
and zip the zippers.

I take Timmy to the park,
and push him in the swing.
I show him how to pump
with his feet
to make the swing go high.

Once when I was sick, Timmy
brought me his teddy bear
to play with.
He wanted to make me feel better.

The passage consisted of pages cut out from a children’s reader. The print was fairly large to minimize reading difficulties. Since there was a substantial incidence of reading difficulties with the passage after the first four simple sentences, it was decided to use only them for the speech analysis.

‘Numbers and Colors’ passage

1  2  3  4  5  6  7  8  9  10
21 32 43 54 65 76 89
101 134 127 265 448 963
1,143 2,655 3,789 4,672

Brown  Purple  Blue
Orange  Yellow  Black
Green  Grey  Red

The colors were actually square pieces of colored paper. The names were not printed on them.
### APPENDIX B. Correlation matrices for variables entered into multiple regression equations

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<tbody>
<tr>
<td>1. Lorge-Thorndike IQ (non-verbal)</td>
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<td>2. Henmon-Nelson IQ (verbal)</td>
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<td>3. Mother's Education</td>
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<td>4. Father's Education</td>
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<td>5. SES-Father's Occupation</td>
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<td>7. Amount of Intonation ('My Little Brother')</td>
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<td>8. Pitch-Masculinity ('My Little Brother')</td>
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<td>9. Fluency-Confidence ('My Little Brother')</td>
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<td>10. Pronunciation, Standard-Accurate</td>
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**Note:** The values in the table represent the correlation coefficients between the variables.
### APPENDIX B. Correlation matrices for variables entered into multiple regression equations (continued)

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<td>9. Fluency-Confidence ('Numbers and Colors')</td>
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Ns vary from 109 to 149, 120 being the smallest for a correlation with a dependent variable.
NOTE

1The research discussed was supported by Canada Council and the Canadian Defense Research Board grants to the Language Research Group at McGill University.

The authors would like to thank the following people for their help and advice: James Ramsey, David Armor, and Robert Rosenthal for statistical advice; Jerome Kagan for thoughtfully discussing the results; François Christen who helped us collect much of the data; Betsy Gibbons who helped transcribe data, and punch IBM cards; Bruce Barkman and Marilyn Jessen who were kind enough to make linguistic ratings. Mr. Louis Dugal, director of guidance for the Montreal Catholic School Board, who advised us on the choice of appropriate schools, and facilitated our entry; the teachers, principals, and students of the schools for their time and cooperation.

REFERENCES


A GRAMMARIAN LOOKS TO SOCIOLINGUISTICS

CHARLES J. FILLMORE

University of California at Berkeley

1. Linguists of the 'generativist' tradition seek to discover, for each language, a system of formal principles capable of determining the grammatical sentences in that language and capable of associating with each of these sentences those structural properties on the basis of which it can be correctly interpreted.¹ Many of the methodological assumptions linked with this definition of the grammarian's task have been challenged from time to time in the last fifteen years; and many of these challenges, and the reactions they have produced, are relevant, I think, to the subject of this year's Georgetown Round Table.

In my first sentence I used the phrase the grammatical sentences. The objections to the generativists' initial view of their task can be organized around a discussion of each of the three words in that phrase. The first word is the. It has been assumed that there is, or ought to be, a determinate set of sentences for a grammar to characterize, and to support that assumption grammarians have needed either to take as an idealization a static and uniform speech community unrealizable in the real world, or to act as if the proper object of linguistic inquiry is a private-access body of data known technically as 'my dialect'. There are thoughtful scholars who have not wished to make either of these choices. The second word is grammatical. The decision to define the set of 'grammatical' sentences in a language as the data to be explained by a theory of grammar—as opposed, say, to a set of sentences found in a fixed body of texts—requires a reliance on certain sorts of intuitive judgments of native speakers. These judgments concern not only the grammaticality of sentences that belong to the language without question, but also, necessarily, the exclusion from sentencehood of certain candidates for grammaticality. Unhappily, the recurrent embarrassment of...
the generative grammarian is that his students and his critics are for- ever contriving situations in which the sentences he had needed to be- lieve were ungrammatical turned out to be completely appropriate. 2

The third word in my phrase is sentences. It has been assumed, in a
great many linguistic traditions, that the maximum unit of analysis,
the unit within which all 'purely linguistic' generalizations have their
scope, is the 'sentence'. But this is surely wrong.

2. The grammarian who has faced these problems sees no hope of
constructing a grammar capable of assigning grammaticality indices to
sentences unless the grammar can be taken as, or as a part of, a theory
which takes into account linguistic contexts larger than sentences, as
well as a great many facts about the process of communication, the
functions of linguistic performances, and the social occasions and liter-
ary forms in which given sentences can have given functions.

I would like to go through a chain of arguments that leads one to this
conclusion. Consider first a sentence like You didn't take any apples.
Our grammarian must surely regard this as a grammatical sentence of
English. He has failed in his work if his grammar does not contain the
principles according to which this sentence can be generated. These
principles must specify the form of negative sentences with simple
verbs, the dependence of any on a set of contexts including negativity,
and so on.

Next, look at a larger sentence: You didn't take any apples, and
Jimmy didn't, either. Here we have a compound sentence, once again
something clearly a grammatical sentence of English. Our grammarian
must ensure that his grammar provides for sentence conjunction, allows
for verb-phrase deletion in the second of two clauses that are related in
the way these two are, assigns contrastive stress on an unlike subject
in the second of two parallel clauses, as well as de-emphasis of didn't,
and accepts either at the end of the second of two conjoined negative
clauses, certain other conditions being satisfied.

But now consider a sequence of two sentences: You didn't take any
apples. Jimmy didn't, either. The second sentence in the sequence is
'authorized' by the first sentence in the same way as what we saw for
the second clause of the compound sentence, but here the generalizations
have to be seen as applying across a sentence boundary.

A solution that is not too unnatural is that of regarding juxtaposition
as a type of conjunction, and to say that although what we seek is basic-
ally a grammar of sentences, the notion 'sentence' needs to be general-
ized to include sentence sequences in coherent discourse.

But now consider a case where the two sentences are spoken by dif-
ferent people. A says You didn't take any apples, and B says Jimmy
didn't, either. The same principles hold here, too, principles govern-
ning stress placement, ellipsis, and the rest; but the term 'sentence'
cannot decently be stretched to include sentence sequences in discourse to which there is more than one contributor.

Grammatical theory, somehow, must take into its scope, as we have seen, conversation or two-party discourse as well as one-party discourse. Some of the new problems the theory must face can be understood by considering a two-line conversation in which A says You didn't take any apples and B says You didn't either. Here contrastive stress is assigned to the subject of the second sentence because it has a different referent, though not a different form, from the first sentence. The two sentences constitute a well-formed piece of discourse only if they are spoken by different people. Take now the opposite case, a conversation in which A says You didn't take any apples and B says Because I knew they were rotten. This time the pronoun in the second sentence is de-stressed because it has the same referent as the pronoun in the first sentence, though, of course, it has a different form. It is obvious, I think, that a theory of grammar must be informed by a theory of conversation, and certain understandings about deixis and pronominal reference that make up part of that theory.

Once the grammarian has made that decision, he becomes aware that in the way that some sentences are limited to occurrence as next-sentence in a discourse, some occur only as next-sentence in a discourse at a point where there has been a change of speakers. An illustration that does not only involve pronouns can be found in the sequence Thank you—You're welcome, a sequence which is appropriate only if its performance is divided between two speakers.

As a first approximation to the truth, we might be tempted to say that the only thing the grammarian needs to say about You're welcome is that it is an acceptable second-part next-sentence for Thank you. My next point, however, will be that a theory of conversation must necessarily take into account the functions of utterances.

Take a trivial example first, an exchange in which A says You're welcome and B says Thank you. We understand this as an exchange in which A is reminding B that B owes him an expression of gratitude and B, perhaps grudgingly, complies. We must, in short, identify the functions of the two utterances and the sequencing expectations that are determined by those functions, in order to explain both the bizarreness of the reversed order and the way we can interpret it.

But in fact we can imagine contexts in which the sequence Thank you—You're welcome is inappropriate. Consider a three-line conversation in which A says You have lovely eyes, B says Thank you, and A then says You're welcome. The sequence can be given an interpretation, of course, but we recognize it as bizarre by realizing that the function of You're welcome is partly that of acknowledging that one has done somebody a favor. A compliment cannot stand as a compliment if its speaker acknowledges that in saying it he has done his subject a favor.
There are still larger sequences in which the judgments get reversed again. Consider this time the following conversation: A says You're a linguist, you ought to be able to help me. I'm writing this paper and I need to come up with an example of an indicative sentence in English having a second-person subject, and I can't for the life of me think of any. Can you help? B says You have lovely eyes. A says Thank you and B then, appropriately, says You're welcome.

Matters are different there, of course, because one of the sentences is, as they say, 'mentioned' rather than 'used'. But it is precisely our understanding of the sentences and our knowledge of the workings of a conversation which enables us to know which of the sentences we just overheard was capable of being a quoted sentence and which were not. And that is what we needed to know in order to find the conversation acceptable.

Take an even more ridiculous case. A says Good morning. B says What did you say? A says What did you say? and then B too, again says What did you say? Here there are three occurrences of the same sentence, but the range of interpretations possible for each of the three is different. A says Good morning. B says, What did you say? to indicate that he did not hear what A said. A says What did you say? either to indicate that he did not hear what B said or to give an untruthful answer to B's question. B then says What did you say? either to indicate that he didn't hear what A said this time either, or to give a truthful answer to A's question.

The point of all this is that conversations, or discourse samples in general, are not well-formed or ill-formed as such, but only on particular interpretations. These interpretations have to be based on assumptions about what is going on in the conversation, who says which lines, what has been said before, whether one is lying or telling the truth, and so on. There is no way, in short, of talking about grammaticality or well-formedness without getting in many ways involved in the details of social interaction by means of language. Inquiry into the ways in which settings authorize linguistic choices can be made respectable and fruitful when motivated by something nobler than the desire to show that some grammarian is mistaken with his asterisks.

3. The issues in sociolinguistics that will concern me most directly are those of what has been called 'microsociolinguistics', the study of language behavior in encounters between people on particular social occasions. We can refer to knowledge of appropriate situated language use as 'communicative competence' and express it as the ability to bring into association (1) instances of language behavior, (2) communication act functions, and (3) the classes of social occasions in which the particular instances of language behavior can have the given functions.
A sociolinguistic model of communication must specify the various possible components of a communication event and must provide a typology of the settings in which communicating takes place in a community, the participant roles which these settings create, and the functions which individual communications can have in these settings. Something along the line of the Bühler-Jakobson model, as this has been exemplified and made familiar to this audience by Hymes, can serve us as a useful starting point, especially, I think, if we are not particularly concerned to work out the pairing that Jakobson sees between the 'factors' and the 'functions' of speech.

The basic factors of a communication event are these: the identity of the sender of a message, the identity of the intended receiver or addressee of the message, an awareness in the sender of an interceptor or witness to the communication event, the code shared by the interlocutors, the topic and the specific content of the message, the encoded form of the message, the properties of the channel through which the message is transmitted, the setting or social occasion within which the message plays a role, and the function which it serves in that setting.

In talking about the various functions and aspects of situated language use, we can take any of various points of view: the sender’s point of view can be usually expressed as the sender’s intentions, the receiver’s point of view can be taken as the receiver’s reactions, and the analyst’s is the interpretation of what is going on between sender and receiver.

I will now attempt to summarize the various communication act functions, keeping in mind these separate points of view where necessary, and giving some indication of the linguistic concomitants of messages having these functions.

Take first the job of notifying the addressee that there is going to be some communicating. The receiver’s work is to know whether he is being addressed. If he is a physician named Abercrombie and feels somebody poke him on the shoulder and hears somebody say Dr. Abercrombie, his job is easy. If she is a young lady in a crowded street and hears somebody say Oh, Miss!, she does not know for sure. The sender’s job is more complicated. He must decide whether or not he wishes to be polite, because on the basis of that decision he will choose between, say, Oh, sir! and Hey, mister! He must decide whether he knows the person’s name, title, age, sex, status, race, kinship affiliation, etc., in order to know whether to say Oh, Mommy, Mr. Smith, Doctor, Jimmy, Hey, gringo or what you have.

The sender can act to identify himself by giving his name, or—to take the receiver’s point of view—he can identify himself unintentionally by means of his voice quality, his handwriting, or his style, for example. The job the sender has in identifying himself to his addressee involves deciding whether or not the addressee recognizes his voice, knows his name, has visual contact with him, has a specific kinship
relationship to him, and so on. On such matters depends the choice among such locutions as It's me, This is Harvey Schwartz, I'm Harvey Schwartz, I'm Mr. Schwartz, My name is Harvey Schwartz, and It's Daddy.

The so-called 'contact' or 'phatic' function of communicating, which can be thought of as absorbing the matters of speaker identification and addressee notification, is that of keeping the channel open, of guaranteeing that the receiver is attentive. I am inclined to separate this into two functions, the 'contact' function by which the sender is assured or attempts to guarantee that the message is being processed in real time (consider the uh huh, uh huh of speech) and the 'phatic' function by which people 'keep in touch' for the sake of maintaining a social relationship between them conducive to communicating even though they may have very little to say.10

The 'emotive' function of language is seen in language behavior which expresses (to take the sender's point of view) or exposes (to take the receiver's point of view) the sender's real or pretended inner life, his attitudes and feelings. Certain lexical items, like gosh, ouch and alas are limited to locutions serving this function. Certain grammatical processes seem also to be so limited, for example the use of so/how with adjectives, as in He's so tall! and How tall he is!, or the type of adverb inversion exemplified by Little did I suspect that I was being betrayed and Never had I seen such a sight, to speak only of English. And, of course, there are a great many paralinguistic cues to one's inner life, some typically under conscious control, others not.

The 'reactive' or 'conative' function of communication has to do with the effect of a message on the feelings, actions or understanding of the receiver. Again, situations can be described from the point of view of the sender's intentions or the receiver's reactions. 'Teasing' can be thought of from the point of view of the sender's malice; 'persuading' can be defined in terms of the beliefs or actions induced in the receiver; 'insulting' can be taken as ambiguous in this respect, as we can see by noting that both of these usages are acceptable: first, He didn't even know that I'd been insulting him and I didn't mean to, but apparently I insulted him.

A taxonomy of the things that people do to each other when they communicate can be one basis of a semantic analysis of communication-act verbs in a language, and that classification in turn would be a good start toward an analysis of the specific communication-act functions that are associated with particular choices of linguistic material. I am inclined to think that in fairly precise ways the appropriateness of choices of lexical items, paralinguistic features, some grammatical and phonological processes, will be determined by the nature of the communication act, whether it be insulting, blessing, flattering, accusing, asking, commanding, promising, or what have you.
The so-called 'referential' function of language has to do with the communication of the 'propositional content' of the message. Again there is the difference between what the speaker intends to communicate and what the receiver is able to figure out. In the most typical case of communication, the receiver is not able to figure out what the sender intends. In certain subtle cases, the receiver is able to figure out more than what the sender intends; some receivers are reputed to be skilled in this way to a particularly high degree, and these are: psychiatrists, private detectives, and Kremlinologists.

In the analysis of the communication of content, the analyst's point of view takes one into an awareness of the greatly detailed mass of background information which is shared by the interlocutors and which accounts for many of the deletions, pronominalizations, instances of definite reference not explained in the context, and the use of presuppositions unjustified in the context.

The aesthetic or 'poetic' function of language can be spoken of from the receiver's point of view in terms of his appreciation of the way in which the message form was constructed; from the sender's point of view it consists in making choices so that future receivers will appreciate the resulting form. The analyst, taking his aloof point of view, asks such questions as whether the conventions which determine values in this area are universal or community-specific, whether the receiver's appreciation takes the form of the belief that the effect he perceives was intentional on the part of the sender, and so on.

The 'situational' function of communicating has to do with the ways in which the interlocutors' perception of the setting or social occasion is related to linguistic choices they make. From the sender's point of view the relation can be either a determined or a determining one. In some cases, that is, the speaker's perception of the nature of the setting guides his choice of linguistic form, while in some cases choices which the speaker makes in speaking are what determine the nature of the occasion, or the style level.

It is not likely that there is a fixed and finite number of discrete types of social occasions provided by a given culture for communicative interaction; but it is clear that many fairly well-defined types of them can be enumerated—as, for example, the insult ritual between close friends, psychotherapy sessions of the nondirective type, classroom discussions on topics where the teacher has mastery of the subject and the students do not, haggling between customer and merchant on the price of an item, and so on. These occasions can be ranged according to the degree to which the associated language behavior is highly routine or fairly unconstrained. And they can be divided into occasions in which the interaction is, to use a distinction due to Gumperz, 'transactional' or 'personal'. If it is transactional, the exchange has a fixed purpose and there is a fixed way of achieving that purpose and acknowledging that it has been achieved.
Social occasions can also differ from each other by the specific ways in which the usual expectations of normal, friendly conversation are suspended; this typically shows up, as pointed out in recent unpublished work by Michael Moerman, as a difference between types of interaction periods which do or do not have fixed beginnings and ends. When the committee meeting is called to order, several changes occur: interaction becomes transactional, only certain topics will be welcomed, the right to speak is narrowly limited, and the time at which the participants can return to normal sorts of conversation needs to be clearly marked. Before a class session formally begins, it can be expected that people will ask questions under the normal conditions; once the class is started it then becomes appropriate for one member of the group to ask questions that he thinks he already knows the answers to, or in fact to answer his own questions if nobody else does. Before the session with one’s psychiatrist begins, one expects a question like \textit{When are we gonna meet next time?} to receive as a response something like \textit{What about next Tuesday at four o’clock?} Once the session has begun, the same question would elicit instead the response \textit{Why did you ask that?} It is obviously of great importance for discourse analysis to have secure knowledge, on a given text, of the nature of the interaction or the purpose of the communication, since expectations about sequencing and topic and right to speak may be very different from one setting to another.

4. The linguistic properties by which differently situated instances of appropriate language behavior can differ from each other are extremely extensive.

They include the choice of a code, as in the switch from classical Arabic to colloquial Arabic along with a change from lecture to class discussion,\textsuperscript{12} using standard Japanese rather than the local dialect when politeness is called for,\textsuperscript{13} using Spanish for courting, but switching to Guarani once the purpose of the courtship has been achieved,\textsuperscript{14} and so on.

Certain sorts of activities bring into play certain linguistic routines, activities such as marrying, congratulating, greeting, offering condolences, making an arrest, introducing people to each other, and so on. Somewhat similar is the choice of particular locutions or grammatical properties in connection with specific types of activities. Examples are telling people about their inner wishes as a way of giving instructions (\textit{You wanna make a left at the next corner.}), using the phrase \textit{talk about} when estimating a sum of money (\textit{We’re talkin’ about $60,000.}), using demonstratives while scolding (\textit{Get that beaver out of this house this instant!}) or asking stereotyped scolding questions like \textit{How many times have I told you not to bring your beaver into the living room?}

The choice of personal pronouns and address forms can vary, in widely different ways in different communities, as studied in classical
papers by Brown and Gilman, 15 Paul Friedrich, 16 and Brown and Ford. 17 And there is much more to be done.

The appropriate choice of lexical items can be determined by such things as the age, sex, race, class membership, occupation or education of the sender, addressee, or perceived audience. Javanese, for example, as we have learned, has a wide range of synonyms differing in level of politeness. 18 Medication is a middle-class substitute for the upper-class and lower-class word medicine. Honey, as a term of address, is used to and by waitresses in restaurants of the Holiday Inn variety and downward, in my observation. For all I know, it may be used elsewhere, too, but almost never, I would guess, by a male to a male.

It may be that inflectional categories, as in Koasati, or conversational sentence particles, as in Thai, are chosen on the basis of the sex of sender and/or addressee. 19 Different patterns of stress and intonation usage for the two sexes have been noted frequently. That paralinguistic features are selectively used in the same sorts of sociolinguistic settings that we have been considering has recently been amply documented in a survey article by Crystal. 20

5. The connection between appropriate language choices and the social settings in which their use can be authorized may be thought of in the style of the 'ethnomethodologists' in terms of the 'work' which the participants in a conversation must perform. This work consists in general in detecting certain controlling features of the social situation, in knowing what it is that one wants to accomplish in this situation, in remembering or being able to reconstruct the relevant expectations regarding language behavior for such settings.

The nature of this 'work' can be illustrated by returning to the matter of speaker-identification. If Mr. James Smith is checking in at a hotel where he has made advance reservations, he says My name is James Smith, because he knows that that is the name the inn-keeper must look up. If he is introducing himself to a small child in a context in which it is necessary to provide the child with an address form for him, he says I'm Mr. Smith. If he's introducing himself to a new neighbor and wishes to give the neighbor the right to determine the level of friendship they should expect of each other, he says I'm Jim Smith, thus allowing the other the option of calling him either Jim or Mr. Smith. If he is introducing himself in one of those modern settings that call for intimacy but not familiarity, as in a sensitivity training group, he says I'm Jim.

Or take a situation discussed in slightly different terms by Robin Lakoff, 21 where a hostess is offering a guest a piece of cake. If the guest is a small child, our hostess knows that no child is embarrassed to have it known that he is hankering after a piece of cake, so she can simply say, You can have a piece of cake, Jimmy. In offering cake to
an adult, the hostess can ask if the guest would like some cake (Would you like to try a piece of this cake?) or can say something which presupposes that the guest does not want any (You must have a piece of this cake. I insist.); but the offer You can have a piece of this cake will not do. 22

6. The question a grammarian must ask when he considers the various matters I’ve been talking about is: Where does autonomous linguistics—if there is such a thing—fit in this picture? I despair of answering this question, because although I would like there to be a special job that only linguists can do, I am not at all sure what that would be or whether it would be interesting. I do have a proposal, which I will present at the end of my talk, on how to find out what autonomous linguistics is; but I can warn you now that it is not a very practical proposal.

I no longer believe that it makes sense to talk about a grammar generating a set of grammatical sentences in a language, unless the term ‘grammatical’ means nothing more than ‘capable of being parsed.’ I think of the grammarian’s job as that of discovering and describing the elements, the structures, the processes and the constraints which are somehow made available to the language user as instruments for communicating, but I find myself more and more tending toward the study of how and for what purposes and in what settings people ‘use’ their grammars.

Many of the side issues of linguistic theory require an accounting in the larger theory—as, for example, grammaticality judgments, the processes of language change, and the treatment of linguistic ‘mistakes’.

There are clear cases of grammatical sentences (I love you.) and there are clear cases of ungrammatical strings of words (the a of of); but it seems to me, as I have already indicated, that decisions about the unclear cases will have to be based on understandings of situations of use and not on such a formal basis as that of finding the simplest grammar which generates all the clearly grammatical sentences and fails to generate all the clearly ungrammatical sentences and letting the grammar make the decision for the unclear cases.

Theories of language change must take into account the fact that settings are not only determining of but determined by language behavior. That is, I can select a certain class of lexical items, or transformations, or an articulatory set, or a voice quality or hesitation pattern, either because I perceive that I have a particular position in the conversation group, a position of affinity or authority, for example, or I can choose linguistic material for the sake of symbolizing and therein creating that affinity or that authority. I can center my vowels to let you know that I am one of you, if that is what it takes; or I can pronounce all my underlying /r/s to let you know that I am being formal or pompous or what have you. As Weinreich and others have pointed out, neither of these
influences on the final form of my speech can be explained on the basis of such a minimal sociological variable as 'density of communication'.

Speech modification that comes about as a result of consciously or unconsciously imitating the speech of the people one wishes to become affiliated with creates an interestingly asymmetric situation, because certain kinds of complexities in a linguistic system unperceived by the imitator make the imitation process obvious to members of the imitated group (all of which suggests to me that being a spy must be extremely risky work). To one person, the difference between who and whom is a difference determined by certain syntactic facts; to another person, the principle is that educated people say whom in lots of places where the rest of us say who. (The practical part of this principle is: When in doubt, say whom. Recently a secretary at Berkeley, on being asked if Professor Berlin was in, said Professor whom?) To one person the difference between /nyuwn/ and /nuwn/ is a difference in the phonological representation of two lexical items, the two items being different at both ends; to another, the principle is that your better type of person says /nuw/ where ordinary people say /nuw/, and such a person ends up as the Columbus, Ohio, radio announcer who introduces the /nyuwn nyuwn/.

Data about linguistic 'mistakes' thus permit some sort of accounting in sociolinguistic terms. In the extent to which speakers of a language are motivated to seek mastery of more than one code or style or must acquire control of many different sets of expectations governed by different sorts of social occasions, the speaker must frequently monitor his own productions to evaluate them according to his imperfect perception of the setting and his imperfect mastery of the expected sorts of language behavior. The relationship between the kinds of mistakes, stammerings, false starts, missed agreements, etc., and the contexts in which one feels insecure or carefully attended to must be partly explainable in terms of the complex task that a sender has.

7. It has sometimes been proposed that sociolinguistic principles can be incorporated into a theory of grammar. Some years ago, from this platform, DeCamp proposed that transformational grammar was particularly hospitable to the facts of sociolinguistics, especially as this concerned dialect or style, because of the use of binary features and the redundancy rules by which the choice of one property of a word or segment commits a speaker to the choice of other properties. If a structure could be marked as [+ Pompous], say, then the presence of that feature could trigger certain transformations, could require the selection of certain lexical items and the rejection of others, could trigger or constrain certain phonological rules, and so on.

The list of features one might think of for this purpose could include terms like Pompous, Deferential, Insulting, Female-Speaker, Male-
Speaker, Child-Speaker, Emotive, Excogitative, Hysterical, and innumerable many more; but it is difficult for me to see what these features could possibly be features of. If they are to be features of lexical items, then the theory will require an extremely elaborate system for assigning style or dialect judgments to the selection restriction apparatus in some way. If they are to be features of the sentence as a whole, then it is probably not true that transformational grammar is especially suited for sociolinguistics, because I know of no satisfactory proposals for dealing with ‘features’ of elements larger than lexical units. But much more seriously than that, the one fact that I find least suited to the incorporation of sociolinguistic features into the grammar of a language is that exactly the same sorts of conditions that in one community determine the choice of pronouns or inflectional endings, determine in another community the choice of one language rather than another.

I have no doubt that the theory of transformational grammar is flexible enough to get around that embarrassment--by accepting the performative analysis, by taking a strong version of the theory of a universal semantic base, by assuming a universal theory of sociolinguistic variables and by assigning sociolinguistic features to the highest performative verb--but when an analysis requires that much use of brute force, the facts that led to the analysis are much more interesting than the theory which got reshaped to incorporate them.

8. The central theoretical problem as I see it, is that of determining how to capture the ability of a person to know his language. I find it helpful to conceive of the total theory of linguistic abilities as a very specialized branch of Incarnation Theory. Suppose we assume that some minor god wishes to cross the line, wishes to pass as a member of the human community. It has the standard Divine Sensorium by which it is able to perceive in an instant all of space and time, it is able to assume any form or to occupy and control any existing creature, but it needs to be told how to talk and what constraints it needs to impose on its unlimited potential in order not to give away its divine origin. It will need to acquire the local grammar, or maybe more than one local grammar, and a large portion of the lexicon of that language or those languages. It will need to identify itself as a member of the community with respect to age, sex, family position, social status, educational background, occupation, geographical origins, etc.; and it will need to equip itself with a fairly coherent set of opinions about the world and a set of affective preferences, together with a strategy for changing these, though this last is not necessary for an adult. It will need to be able to perceive among its interlocutors whether they are people it is supposed to know, how its biography is shared with theirs, if and how they are related to it, and so on. It will need to notice what sorts of social occasions it finds itself in or what sorts of social settings its own action has put it
in. It will need to know what linguistic conventions and routines govern conversation on these occasions, what it and its interlocutors are expected to accomplish with each contribution to the conversation, when it can can appropriately talk at all and when it should remain silent. And it will apparently need to know how to vary its speech from time to time and how past decisions to vary its speech in one respect might require it to vary its speech in some other respect more often or less often so that certain proportions come out right.

This is an enormous undertaking, as you see—and even at that I have said nothing about what it will need to know about how people perceive the world in order for it to acquire the semantics of the system—but it all seems to be part of the job.

My proposal for determining the boundary between linguistics and other disciplines connected with a speaker's control of language use, is to start a project for writing an instruction manual for an Immigrant of the sort I have in mind, to have on this project a large and capable research team with workers from a great many academic disciplines, and to determine empirically which tasks the linguists can carry out without any help from the others.

NOTES

1 For a discussion of some of the practical and conceptual difficulties with this definition, see my paper 'On generativity' in P. Stanley Peters, ed., The goals of linguistic theory, Prentice-Hall, to appear.

2 I do not wish, on this last point, to be understood as putting down armchair linguistics. Far from it. I am pointing out only that the intuitive judgments which the grammarian needs to consult cannot be those of grammaticality alone.

3 I think that one way to become particularly conscious of the working of context on appropriateness judgments for discourse is to practice constructing sample texts for which no supporting context is conceivable, and to practice devising contexts for contrived or bizarre texts. Schegloff has mentioned certain locutions that are appropriate over the telephone but not in face-to-face conversation [Emanuel Schegloff (1968), 'Sequencing in conversational openings', American Anthropologist, 70. 1075-1095], and I have been interested in situations of the opposite sort. One might be inclined to think that a text like the following, containing something of each type, has to be bad. **Hello, this is Chuck Fillmore. Could you send over a box about yea big?** Speaker identification by means of the formula This is X is not appropriate in face-to-face encounters, yet the size-indicator yea requires that the interlocutors be in visual contact. A setting which could authorize the text, however, is one of communication by videophone. Texts which cannot be contextualized at all are fairly easy to construct, as for example, a letter that begins **To whom it may concern** and ends **As ever, Mary Lou.**
The term, I believe, was first used by Joshua Fishman.


My name for the first term of this triad is left deliberately vague, so as to include such things as the choice between speech and silence, the choice between one language and another, the choice between first name or title plus last name, the choice between performing or not performing a contraction, the choice of a paralinguistic feature, etc.


We can take as the simplest case one in which all three of these points of view are identical: the receiver reacts in precisely the way that the sender intended, and both sender and receiver perceive the situation in the same way that the analyst does. (The secretary, for example, in the usual way and for the usual purpose, says Good morning to her boss.) Cases where the sender's and receiver's points of view differ include cases of deceit, where the sender knows better what is going on than the receiver, cases of 'humoring' someone, where the receiver has the upper hand, or cases of two-way miscommunication, as in conversations between linguists and sociologists. A case where the sender's and receiver's points of view are identical but differ from that of the interpreter is the case where the interlocutors share a background of expectations, values and beliefs which the analyst must discover for himself or must impute to the situation in order for it to make sense to him.

The cultures that are the most comfortable to live in are those which offer their members a well-defined repertory of types capable of providing the content of messages whose function is merely phatic. 


Paul Friedrich (1972) 'Social context and semantic feature: the Russian pronominal usage', in John Gumperz and Dell Hymes, eds.,
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22The guest, in her turn, may confess to the most debilitating lust. Something like Oh, all right, why not? is inappropriate, whereas a sordid confession like The very sight of it makes my mouth water is quite in place.


RULES IN LINGUISTIC, SOCIAL, AND SOCIOLINGUISTIC SYSTEMS AND POSSIBILITIES FOR A UNIFIED THEORY*

ALLEN D. GRIMSHAW

Indiana University

Until recently there have been two principal planks in the platform from which I proselytize sociolinguistics to my fellow sociologists. First, I have argued that by ignoring sociolinguistic materials sociologists have overlooked data which: (1) could be of critical value in research on traditional sociological concerns and, (2) is simultaneously easy to collect (cheap) and characterized by measurable and criterially satisfactory reliability and validity characteristics. Second, I have attempted to demonstrate the problematic character of much of that large proportion of the sociologist's data which is obtained in interrogative encounters—simply because we so often must question people about behaviors and attitudes not easily subject to direct observation. These arguments are becoming familiar to an increasingly large number of our colleagues in the social sciences and further demonstrations of their validity have occurred during the workshops on the first day of the Georgetown Round Table meetings. I will not repeat these arguments today (see, inter alia, Grimshaw 1966, 1969a, 1969b, 1969-70, 1971, 1972b).

Nor will I use my time to talk about some of the fairly straightforward gains which might come to linguistics proper through adopting (or adapting) certain technological features of sociological methodology. Linguists themselves are increasingly persuaded of the necessity of good research designs and of the usefulness of certain statistical concepts and techniques. Linguists are also learning, through the imperatives of interpreting their own data, of the importance of social contexts for linguistic behavior—as can clearly be seen in their increasingly frequent invocation of socially based presuppositions.
I will talk instead about a new plank now being rough hewn for my platform—the claim that development of a unified theory of sociolinguistic description (Hymes 1972a) can provide us with an extremely powerful set of concepts to help us in our search for a more comprehensive unified theory of social behavior. I will talk about the possibilities for constructing typologies of universal (extrasystemic) and categorical (intrasystemic) rules which can be used to identify parallels (or at least analogies) in the rule-governed nature of linguistic, social, and sociolinguistic systems.\(^1\)

As a sociologist, I have long been aware of the fact that certain social relationships seem to occur—though in different manifestations—in all societies. The relationship of superordination-subordination, so nicely discussed by Simmel, is a case in point. (On this relationship and perspective more generally, see Simmel 1950, Grimshaw 1970.) My recent involvement in working with linguistic and sociolinguistic materials has led me to the conclusion that it is highly probable that there are similarities in organizational principles for social behavior in all societies just as there are for that particular variety of social behavior that we call speech, or talking, and that it ought to be possible to write rules for these similarities just as linguists write rules for the human behavior that they study. I am increasingly convinced that: (1) there are deep structures and surface structures of social interaction just as there are for language, (2) there quite possibly are universal relations in social interaction (or social structure) which hold in all societies but which are differently manifested in surface behavior after undergoing different social transformational rules, (3) there may very well be grammars of social interaction for societies and a grammar of social interaction in a quite precise analog to the linguist's rules of a grammar and rules of grammar.

The goal of a general or unified theory of social (or, more broadly, human) behavior is not new. Before turning to a discussion of the types of rules which I think may be useful in still another assault on the problem it may be instructive to turn briefly to an examination of the form and content of some earlier and, for us particularly relevant, attempts. In the past two decades two books have appeared which have included in their titles the words 'unified theory of human behavior'. The first, which appeared in 1956 (Grinker, ed.) reported on a series of conferences in which a group of distinguished scholars representing a range of disciplines from anthropology to zoology (and including philosophy, mathematics, and psychiatry but not linguistics) attempted to distill from the systemic approaches to their several fields the fundamental components of such a general theory. The second, which appeared in a revised and enlarged edition in 1967 (Pike), is the work of a single linguist who draws deeply on the literatures of many cognate (and sometimes more distant) fields in an attempt to generate a non-discipline-
bound interpretation of human behavior. (There are also more modest attempts, for example, the joint project attempting to integrate anthropological, psychological and sociological perspectives which culminated in the Parsons and Shils (eds.) volume, Toward a general theory of action, 1951.)

Each of these volumes condenses (or distills) an impressive amount of knowledge about the human condition and the ways in which humans in social groups behave. Each is stimulating and provocative. Yet neither has had the impact on the scholarly community which was, I imagine, hoped for by those who toiled to produce it. The Grinker volume is occasionally cited; the citation is most frequently in the form of reference to the work of a particular author rather than of genuflection to the notion of the unified theory. I have been surprised to find that Pike is rarely cited, even by those attempting to construct theoretical frames for interpretation of the same kinds of data he used in developing his unified theory.

I do not think the reason for this failure to influence theoretical developments is attributable either to disinterest in the task of constructing a unified theory or, solely, to its magnitude (although Grinker observes that the reason for a joint enterprise was that there seems to be a shortage of Newtons capable of integrating all the elements required for the kind of theory contemplated!). I think, rather, that Grinker and his colleagues failed because in moving toward a small set of general processes (reduction of great disciplinary richnesses to a theoretical common denominator) they arrived at banality. According to Grinker they ended up concluding that only three general principles "could be dignified by the concept 'unified'". These three principles were: (1) homeostasis, (2) transaction ("a reciprocal relationship among all parts of the field and not simply an interaction which is an effect of one system or focus on another"), and (3) communication of information. It was subsequently suggested that either communication theory or homeostasis themselves might be general theories covering 'several universes'. I do not doubt that all this may be true; but I could not help wondering what happened to the interesting notion of value orientations in coping responses to 'five common human problems of key importance'—(Kluckhohn 1956) or to the richness of Jules Henry's 'invariant equations' (Henry 1956) and his explicit assertion of their status as universal relational rules.

My characterization of the efforts of Grinker and his colleagues is obviously an unfair caricature; I personally believe that much can be gained by a careful study of their attempt. The same can be said of the monumental volume produced by Pike, which in its nonsuccess is also filled with a very large number of potentially fruitful suggestions, both for the organization of already existing knowledge about language and other social behavior and for new directions in research.

Pike's effort fails, I believe (and my characterization here is no more fair than that of the Grinker volume), because he has erred in a
direction opposite to that of the multi-disciplinary effort. The book appears to be an encyclopedia of neologisms in which Pike has taken one perspective within linguistics—that of tagmemics (tagmatics) and attempted to directly transfer that perspective and an extremely unwieldy taxonomy and vocabulary (e.g. S-tagmemes and I-tagmemes become fundamental elements in analysis of social and personality [individual] systems, respectively) to analysis of social behavior more generally. While Pike is very useful in his identification of materials which need to be incorporated into any complete analysis of social behavior, the analysis itself consists primarily of: (1) naming discrete minimal units in the several systems (linguistic and social), (2) the suggestion of a set of processes of combination, and (3) the listing of attributes of systems (e.g. substitutability, hierarchy). While there is invocation of the notion of intersystemic relations (see 580ff.), the discussion never focuses on the rule-governed nature of the phenomena we are studying. In brief, Pike gives us an apparatus for isolating minimal units but doesn’t tell us how they are systemically interrelated and integrated; and Grinker and his colleagues have generated an apparatus which leaves us with very general rules for systems but only the vaguest idea of what the minimal units may be for those systems in which we are primarily interested.

Let me state this conclusion more positively. I believe that in his development of concepts like distribution, focus, segmentation, and substitution, Pike has provided us with a frame that permits us to say what languages and societies may look like and even something about how language and society operate. He has given us an apparatus for a ‘what description’. By ignoring rules and rule discovery procedures, however, Pike has failed to give us the apparatus for a ‘why description’.

Grinker and his colleagues, on the other hand, have, in their identification of the principles of homeostasis, transaction, and communication, provided a set of abstract or metatheoretical rules about constraints on rules in any system and thereby have implied a rule discovery procedure. They have, then, given us an apparatus which can be used to generate a ‘why description’ or, at the least, to guide a search for such a description. There are, as I have already noted, fragments of rule systems and of minimal unit catalogs in the contributions of individual participants to the Grinker volume (e.g. in the Henry and Kluckhohn chapters, respectively). These are not organized, however, either into a ‘what description’ or into a conceptualization of an apparatus for such a description.

Any useful unified theory will minimally include both what descriptions and why descriptions. These two volumes can, jointly, start us off in the right direction.

In the remainder of this paper I will try to demonstrate that the statement of rules (of different levels of abstraction) permits both what and
why descriptions of behavior systems. In the next section I will discuss briefly some additional reasons for looking for rules (beyond their usefulness in generating what and why descriptions). In the third section I will talk about different levels and types of universal and system specific categorical rules. In the final part of the paper I will offer tentative illustrations of these different levels and types of rules in linguistic, social and sociolinguistic systems. Then I’ll run! I will not talk about rule discovery procedures; I have suggested already that I have found linguists to be useful role models for work on rules. (I have sketched preliminary steps for a search for social rules in an earlier paper, Grimshaw 1972a.)

Why the search for universal rules? One reason why scholars in any discipline are interested in finding invariant rules or laws is the same as that given by mountaineers for assaults on remote and previously unscaled mountain peaks, viz. ‘Because they’re there’. Scientists hold axiomatically that there is order in the universe they study; whether their own particular universe is that of molecular particles, of celestial movement, of language, or of social behavior. As we shall see below, linguists have historically given their attention directly to a search for universals holding for the organization of all languages and sociologists have looked at patterned concomitant variation rather than searching for invariant rules; both, however, have accepted the search for patterning as central to the intellectual task of their respective disciplines.

A second reason for interest in universals is methodological in scope (and therefore related to the essential role of rules in constructing what descriptions and why descriptions), viz. statement of universals facilitates research through narrowing problems. If the researcher knows that certain attributes will be present and that certain relationships will obtain in any case of a phenomenon he intends to study it will not be necessary for him to start each study of a newly discovered galaxy, or language, or social group, de novo. There will be some things he already knows. One of the purposes of identifying universals, then, is to provide a framework within which the scholar can more parsimoniously locate that set of things he ‘already knows’. And, in those instances where the expected attributes or relations are not easily identified, he will be directed to analyses which show these underlying features in different expressions and responding to different embedding contexts. In this way the student will be led to new insights, variously on the nature of the universe, of society, of man.

There is another set of reasons, almost a congeries rather than a complex. If there are indeed universals of human behavior, whether that behavior be linguistic, social interactional, or something else, it follows that identification and study of these universals can tell us something about the nature of the human mind. It may not tell us
directly about limitations on mental organization because, being human, we cannot see what other organizational principles might occur. But it will tell us something about mental activities shared by all humans.

This fact is variously interpreted. Some say that it is evidence for genetically preprogrammed capacities for linguistic (and, presumably, social interactional) competence. Some say that it shows that human behavior, like other behavior in other systems, follows rules of order and logic which are intrinsic in the universe. Others, who may or may not subscribe to one or the other or both of the first two interpretations, say that this commonality in human behavior shows the wisdom and creative powers of God.\(^2\)

It is possible to envision another instrumental application of knowledge about universals. Knowledge is at least potential power; and it has been suggested that knowledge of the working of the human mind could be used to invent means of controlling human behavior. I don't believe that this is a motivation for most linguists and sociologists (although benign control may be appealing in some applied areas); I don't believe either that it is a reason to avoid studying universals. If such possibilities exist it will be better if men and women of good will find them first (London 1969).

The discussion of reasons for looking for rules has been directed to the search for universals. Many of the reasons for searching for universals also hold for attempts to identify categorical rules which hold for specific systems. This also is true of the brief historical statement about rules I want to make now.

Mathematicians and logicians have long been in the business of stating invariant 'laws' (these 'laws' have, of course, frequently been found to be spurious or to be limited to particular mathematical or logical systems). Physical scientists and natural scientists have been engaged for some centuries in the search for invariant relationships which can be stated as laws (the same caution holds for these laws). Linguists have been able to identify universal 'attributes' of languages; in the past they were less interested in relational statements and tended to discard as irrelevant those aspects of language behavior which could not easily be incorporated into their descriptions of languages as entities. Sociologists have only occasionally attempted to identify characteristics of all societies (except in the fairly general descriptions of sociological concepts like status and role which are found in introductory texts) and have generally believed that the behavior they study is so complex that they can only make contingent relational (probability) statements. This has not been an unprofitable posture for sociologists—a good deal is known about social behavior in social structures. Sociologists have been reluctant, however, to claim knowledge of the underlying laws which must hold for social behavior just as for any other behavior in an ordered universe.
Recent work by linguists has shown an increasing interest in variable behavior; note for example the current discussion of 'presuppositions'. Recent work by sociologists has shown an increasing interest in the possibilities of discovering invariant behavior in social interaction; the work of some ethnomethodologists is one reflection of this trend. It is plausible that these shifts in perspective may converge in sociolinguistic research and that it is in sociolinguistic data and analyses that linguists will come to recognize the ordered nature of what they have in the past called 'free variation' (or grammatically irrelevant performance), while sociologists come to recognize that some social behavior always occurs in the same way.\(^3\)

In summary, there are a number of reasons for searching for rules governing human behavior—some instrumental and some aesthetic.

Levels and types of rules. As is not infrequently the case with topics on the frontiers of scholarly disciplines, there seems to be considerable debate among linguists as to just what kinds of language attributes or relationships can properly be labelled as linguistic universals.\(^4\) Some linguists seem to make only the quite conservative claim that all languages share an organizational scheme whereby each individual language is constituted by syntactic and phonological rules and discrete lexical items (Langacker 1966:241-242).\(^5\) Others prefer a perspective in which they suggest hypotheses which make increasingly strong claims about universal grammar, e.g. (1) the base rules for any language are constructed out of a fixed set of elements, (2) there is a fixed set of universal grammatical relations such as 'subject-of', 'object-of', (3) the actual rules of the base are the same for every language (Bach 1968:113-114). It will be seen from the discussion following that similar perspectives can be invoked in organizing searches for social interactional or sociolinguistic universals.

In outlining sets of rules for linguistic, sociological, and sociolinguistic systems, I have chosen a strategy which makes claims on the first two but not the third level of Bach's increasingly strong claims. I distinguish between extrasystemic universals and intrasystemic rules. I further distinguish between types of universals and levels of strength of claim in intrasystemic rules. Universal rules are those which hold for all language, social, or sociolinguistic systems. Intrasytemic rules hold for individual systems; they may hold for more than one such system, they are not true of all systems of their type.

Extrasystemic universals. There are two types of universals: (1) metatheoretical, and (2) substantive or empirical. I am using the designation metatheoretical to identify those universals which reflect features which are found in all systems and as characteristics of all theories.\(^6\) There are three kinds of metatheoretical universals which
can be easily identified for purposes of the task at hand. They are:
(1) all linguistic, sociological, and sociolinguistic systems are characterized by order (structural and/or temporal), (2) all three systems are constituted out of specifiable sets of components or minimal units, and (3) all three systems manifest specifiable sets of internal relations and have characteristics which permit them to meet specifiable sets of functions.

Substantive or empirical universals are attributes or relations found in all systems of a particular kind for which there is no metatheoretical explanation. We must assume that the reason that there is no such explanation is that we simply haven’t found it yet; the long-range fate of empirical universals is that they should come to be subsumed in some metatheoretical universal (or, possibly, that exceptions will be discovered). There are two types of substantive universals: (1) absolute and (2) quantitative. An example of an absolute substantive linguistic universal is that there are restrictions on base rules, e.g. we star S → Object-Subject-Verb in any language. Lexical and phonological marking provide examples of quantitative substantive linguistic universals. In both the absolute and quantitative cases we have phenomena which are found in all languages; we do not yet have theoretical explanations for this fact.

Intrasystemic Rules. While linguists have long been engaged in a search for linguistic universals, they have not, until very recently, attended to the variable character of rules ‘within’ systems. During the late 1960s William Labov, whose earlier work on linguistic change (1963, 1968a) and the social stratification of language ‘in use’ (1964, 1966a, 1966b) had already reflected his sociological training with its emphasis on concomitant variability, began to direct his attention to the specification of invariant and variable rules within linguistic (and sociolinguistic) systems. Labov’s demonstration that much of that language behavior previously labelled as ‘free variation’ is, instead, governed by variable ‘rules’ has had a revolutionary impact on linguistic (and sociolinguistic) theory and research. This impact is in no way reduced by the fact that once rule-governed variability had been demonstrated many linguists had an ‘of course’ reaction—a response many sociologists had earlier had to Goffman’s disclosures of undiscovered obviousnesses.

Labov talks about three levels of intrasystemic rules (he retains the notion of ‘free variation’; behavior falling in that category is generally uninteresting). The three types of rules are: (1) categorical rules, (2) semicategorical rules, and (3) variable rules. Labov’s initial summary chart characterizing these three types of rules, violations of rules, and reportability of violations is reproduced here as Chart 1.
### CHART 1. Three types of behavioral rules: violations and their responsibility

<table>
<thead>
<tr>
<th>A speech event: ‘Asking for directions’</th>
<th>Linguistic forms</th>
<th>Properties of violations</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Type 1: Categorical</strong></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Linguistic forms</th>
<th>Properties of violations</th>
</tr>
</thead>
<tbody>
<tr>
<td>This is my twoth cookie.</td>
<td>Occur very infrequently, less than 5% Interpretable. Reportable. Response: ‘He did?’</td>
</tr>
<tr>
<td>‘Miss Hayashima?’ ‘Yes, sir.’</td>
<td>‘What’s your name, boy?’ ‘Dr. Poussaint.’ ‘What’s your first name, boy?’ ‘Alvin.’</td>
</tr>
<tr>
<td>The Soul has bandaged moments--</td>
<td></td>
</tr>
</tbody>
</table>

| **Type 2: Semicategorical** | |
| ‘Where’s the Empire State Building?’ ‘Go ask a cop!’ | ‘Miss Hayashima?’ ‘Yes, sir.’ |
| ‘I want to get to the Empire State Building. Where is it?’ | ‘What’s your name, boy?’ |
| ‘... Thanks. By the way, can you loan me twenty cents for the subway?’ | ‘Dr. Poussaint.’ ‘What’s your first name, boy?’ ‘Alvin.’ |

| **Type 3: Variable** | |
| ‘Hey, Mac, how d’ya get t’ the Empire State Building?’ | Just nobody didn’t hit nobody. Nobody just didn’t hit anybody. |
| ‘Excuse me, officer, can you tell me how to get to the Empire State Building?’ | I say, ‘Calvin, I’m bust your head for dat.’ I said, ‘Calvin, I’ll break your head for that.’ |

Source: William Labov sociolinguistics seminar syllabus (1968)
The history behind Chart 1 is, of course, considerably more complex than the very brief discussion above indicates. Labov has been working on the identification of variable rules in linguistics since 1961. His ‘Contraction, deletion...’ paper (1969) is the principal empirical demonstration of the existence of these rules; at the end of that paper he outlines further goals of: (1) the working out of the internal structures of linguistic rules, (2) the contrasts of variable with invariant properties, (3) how rules change, and, (4) how children acquire rules. Labov has emphasized that variable rules have no significance except as they are contrasted to invariant properties; his 1968b ‘Negative attraction...’ paper is addressed to the empirical demonstration of these differences.

Simultaneously with the work on properties of rules themselves Labov has been working out the details of the typology (Chart 1) and on the question of how variable rules become categorical or semi-categorical. The principal data base for an attack on this latter question has been in investigations of sound change; the principal empirical demonstration here is Labov’s re-analysis of his Martha’s Vineyard materials in his paper, ‘The internal evolution of linguistic rules’ (1969a).

The use of violations to characterize the contrast properties of rule types is, of course, a continuation of the approach of Chomsky and of Weinreich. The concept of ‘reportability’ of violations, however, is drawn from Labov’s work (with Waletsky) on narrative analysis (1967) elaborated in the ‘Harlem study’ (Labov, et al. 1968). The notion of reportability is analytical (though reporting occurs empirically); in Chart 1 the notes on responses and reportability are drawn from the work on narrative analysis of Labov and his colleagues.

There are, then, four empirical bases for the development of the typology of categorical (invariant), semi-categorical, and variable intrasystemic linguistic rules. They are: (1) identification of variable rules, (2) differentiation of properties of variant and invariant rules, (3) delineation of processes of change of types of rules, and (4) reportability. The particular sociolinguistic value of Labov’s contribution has been his extension of the rule typology to speech events (a theme in his teaching in sociolinguistics since 1968) and his observation of the contrast in emphases in linguistics and in the (other) social sciences (viz. linguistics has historically developed categorical rules and the other social sciences variable rules) and encouragement of current moves to correct the one-sidedness of these emphases.

Categorical rules. Categorical rules are invariant rules for specific systems (languages or societies) which will not be violated by competent native members. Indeed, violators will be assigned statuses either as non-natives or incompetents. Thus, to use a linguistic example in English (linguistic examples, as will be seen below, are generally
easier to find), no native speaker will say, Go wash you. As I have already noted, there are frequently other specific systems which share a categorical rule; not all systems will. It may be, of course, that some categorical rules may become substantive universals, just as we expect that some substantive universals will become metatheoretical universals.

There are two very difficult, though hopefully not intractable, problems associated with the identification of categorical rules. Since categorical rules are system specific, the first problem is that of establishing the boundaries of systems. It might be suggested that the relevant system for linguistic analysis is the 'homogeneous speech community'. But is this satisfactory? In the case of English do we mean the 'English speaking community' of the world (with the directly implied inclusion, for example, of Indian English); do we mean American English; do we mean American Standard; do we mean that Negro Non-standard is a system? How do we handle regional and class dialects? Do we handle the case of Indian English by including only native speakers as members of the speech community? Obviously not, because the example given above (of the reflexive) holds for competent English speakers however exotic their dialects may be. In the case of bilinguals, do we attempt to establish some quantitative measure of interference and simply say that if more than some specified amount of interference occurs a speaker is not a member of the community subject to categorical rules of the system? Such a solution seems unlikely both because we have no adequate measures of this sort and because of the dangers of tautology introduced (viz. competent speakers don't break categorical rules—those who break categorical rules are not competent speakers!).

The problem also obtains with regard to sociological and sociolinguistic systems. It is clear that national boundaries cannot, in many cases, be used as boundaries of social systems. Many nations include large numbers of specific social systems within their boundaries; in the case of the United States there are native American societies which clearly do not share rules about, e.g. silence or questioning behavior with the dominant white majority. On the other hand, there are dangers in suggesting that each sub—culture constitutes a separate system. Intellectual entropy lies down such a route—since it is always possible to find differences and we could well end up finding individual families or even dyads in the final Chinese box; families do indeed have special rules which they always follow and which are not followed by all other families.

While I have not yet done the thinking necessary to resolve this problem, I think that the solution may be found in the notion of integrated 'sets' of rules and that some variant of componential analysis may provide us with the necessary technological apparatus for specification of such sets. In other words, we must find clusters of rules
which are shared by sets of members such that those sets are coterminous. This would mean that English rules for the reflexive and for copula deletion or reduction would be part of a set of categorical rules, but that the special additional rules for the copula found in NNS would not be part of the set of categorical rules for the English system. If any reader can help me with this problem I'll be most grateful.

The second problem is almost paradoxical. This is, that while the finding of an exception to a universal rule means that the rule must be discarded or the deviant case subsumed under some more abstract rule (exceptions are improbable in the case of most metatheoretical rules by their very definition); in the case of categorical rules exceptions must be found in some system. I have found it very difficult to identify categorical rules in our social system, simply because those rules which hold for our system seem likely to hold for all other systems as well, therefore being substantive universals. (Some social scientists have simply denied that intrasystemic categoricals exist for social systems.) This is true even for non banal rules. I will note specific instances of this problem when I discuss my illustrative cases. What is needed here is some method for finding cases; it may be that I'll have to state rules as substantive universals and wait for someone to shoot them down with exceptions! I don't believe that the Human Relations Area File, as currently organized, provides a very useful mechanism for this task.

Semi-categorical rules. Semi-categorical rules are intrasystemic rules whose violation is reportable, interesting, and interpretable. Semi-categorical rules seem to be both easier to identify than categorical rules, quite possibly because the exceptions are reportable and noticeable thereby calling attention to the rule; and more interesting, quite possibly because the need for interpretation leads the student in the direction of new understandings of the system under consideration. Moreover, since the content of these rules is system specific, I have found it easier to find different rules of this type for different systems.

Variable rules. Variable rules are clearer for sociological and sociolinguistic than for linguistic systems, probably because the search for concomitant variation has been a long tradition in sociology and the social sciences while similar activity has only recently been initiated in linguistics (see, however, Labov 1969b). As in the case of semi-categorical rules, it may well be that similar correlations obtain for systems across linguistic or social boundaries; specific variables and correlations will differ.

Free variation, such as some phonological differentiation (within ranges), is simply not very interesting. Cases which do seem interesting will probably turn out to be instances of variable rules.

At this time we do not have the data necessary, either in linguistics or in sociology, for answering the question as to what form universal
grammars (as contrasted to universal rules) will take. At this juncture we are probably better advised to direct our efforts to attaining the somewhat less ambitious goal of enlarging our conceptions of social and linguistic interaction as rule-governed behavior. I read this to imply, for the present, that we should focus attention primarily on the identification of categorical (and other intrasystemic) rules for specific linguistic, sociological, and sociolinguistic systems. Labov has suggested that in the case of linguistic rules this will require incorporation of inherent variation as a part of linguistic structure; in the case of social interactional rules it will require incorporation of invariant relations and processes as a part of social structure.

Labov has asserted (1968c) that in the case of linguistic knowledge this entails collection and analysis of four aspects of speakers' knowledge (competence) which have not traditionally been considered as within the scope of linguistic analysis: (1) variable inputs to rules (viz. frequencies with which particular rules are selected to express given meanings), (2) variable constraints on rules (effects of syntactic and phonological environments on those frequencies), (3) categorical features within variable rules (the presence of features which make optional rules obligatory), and (4) formal representation of social and stylistic constraints upon rules. Each of these aspects has a counterpart in contemporary sociological work; in some instances sociologists have long attended to the problem identified, in others we have not made much more progress than linguists.

Universal and intrasystemic rules in linguistics, sociological, and sociolinguistic systems: Some illustrative examples. Charts 2-4 contain illustrative examples of rules of different types for linguistic, sociological, and sociolinguistic systems, respectively. The charts are meant to be suggestive and evocative, not definitive. I am interested in generating possible taxonomies of rules, not in unimpeachable identification or assignment of rules within levels, types, or systems. I have included rules of different levels of generality and comparability and I am aware that fuller familiarity with empirical data on linguistic, social, and sociolinguistic systems may necessitate the deletion of some rules and, e.g. the movement of some rules from the categorical to the universal level. While I am not interested in arguing about specific cases or assignments, I am interested in obtaining suggestions for corrections. I hope that the charts will be seen as providing suggestive evidence of the usefulness of this taxonomic approach as a possible route to the specification of a unified theory of sociolinguistic description and perhaps, more ambitiously, as one kind of beginning in the direction of a unified theory of human behavior. I have no illusions, however, about having attained Nirvana.

In order to underline the parallels among the three systems under consideration, I have used the same wording for the metatheoretical
**CHART 2. Linguistic system**

<table>
<thead>
<tr>
<th>Level and type of rule</th>
<th>Example(s)</th>
<th>Violation(s)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Extrasystematic universals</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Metatheoretical</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Order, hierarchy[^a](x→a, b; x→a*b)</td>
<td>Minimal units arranged in order; resulting constituents also ordered. (Order can be structural or temporal)</td>
<td>*Random ordering of words.</td>
</tr>
<tr>
<td>Componential (Minimal units)</td>
<td>Categories (Nouns, Verbs)</td>
<td>*S → V</td>
</tr>
<tr>
<td>Relational (Functional)</td>
<td>Syntactic and semantic relations among components (e.g. subj., obj.) Interrogatives; statements</td>
<td>*Random semantic assignment</td>
</tr>
<tr>
<td>Absolute</td>
<td>Lexical and phonological marking</td>
<td>*Lexical and phonological marking → *</td>
</tr>
<tr>
<td>Quantitative</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Intrasystemic</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Categorical (Invariant) [Violators nonmembers]</td>
<td>Copula deletion (reduction) Reflexive</td>
<td>*I'm smarter than he's. *Go wash you.</td>
</tr>
<tr>
<td>Semi-Categorical [Exceptions reportable and interesting]</td>
<td>Selectional agreement. No geminate consonants within word boundaries.</td>
<td>The soul has bandaged moments. ? [ditto]</td>
</tr>
<tr>
<td>Variable (Concomitant Var. Correlation)</td>
<td>Release of final consonants. Geminate consonants across word boundaries.</td>
<td></td>
</tr>
<tr>
<td>Free Variation</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

**NOTES (Charts 2-4)**

a. Question mark refers to recent questioning of viability of assertion on ordering of minimal units.

b. *E.g. social differentiation or the life cycle.*

c. 'Interpretive procedures' as defined by Aaron Cicourel might be a better analog to syntactic-semantic relations among components. At this time I'm not sure exactly where interpretive procedures belong in the taxonomy; I'm sure they belong somewhere. It may be that there are different kinds of relational rules; it may be that those I have suggested are not viable.


e. Hymes 1972a (see also 1967)
### Chart 3. Sociological system

<table>
<thead>
<tr>
<th>Level and type of rule</th>
<th>Example(s)</th>
<th>Violation(s)</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Extrasystematic universals</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Metatheoretical</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Order, hierarchy ((x \rightarrow a, b; x \rightarrow a^\ast b))</td>
<td>?Minimal units arranged in order; resulting constituents also ordered. (Order can be structural or temporal)</td>
<td>*Random allocation of roles.</td>
</tr>
<tr>
<td></td>
<td></td>
<td>*Random ordering of events.</td>
</tr>
<tr>
<td>Componential (Minimal units)</td>
<td>Categories (Members; roles; norms)</td>
<td>*Group → Roles; Norms, (Members → ø)</td>
</tr>
<tr>
<td>Relational(^c) (Functional)</td>
<td>Social relations among categories ((Superordination-subordination; Cooperation). Leaders; followers.)</td>
<td>*Full equality in non-biologically determined social relationships.</td>
</tr>
<tr>
<td><strong>Substantive (Empirical)</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Absolute</td>
<td>Public acceptance, normative definitions, and sanctions do not covary simultaneously. ((Restrictions on base rules ?))</td>
<td>*All deviants defined as non members (viz. as violators of categorical rules).</td>
</tr>
<tr>
<td>Quantitative</td>
<td>Age marking on social roles.</td>
<td>*Age marking → ø ((E.g. teachers, students, husbands, wives randomly older or younger.))</td>
</tr>
<tr>
<td><strong>Intrasystemic</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Categorical (Invariant) ([Violators nonmembers])</td>
<td>Privacy rule, ‘A man’s home is his castle.’ Uninvited residential entry requires pre-negotiation or post-justification.</td>
<td>*Adult (non-thief) stranger enters closed house uninvited and sits down with family.</td>
</tr>
<tr>
<td>Variable (Concomitant Var. Correlation)</td>
<td>Socioeconomic status and a, b, c...n.</td>
<td></td>
</tr>
<tr>
<td>Free Variation</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

\(^f\). There are, of course, co-occurrence restrictions in all sociolinguistic systems. The claim here is that types and contexts of such restrictions are system specific. The violation specified here may be a bad example. On
<table>
<thead>
<tr>
<th>Level and type of rule</th>
<th>Example(s)</th>
<th>Violation(s)</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Extrasystemic universals</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Metatheoretical</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Order, hierarchy (x→a, b; x→a→b)</td>
<td>?Minimal units arranged in order; resulting constituents also ordered. (Order can be structural or temporal)</td>
<td>*Components of Speaking independent. *Random ordering of Q. A.</td>
</tr>
<tr>
<td><strong>Componential (Minimal units)</strong></td>
<td>Categories (Speaking)</td>
<td>*Speech event → with any element of Speaking Ø.</td>
</tr>
<tr>
<td><strong>Relational (Functional)</strong></td>
<td>Genre</td>
<td>*No stylistic differentiation. *Unclosed conversations.</td>
</tr>
<tr>
<td><strong>Substantive (Empirical)</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Absolute</strong></td>
<td>Telephone calls (and other non face-to-face summonses) require intentionality. [Will need to incorporate agency and object?]</td>
<td>*'Hi', 'Hi' as complete telephone conversation.</td>
</tr>
<tr>
<td><strong>Intrasystemic</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Categorical (Invariant) [Violators nonmembers]</strong></td>
<td>No self-identification in seeking directions from strangers.</td>
<td><em>'Hello, my name is Dell, can you please tell me how to get to the University?</em></td>
</tr>
<tr>
<td><strong>Semi-Categorical [Exceptions reportable and interesting]</strong></td>
<td>Co-occurrence restrictions.</td>
<td>*Professor says 'crap' on television. <em>Answers phone by asking 'Is Charles there?</em></td>
</tr>
<tr>
<td><strong>Variable (Concomitant Var. Correlation)</strong></td>
<td>Repertoire selection.</td>
<td></td>
</tr>
<tr>
<td><strong>Free Variation</strong></td>
<td>Hypercorrection.</td>
<td></td>
</tr>
</tbody>
</table>

co-occurrence restrictions, see Gumperz 1964.

g. Schegloff 1968.
i. Labov 1966a.
universals of order and hierarchy in each of the three system sets. The symbolic notation is intended to represent attributes of both dominance (hierarchy) and temporal ordering. The introduction of the concatenation symbol is to show that $x$ includes both $a$ and $b$ and that there are instances in which $x$ includes $a$ and $b$ and $a$ precedes $b$. By saying that order can be both temporal and structural I intend to communicate the fact that there are both instances: (1) in which different elements of a whole do not always precede one another in time in the same sequence but that the whole is not complete without all its elements (as is the case, e.g. with social differentiation in groups) and (2) where elements or stages precede and follow one another (e.g. the life cycle or certain cyclical transformations). It is simply not possible to think of violations of rules on this level; they don't occur.

The minimal units I have listed for each of the systems are not, of course, the smallest elements into which linguistic, social, and sociolinguistic behavior can be decomposed. They are, however, the smallest units which retain organized meaningfulness and individual identity after they have been combined into the next higher order of structure. Thus, nouns and verbs are parts of sentences, and recognizably so, just as roles and members are parts of groups and setting and participants are constituents of speech acts and events. They are also minimal units in the sense that without their presence the next higher order unit cannot exist. There is no sentence without a noun; no group without members; no speech act with any element of Hymes' (1972) Speaking paradigm absent.

Beginning with relational or functional universals I encountered difficulty in identifying rules for the three systems which closely paralleled each other. In each case there are, it is true, functions which can be specified, in the illustrative cases the designated functions have the added elegance of belonging to complementary pairs, e.g. interrogatives-statements, leaders-followers, openings-closings. There is, in addition, some affinity between the notion of syntactic and semantic relations among components such as the subject-object relationship and that of social relations among categories such as that of superordination-subordination. I am not sure exactly what that surface similarity means in terms of organizational similarity of the systems themselves. Nor, have I been able to identify an analog to the first two sets of relations in the sociolinguistic system. Genre is not functionally equivalent to subject-object or superordinate-subordinate relations. I suspect, however, that the failure to find functionally equivalent relationships results not from the absence of those relationships in the sociolinguistic system but rather from my inability to bend my mind in the right way. It may be that the sociolinguistic relations we want to identify will be found through closer scrutiny of the interrelations of elements of the Speaking model; it may be that they will be found through a fuller analysis of such
functional components as openings and closings—with the addition of
categories such as interruptions, elaborations, continuities, ellipses,
misfires—or, more broadly, sequential markers (on such components
see, e.g. Churchill’s n.d.). Again, I welcome suggestions.

Since there is an explicit expectation that substantive universals will,
with richer data resources and/or more sophisticated theoretical form-
ulations, move up in the charts to statuses as metatheoretical rules, it
is not surprising to find that they show little similarity across the three
systems in either form or content. It is not clear to me how the empi-
rical observation, which holds in all societies, that public acceptance,
native definitions, and sanctions do not covary simultaneously, can be
seen as a restriction on a base rule. It may be that the sociolinguistic
example, with its included notions of agency and object, may be closer
to the kind of claim made with regard to the linguistic system.

I think that the similarity of quantitative substantive universals
across the three systems may possibly indicate that we should look
for a metatheoretical universal having to do with distribution—such
a rule would probably be on a level of abstraction equivalent to that
of rules of order and hierarchy. Up to this time, however, I have not
found any exposition of such a distributional rule—as other than an
empirical datum—which persuasively argues a logic of why such rules
must be a part of every system.

I am happier with some of the intrasystemic rules I have suggested
than with others. I suspect, although I do not know enough about dif-
ferent languages, societies, or sociolinguistic systems, that some of
the rules I have suggested are ‘categories’ of rules which may appear
in all societies, but with content specific to specific societies. Others
of the rules I have suggested, however, are of sufficient specificity
that they clearly relate to kinds of grammatical or social or sociolin-
guistic phenomena which do not occur in all societies. Thus, with
regard to intrasystemic rules within the linguistic system of English
I suspect, although I don’t know, that there are other languages where
rules for the reflexive are quite different than in English and where
there are different expectations for the handling of geminates within
word boundaries. On the other hand, there are probably semi-
categorical rules of selectional agreement in all grammars; they will
vary in content and with reference to the categories which are restricted.

My problem with intrasystemic rules for American society was
somewhat different (keeping in mind the earlier discussion of the
problem of system boundaries). I had difficulty in finding a catagori-
cal rule for our society which simultaneously operated for all com-
ponent members of the society (or even for a restricted segment such
as the white middle class) which did not also hold in other societies as
well. The example I have given is not completely satisfactory even
though I have been assured that in Samoa adult (non-thief) strangers
can enter houses uninvited and sit down without postjustification. I am hopeful that I can find clues to categorical rules for our society in Goffman's work or in the work of the ethnomethodologists (see e.g. Cicourel 1970, Goffman 1971, Schegloff 1968).

I do think that the semi-categorical rules I have stated are sound. While status consistency and categorical endogamy may obtain in most societies, there are societies in which certain patterns of status inconsistency or of categorical exogamy are routine. Thus, high status caste Hindus become mendicants and hermits; this is a religiously sanctioned behavior which occurs toward the end of the life cycle. Thus again, once more in India, there are institutionalized patterns of hypergamy.

Finally, I'm moderately happy about the intrasystemic rules I've listed for our American sociolinguistic system, again with all the necessary cautions about systemic boundaries. With regard to the categorical rule, it is true that someone might identify himself as 'new in town' in asking for directions; this is justification rather than identification. It is also true that someone at a professional meeting might say, 'I'm Hugh Bird, can you tell me how to get where the plenary session is being held?' In such a case, of course, it would be expected that those to whom the request was addressed would not be strangers to the name and the self-identification would again be interpreted as justification. There are, of course, co-occurrence restrictions and sequencing rules in other sociolinguistic systems; my expectation is that different prescriptions and prohibitions would operate in different speech communities.

I have spent little time in elaborating or defending the rules which I have offered as illustrations. There are two reasons for this. One is that I have already used up my allotted space. The other, already alluded to, is that the entries in the three charts are intended to be evocative rather than definitive. I have already changed some cell entries; consensus on assignment of cases is not my goal. That goal is, rather, to demonstrate similarities in the rule-governed character of the three systems and the possibilities of starting from this perspective in the discovery of a unified theory of sociolinguistic description and even, perhaps, as a first step toward a unified theory of human behavior.

I will welcome comments and suggestions. In the meantime, I hope to encourage colleagues in psycholinguistics and in the natural sciences dealing with the biological ordering of human life to start thinking about similar sets of rules in phenomena they study.

NOTES

*Among those who have helped me learn about language in society, John Gumperz, Dell Hymes, and William Labov have had a particularly
strong influence. In this paper I have used Labov's work on intrasystemic rules as the base for a principal part of my argument. I could not have written the paper without the prior opportunity for close work with these scholars; I fear that sometimes I have absorbed their ideas without realizing where I've learned them. I suppose I could claim, in a parallel fashion, that they are also responsible for errors in interpretation or exposition; I fear here that I've simply failed to absorb their warnings and corrections. At Indiana University I have enjoyed the opportunity of numerous helpful sessions with Charles Bird and Austin Turk—who have argued at length and with persuasive vigor over entries in Charts 2-4. In the last analysis, however, I suppose I must take the responsibility for heresies and other errors.

I am grateful for modest support from the Graduate School of Indiana University in the form of a Faculty Research Grant.

1To say that behavior is rule governed can mean several different things. It can mean that there are statistical regularities in behavior which can be captured in ‘if... then’ propositions. It can mean that there are normative implications; that rules say how men ‘ought’ to behave. It can mean that ‘natively competent’ members of a social group don’t behave randomly; that their behavior is derived from shared understandings which simultaneously govern their own behavior and their interpretations of the behavior of other members. In this paper I use the term in the last of these senses. For a fuller explication and sources, see Grimshaw 1972a.

2For an introduction to some of these issues, see Chomsky 1968. For a perspective emphasizing the similarity of linguistic universals to logical systems, see Bach 1968.

3I first heard the suggestion that linguists were beginning to study variation and sociologists to look for invariant behavior made by William Labov in a presentation to the Social Science Research Council Committee on Sociolinguistics (Labov 1968c). Labov has continued to develop this perspective in a number of publications; especially in his reports on the Harlem studies (Labov et al. 1968), in his work on variable rules themselves (especially 1969b) and in his writing on linguistic ‘systems’ (e.g. 1972a). Readers familiar with Labov’s work will see that I have incorporated this perspective in my discussion (infra) of intrasystemic rules.

4The first time I met Dell Hymes (at a session on the sociology of language at a regional sociology meeting in the early Sixties) I remarked on the ‘linguistic universal’ that more negative than positive personal epithets seem to appear in every language (Grimshaw 1967). Hymes corrected me and gave me my first introduction to the linguist’s notion of universal. In the context of this paper I now suspect that I had identified a quantitative substantive sociolinguistic universal.

5All languages exemplify the same basic organizational scheme. More specifically, every human language comprises an infinite number
of sentences, each of which manifests, in phonetic form, a conceptual structure. A complex series of syntactic rules serves to connect conceptual structures with surface structures, which are linear strings of lexical items grouped hierarchically. A series of phonological rules connects the surface structure of each sentence with its phonetic manifestation on the basis of the underlying phonological representations of its lexical items. Each individual lexical item consists in the association of semantic, syntactic, and phonological properties, the relation between its semantic and phonological properties being arbitrary in most cases. Phonologically, a lexical item is represented as a linear series of segments, each segment specified with respect to distinctive phonological information.

'There are simply no exceptions to this organizational scheme. No one has ever found a human language lacking syntactic rules, phonological rules, or discrete lexical items. No one has found a language in which lexical items were not composed of linear sequences of sound segments. Out of all conceivable ways in which a language could be put together, actual human languages are unanimous in picking this particular way. Linguistic systems differ somewhat in structure, but they vary only within the confines of this common framework.'

6While directed to some of the same classificatory problems, my metatheoretical and substantive are not equivalent to Chomsky's (1965:27-30) formal and substantive.

7It may be true that if I pronounce a morpheme 10,000 times I will never reproduce a pronunciation identical to an earlier one. It is conceivable that the parameters governing this variation can be identified; it is unlikely that anyone will do the work required very soon. The variations simply aren't that interesting.

8I am indebted to Labov for his assistance in reconstructing the rich history tersely capsulized in the paragraphs immediately above.

9Labov has also attacked this problem, see his paper in this volume (1972b).

10That the 'homogeneous speech community' is found only in the Never-Never Land of 'ideal speaker-listeners' does not diminish the heuristic usefulness of the notion. A useful summary of the concept of speech community is Gumperz (1968, reprinted in 1971). Hymes has pointed out in a forthcoming paper (1972b) that in an important sense the boundaries of speech communities are socially defined by members' own self-identification.

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THE SCOPE OF SOCIOLINGUISTICS

DELL HYMES

University of Pennsylvania

Abstract. The term 'sociolinguistics' began to gain currency about ten years ago. The subsequent decade has been symposia, readings, textbooks, and a specialized journal. Where do we now stand? In a fundamental sense, we have gotten, I think, to the threshold. An adequate sociolinguistics would lose itself and its name in the standard practice of linguistics and ethnography. From the standpoint of linguistics, we have legitimized the social as well as the linguistic as a concern; have seen great strides in socially realistic linguistics; but have had only the first steps (though fundamental steps) toward a sociolinguistics that is not only a parallel concern or a methodological adjunct, but a new, more adequate way of describing and explaining the organization of linguistic features, hence a new conception of the tasks of linguistics proper. Some of the motives and themes of such a conception are noted, together with its possible place in the history of linguistics as seen from the year 2000.

1. State of sociolinguistics

The term 'sociolinguistics' began to gain currency about ten years ago. The subsequent decade has seen a great deal of activity. There have been general symposia (e.g. Bright 1966, Lieberson 1966, Istituto Luigi Sturzo 1970, Ardener 1971, Smith and Shuy 1972); symposia on major topics (e.g. Gumperz and Hymes 1964, Macnamara 1967, Fishman, Ferguson and Das Gupta 1968, Hymes 1971, Whiteley 1971, Rubin and Jernudd 1972, Cazden, John and Hymes 1972); notable major research efforts (e.g. Fishman 1966, Labov 1966, the several surveys of East African countries, Le Page's survey in British Honduras,
Labov's U.S. Regional Survey); the launching of working papers (e.g. Berkeley's Language and Behavior Laboratory series, and now the 'Penn-Texas' series out of Texas, and the Georgetown series); books of readings, increasingly specific to the field (e.g. Hymes 1964, Fishman 1968, Giglioli 1972, Gumperz and Hymes 1972, Fishman (in press)); textbooks (e.g. Burling 1969, Pride 1970, Fishman 1970, Fishman 1972); even a series of collected papers of middle-aged men who find themselves senior scholars (Greenberg 1971, Ferguson 1971, Gumperz 1971, Haugen 1972, to be followed by Lambert), as well as Bernstein (1972); and specific journals, one more applied in orientation (La Mundo Lingvo-Problemo), one more theoretical (Language in Society).

Our present meeting is in a way a culmination of the decade's activity. Where do we stand? To what point have we gotten? In some ways, very far. In one fundamental regard, I think, simply to a threshold.

Energetic activity and prolific publication need not warrant confidence in the scientific worth of what is done. We are all familiar with the gap that can exist between public concerns and the competence of scientists. Sociolinguistics is nourished in important part by the obvious relevance of much of its subject-matter, joining other academic fields in which concern for education, children, ethnic relations, governmental policies, find expression. But the importance of the questions is no guarantee of the value of the answer. Indeed, one can wonder if research funds are not implicitly ways of finding employment for sons and daughters of the middle class, while avoiding confrontation of the real problems; perhaps what is needed is not research but substantial doses of money, love, and democratic participation. In a society which expects to organize bureaucracies and to retain scholars to minister to whatever is defined as a national need, sociolinguistics might drift indefinitely, profuse and shallow, 'a mile wide and an inch deep'.

I take it that most of us aim higher than that. We see a scientific as well as a practical need. If relevance to social problems were not recognized, sociolinguistics research would still be needed for the sake of an adequate theory of language. Some of what is done under the heading of 'sociolinguistics' may be justified only in the sense that something is better than nothing, when need is great. But in the present state of sociolinguistics, I would maintain three things: (1) the scientific as well as the practical side of linguistics stands in need; (2) the scientific and practical needs converge; and (3) the past decade has seen steps taken which do bring us to the threshold of an integrated approach to linguistic description. As to (1), witness the current disarray with regard to arguments in syntax and semantics, and with regard to the place of semantics, intonation, and indeed phonology and lexicon in a model of grammar itself, as issues of empirical adequacy and validity are pressed against the dominant 'institutionist' approach, and as other, contextually-oriented traditions of work are gradually reinvented or
grudgingly rediscovered. As to (2), note that findings as to the organization of variation and the structure of speech acts, both issues central to linguistic theory, contribute to the scientific basis of which successful practice stands in need, while patent facts of practical experience (e.g. the organization of linguistic features in terms of verbal repertoires, the role of social meaning as a determinant of acceptability and the 'creative aspect of language use', the effects of personal identity, role, and setting as constraints on competence) point to severe limitations of present linguistic theory and motivate efforts to overcome them. As to (3), if we take 'integrated' to encompass the structure of sentences within the structure of discourse, of referential meaning within the meanings of speech acts, and of dialects and languages within the organization of verbal repertoires and speech communities, then we can both see a convergence implicit in much of the best recent work and envisage a unity to which it can arrive.

To explain this view of the state of sociolinguistic research, I must say something about its goals.

2. Goals of sociolinguistics

The term 'sociolinguistics' means many things to many people, and of course no one has a patent on its definition. Indeed not everyone whose work is called 'sociolinguistic' is ready to accept the label, and those who do use the term include and emphasize different things. Nevertheless, three main orientations can be singled out, orientations that can be labelled: (a) the social as well as the linguistic; (b) socially realistic linguistics; (3) socially constituted linguistics. Let me characterize each of these orientations in relation to linguistic theory.

(a) The social as well as the linguistic. Here may be placed ventures into social problems involving language and the use of language, which are not seen as involving a challenge to existing linguistics. American linguistics does have a tradition of practical concerns—e.g. one can mention Sapir's semantic research for an international auxiliary language, Bloomfield's work in the teaching of reading, Swadesh's literacy work, the 'Army method' of teaching foreign languages. The salient examples today involve American cities and developing nations, and concern problems of education, minority groups, and language policies. For the most part this work is conceived as an application, lacking theoretical goals, or else as pursuing theoretical goals that are in addition to those of normal linguistics, or perhaps even wholly unrelated to them. When 'sociolinguistics' serves as a legitimizing label for such activity, it is, as said, not conceived as a challenge to normal linguistics; linguists who perceive such a challenge in the label tend to eschew it.

(b) Socially realistic linguistics. This term is apt2 for work that extends and challenges existing linguistics with data from the speech
community. The challenge, and indeed the accomplishment, might be summed up in the two words, 'variation' and 'validity'. A salient example is the work of William Labov, whose orientation toward linguistics is represented in such papers as 'The study of language and its social context' (1970) and 'Methodology' (1971). The expressed theoretical goals are not distinct from those of normal linguistics, e.g. the nature of linguistic rules, the nature of sound change, but the method of work, and the findings, differ sharply. Here might also be put work which recognizes dependence of the analysis of meaning and speech acts on social context (e.g. R. Lakoff, mss.).

Both of these orientations are thriving, here at this meeting and elsewhere. Less developed, but representing, I think, the fundamental challenge to whose threshold we have come, is socially constituted linguistics.

(c) Socially constituted linguistics. The phrase 'socially constituted' is intended to express the view that social function gives form to the ways in which linguistic features are encountered in actual life. This being so, an adequate approach must begin by identifying social functions, and discover the ways in which linguistic features are selected and grouped together to serve them. Such a point of view cannot leave normal linguistic theory unchallenged as does the first orientation, nor limit its challenge to reform, because its own goals are not allowed for by normal theory, and cannot be achieved by 'working within the system'. A 'socially constituted' linguistics shares the practical concerns of other orientations; it shares concern for social realism and validity; but even if it could wait for the perfection of a 'linguistic theory' of the normal sort, it could not then use it. Many of the features and relationships with which it must deal would never have been taken up in a 'theory' of the normal sort. (That is why, indeed, 'linguistic theory' of the normal sort is not a 'theory of language', but only a theory of grammar.) A 'socially constituted' linguistics is concerned with 'social' as well as referential meaning, and with language as part of communicative conduct and social action. Its task is the thoroughgoing critique of received notions and practices, from the standpoint of social meaning, that is, from a functional perspective. Such a conception reverses the structuralist tendency of most of the twentieth century, toward the isolation of referential structure, and the posing of questions about social functions from that standpoint. The goals of social relevance and social realism can indeed be fully accomplished only from the standpoint of the new conception, for much of what must be taken into account, much of what is there, organized and used, in actual speech, can only be seen, let alone understood, when one starts from function and looks for the structure that serves it.

I have given examples to support this thesis in other papers (e.g. 1964a, 1970b, 1971a) and can only hurry past them now. Let me
merely mention that from a comprehensive functional standpoint, a phonetic feature such as aspiration appears a true phonological universal, specialized to referential function in some languages, and to stylistic function in others (hence not of indifference to general theory in its role in English, as Chomsky and Halle (1968:viii) would have it); recognition of a social-identifying function motivates an independently controllable articulation otherwise left unintelligible (Chomsky and Halle 1968:298; cf. Hymes 1970b:134-5); the status of a sentence as a speech act depends upon the rights and obligations, roles and statuses, of the participants; unless one extends the rules governing a verbal summons in English to include non-verbal acts (a knock, a telephone ring), a significant generalization is lost (Schegloff 1968); similarly, the function of deixis in San Blas Cuna is served by a set of forms that includes lip-pointing (Sherzer 1973); speech probably serves to mark sex-role status in every community, but linguists have hitherto discovered it only when intrusive in a normal grammatical description; some consistent ways of speaking make use of the resources of more than one language (e.g. the Dutch of Surinam blacks, which should be grammatically and lexically standard, and phonologically creole (Eersel 1971)); in some communities distinct languages can be described as lexically distinct with a common grammar and phonology (Kupwar approaches this, according to Gumperz and Wilson (1971)); the semantic structure represented by a choice of pronoun in one community may be expressed by a choice of dialect in another, and choice of language in still a third, so that analysis of the function from a universal standpoint cannot stay with one part of language, or even within the category, language. In sum, and I apologize for this rapid review of instances, if our concern is social relevance and social realism, we must recognize that there is more to the relationship between sound and meaning than is dreamt of in normal linguistic theory. In sound there are stylistic as well as referential features and contrasts; in meaning there is social as well as referential import; in between there are relationships not given in ordinary grammar but there for the finding in social life.

From this standpoint, what there is to be described and accounted for is not in the first instance a language (say, English), but means of speech, and, inseparably, their meanings for those who use them. The set of conventional resources available to a competent member of a community can be so described. As we have seen, this set of resources is more extensive than a single norm, grammar, or language; nor can the nature of its organization be given in those terms. Yet it is this set of resources with which one must deal, if 'linguistic theory' is to become synonymous with 'theory of language'.

It is not that phenomena pointing to a more general conception of the relationship between sound and meaning have not long been noted, and often enough studied with insight and care. Expressive language, speech levels, social dialects, registers, functional varieties, code- and style-
switching, are familiar and essential concepts; the interlocked subjects of stylistics, poetics, and rhetoric have flourished in recent years. Anything that can be accomplished in theory and method for a socially constituted linguistics must incorporate and build on that work, which has done much to shape what I say here. But the tendency has been to treat such phenomena and such studies as marginal or as supplementary to grammar (cf. Hymes 1970b:117-120). (Certainly that has been the tendency of grammarians.) The hegemony of grammar as a genre, and of the referential function as its organizing basis, has been preserved. Whereas the essence of a functional approach is not to take function for granted, but as problematic; to assume as part of a universal theory of language that a plurality of functions are served by linguistic features in any act and community; to require validation of the relationships between features and functions, and of their organization into varieties, registers, ways of speaking, ethnographically within the community; and to take functional questions, a functional perspective, as having priority, that is, as being fundamental, both in general theory and in specific accounts, to whatever can be validly said as to structure, competence, universals, etc. (cf. Hymes 1964).

Such a perspective was present in the structuralism of the period before the Second World War (cf. Jakobson 1963, Firth 1935, and the quotations in (5) below), and has never been wholly lost. In Anglo-American circles it has begun to come to the fore in work under the aegis of sociolinguistics in recent years. Salient examples include the work of Labov (1966, 1970, section 3) on 'sociolinguistic structure', of Gumperz (1964) on verbal repertoire, of Bernstein on codes (see now Bernstein 1972), of Fishman on domains (1966), of Denison (1970) and Le Page (1969) on multilingualism, and of Ervin-Tripp (1972) on sociolinguistic rules. What is important here is the element in each work that contributes to a general methodological perspective. Such work goes beyond the recognition and analysis of particular cases to suggest a mode of organization of linguistic features other than that of a grammar. The common implication, which I want to draw, emphasize, and elaborate, is, in its weaker form, that such alternative modes of organization exist; and, in its stronger form, that one or more such alternative modes of organization may be fundamental.

There is a second point, linked to the first, and owing its full recognition to much the same body of work: a conception of the speech community not in terms of language alone (especially not just one language, and a fortiori, not just one homogeneous language).

Although they would find the wording odd, many linguists might accept a definition of the object of linguistic description as: the organization of features within a community. From the present standpoint, the wording is not odd, but vital. The two points just stated in negative terms can now be put positively.
(1) The organization of linguistic features within a speech community is in terms of ways of speaking within a verbal repertoire.

(2) Membership in a speech community consists in sharing one (or more) ways of speaking.

From this standpoint, the usual linguistic description identifies a part (not the whole) of the linguistic features, resources, verbal means, of a community, and says little or nothing about their actual organization. Grammar indeed originated as a pedagogical and literary genre, and has been revitalized as a logical one; neither its traditional nor its mathematical pedigree is much warrant for taking it for granted that it is the form in which speech comes organized in use. Psychologists and psycholinguists have recently discovered and begun to build on recognition of this fact, with regard to the organization of language for production, reception, and acquisition. Those of us interested in the existence of social facts and customary behavior must build on it too. Classical antiquity did not stop with grammar, but went on to rhetoric (cf. Marrou (1965) 1964, Part Two, Chs. VII, X, and Part Three, Chs. V, VI). So should we, without the normative, exclusionary bias that has dogged the genre of grammar throughout its history, in a thorough-going, reconstructing way.

What is the nature of such a reconstruction, of a method of description adequate to the goals of a socially constituted linguistics, as just stated? Briefly and broadly put, 'the task is to identify and analyze the ways of speaking in a community, together with the conditions and meanings of their use. In sociolinguistic description, the first application of the commutation test is to ways of speaking' (Hymes 1970b:117-8). In what way is a person speaking? What is the set of such ways? And the contrastive as well as identificational meaning of each? Within ways of speaking, commutation will further discover two mutually implicated modes of meaning, the 'referential' and 'social'. There is the systemic invariance in terms of which two utterances of 'fourth floor' are repetitions, the same utterance, and there is the contrast in virtue of which they may be different (see Labov 1966 on style-shifting in New York City department stores with respect to 'repetitions' of utterances with post-vocalic reconstriction). Conversely, there is the contrast by which utterances of 'third floor' and 'fourth floor' may differ in what they convey (as to location), and there is the systemic invariance in terms of which they are repetitions, conveying the same meaning (as to social position and speech community identification). It is not obvious, is it, after all, that the energies of linguistics should be devoted entirely to the signals that tell where things can be bought in department stores, and not at all to the signals that tell where the people in department stores have come from, are now, and aspire to be?

The principle of mutually implicated modes of meaning holds for underlying relationships as well as overt utterances--sentences the
same in ‘referential’ basic structures may be contrastive in social/stylistic meaning, and conversely, as any reflective writer has occasion to know. In sum, the often stated foundation of linguistic theory, that in a speech community some utterances are the same, differing only in ‘free’ variation, that the goal of theoretical explanation is to account for what counts as contrast, what does not, has perhaps served the development of linguistics well in its purely ‘referential’ interpretation. One bird of function in the hand, so to speak, may have been preferable to entering the bush to cope with two. But, to elaborate the figure, it appears that neither bird will fly without the other, even that neither is itself a whole bird. To pursue the figure no doubt too far, the bird in the hand proves to be a featherless monopter, to be restored only out of the ashes of conventional grammar. The true foundation of theory and method is that in a speech community some ways of speaking are the same, that some of the persons talk the same way.

A community, then, is to be characterized in terms of a repertoire of ways of speaking. Ways of speaking are to be characterized in terms of a relationship between styles, on the one hand, and contexts of discourse, on the other. The formal concept underlying speech styles is what Ervin-Tripp (1972) has called rules of co-occurrence. The formal concept of relating speech styles to contexts of discourse is called by her rules of alternation. The speech styles defined by rules of co-occurrence draw on the linguistic varieties present in a community, from whose resources they select and group features in sometimes complex ways. The relationships dubbed ‘rules of alternation’ are in the first instance considerations of appropriateness, and of marked and unmarked usage.

Ervin-Tripp’s delineation of these two concepts is a culmination of the quest in linguistics throughout this century for adequate descriptive concepts, concepts that would be formal and universal to all languages, yet concretely valid in application to each. This history can be traced in American linguistics from discussion early in the century as to grammatical categories (e.g. incorporation, compounding, inflection) by Boas, Sapir, and Kroeber, through the generalization of the terms ‘phoneme’, ‘morpheme’, and ‘distinctive features’, to the discovery of transformational relationships. The notion of speech-style, as a mode of organization of features cutting across the standard sectors of phonology, grammar, and lexicon, has indeed been advanced a number of times (Whorf 1942:92, Harris 1951:10, Joos 1959, Pike 1967:463-4), but not really generalized as a perspective for the analysis of a speech community, and always stopping short of the decisive step taken by Ervin-Tripp, which is to recognize speech-styles themselves as the elements of a further system of rules. The recognition of this fact is comparable in nature and importance to the recognition of transformations (as rules operating on rules). The study of the structure of
relationships among speech-styles opens up the possibility of a generative approach; and it makes the study of social meaning as embodied in roles, activities and situations integral to the explanation of the meanings of the speech styles themselves. What Friedrich (1972) had shown with regard to 'pronominal breakthrough', meaning emergent from the interaction of pronominal and contextual meanings, is here generalized as a methodology.

Many of you will be quick to note that linguistics does not itself command analysis of social role, activities, and situations. Of this, two things can be said. First, such analysis is necessary. There really is no way that linguistic theory can become a theory of language without encompassing social meaning, and that means becoming a part of the general study of communicative conduct and social action. Second, this step is dictated by the development of linguistics itself. Having begun its structural course at the far side of meaning, with a focus on phonology, linguistics has proceeded through successive foci on morphology, syntax, semantics, and now performative and speech acts. There is no way to analyze speech acts adequately without ethnography; no language is a perfect metalanguage for the acts that can be performed with it. The study of speech acts can indeed be a center of a socially constituted linguistics; but its own logic broaches the general study of the vocabulary of action, in communities and in social science. Again, if we take seriously Chomsky's implicit call for linguistics to concern itself with the 'creative aspect' of language use, and with the basis of the ability to generate novel, yet appropriate sentences, we again are forced into analysis of setting as well as syntax. For appropriateness is not a property of sentences, but of a relationship between sentences and contexts. A fortiori for the property of 'creativity', whether saying something new in a familiar setting, or something familiar in a setting that is new. At every turn, it almost would seem, linguistics is wrestling with phenomena, and concepts, that turn out to entail relationships, only one pole of which is within linguistics' usual domain. The true generalizations can never be captured except from a perspective that encompasses both poles.

One way to bring out this point is to say that a socially constituted linguistics has as a goal a kind of explanatory adequacy complementary to that proposed by Chomsky. Chomsky's type of explanatory adequacy leads away from speech, and from languages, to relationships possibly universal to all languages, and possibly inherent in human nature. It is an exciting and worthwhile prospect. The complementary type of explanatory adequacy leads from what is common to all human beings and all languages toward what particular communities and persons have made of their means of speech. It is comparative and evolutionary in a sociocultural, rather than a biological, sense. It sees as in need of explanation the differential elaboration of means of speech, and of
speech itself. At a surface level it notices gross contrasts in speech activity, from great volubility to great taciturnity; gross contrasts in elaboration of message-form; gross contrasts in the predominance of traditional and of spontaneously encoded utterance; gross contrasts in the complication, or simplification, of the obligatory surface structure of languages themselves. These contrasts, and the typologies to which they point, no doubt find their explanation at a deeper level. Rules of conduct in relation to roles and settings; the role of a language-variety in socialization or in boundary-maintenance; values, conceptions of the self, and beliefs as to the rights and duties one owes to others as fellow members of a community, all will be found to have a place. The general problem, then, is to identify the means of speech and ways of speaking of communities; to find, indeed, where are the real communities, for language boundaries do not give them, and a person or a group may belong to more than one—to characterize communities in terms of their repertoires of these; and through ethnography, comparative ethnology, historical, and evolutionary considerations, become able to explain something of the origin, development, maintenance, obsolescence, and loss of ways of speaking and types of speech community—of the face speech wears for human beings before they learn that it is language, a thing apart, and the property of linguists.  

This complementary goal of explanatory adequacy comes not, it must be admitted, from the internal logic of linguistics, but from an external aspiration. Chomsky's goal of explanatory adequacy, to be sure, would seem to owe much to his own concern to understand the human mind and to revitalize rationalist philosophy. He has made his concern an effective goal for many in linguistics, philosophy, and psychology (cf. Dingwall 1971). The concern that motivates explanation directed toward ways of speaking and speech communities may or may not find a similar response. This concern, put simply, is with human liberation. The goal of a second type of explanatory adequacy is necessary if linguistic research is to serve that human goal.

Consider the present stance of linguistics, as reflected perhaps in the response of many members of the LSA to suggestions of racial inferiority. Many linguists, like many anthropologists, believe that no group of human beings is innately incapable of the highest achievements of civilization. They have much reason so to believe. And they speak out. What they can speak to is the potential equality of all human groups. Neither their theory, nor their liberalism, quite prepares them to speak concretely to actual inequalities. Difference itself is offensive. The scientific equality of all languages was declared early in the century, and for most scientists, that continues to suffice. But means of speech are what their uses make of them; they have been put to different ends, in differing circumstances, and sometimes been caught up in the ends and circumstances of others. No minister of
education in a developing nation is of the view that anything can be said and read in any language. That it could be, were there time, money, and intent, does not speak to the actual situation. And quite apart from situations that suggest judgments of inferiority, it is simply the case that a certain language, or speech itself, has meant and been made to mean different things in different communities. The general observations on the functions of language, typically on the manifold marvelousness of language, found in texts are trash. We hardly know in any systematic way what communities have made of language.

If linguistic research is to help as it could in transcending the many inequalities in language and competence in the world today, it must be able to analyze these inequalities. In particular, a practical linguistics so motivated would have to go beyond means of speech and types of speech community to a concern with persons, and social structure. If competence is to mean anything useful (we do not really need a synonym for grammar), it must refer to the abilities actually held by persons. A salient fact about a speech community, realistically viewed, is the unequal distribution of abilities, on the one hand, and of opportunities for their use, on the other. This indeed appears to be an old story in mankind, and even a cursory look at the globe discloses definition of women as communicatively second-class citizens to be widespread. When, where, and what they may speak, the conceptions of themselves as speakers with which they are socialized, show again and again that from the community point of view, they at least are not ‘ideal speakers’, though they may on occasion be ideal hearers.

Beyond the structure of ways of speaking, then, is the question of explanation, and beyond that, the question of liberation. This is not a simple matter, and I cannot say much about it now except to observe that some seemingly attractive views have hidden pitfalls. Simply to overcome restrictions is not enough, for a community in which everyone could say anything might have no one listening. To overcome ‘restricted’ context-dependent, codes is not enough, for, as the German sociologist Habermas has been pointing out (and as Sapir did before him), human life needs some areas of symbolic interaction and communication in which much can be taken for granted. Simply defending ‘restricted’ codes is not enough, for the explicitness of the ‘elaborated’, context-independent code may be needed to analyze publicly and so transform the existing order. (See ‘A critique of compensatory education’ in Bernstein 1972).

The goal of explanatory adequacy with regard to speech communities as comprising ways of speaking, will, I suppose, be quite enough for most linguists to consider, let alone to accept. Yet, I believe, if linguistics is to realize its potential for the well-being of mankind, it must go even further, and consider speech communities as comprising not only rules, but also sometimes oppression, sometimes freedom, in the relation between personal abilities and their occasions of use.
3. Themes of sociolinguistics

If we associate ‘sociolinguistics’ with ‘socially constituted linguistics’, then the following might be a set of themes, or slogans, for a sociolinguistics of the scope just sketched. (If this were a political movement, or a Chinese banquet, we might put them on banners about the walls).

1. Linguistic theory as theory of language (not just of grammar).
2. Foundations of theory and methodology as involving questions of functions (not just of structures).
3. Function as empirically problematic (not taken for granted).
4. Function as plural, involving social meaning (not just referential meaning).
5. Function as empirically prior, as constituting structure (not as derivative of structure).
6. Speech as organized social occasion (not merely a physical utterance).
7. Speaking as a culturally patterned activity (not merely implementation of grammar).
8. Competence as personal ability (not merely grammatical knowledge, systemic potential, superorganic property of community, or, indeed, irrelevant in any other way).
9. Performance as accomplishment and responsibility (not merely psycholinguistic processing, let alone impediment).
10. Speech community as an organization of ways of speaking (verbal repertoire) (not merely equivalent to a language).
11. Speech community boundaries as defined by shared ways of speaking, (not merely by boundaries of a language).
12. Means of speech, ways of speaking, speech communities as partly social in definition and description (not merely linguistic).
13. Languages as what their uses have made of them (not merely what human nature has given).
14. Liberté, Egalité, Fraternité of speech as something achieved in social life (not merely postulated as a consequence of language).

4. The scope of sociolinguistics

What, then, is the scope of sociolinguistics? Not all I have just described, but rather, that part of it which linguists and social scientists leave unattended. The final goal of sociolinguistics, I think, must be to preside over its own liquidation. The flourishing of a hybrid term such as sociolinguistics reflects a gap in the disposition of established disciplines with respect to reality. Sometimes new disciplines do grow from such a state of affairs, but the recent history of the study of language has seen the disciplines adjacent to a gap themselves grow
to encompass it. Some can recall a generation ago when proper American linguists did not study meaning, and ethnographers had little linguistic method. A study of meaning in another language or culture (say, grammatical categories or kinship terms) could qualify as 'ethnolinguistic' then. Today, of course, semantics is actually pursued in both linguistics and ethnography, and a mediating interdisciplinary label is unnecessary; 'semantics' itself will usually suffice.

Let us hope for a similar history for 'sociolinguistics'. In one sense, the issue again is the study of meaning, only now, social meaning.\(^8\)

What are the chances for such a history to be written, say, from the vantage point of the year 2000 A.D.? To see, in retrospect, the flourishing of 'sociolinguistics' as a transitional stage in the transformation of linguistics and adjacent social sciences disciplines to encompass what I have called 'socially constituted linguistics'? The chances, I think, are quite uncertain.

Clearly recognition of the gap, and advocacy of a perspective to overcome it, are not enough. The future historian will notice that there were 'efforts towards a means-ends model of language' between the First and Second World Wars (Jakobson 1963). And in the literature of that period, he will find in the writings of another of the five or six great linguists of the century such statements as the following:

The true locus of culture is in the interaction of specific individuals and, on the subjective side, in the world of meaning which each one of these individuals may unconsciously abstract for himself from his participation in these interactions [1932] 1949:515);

for it is only through an analysis of variation that the reality and meaning of a norm can be established at all, and it is only through a minute and sympathetic study of individual behavior in the state in which normal human beings find themselves, namely in a state of society, that it will ultimately be possible to say things about society itself and culture that are more than fairly convenient abstractions ([1938] 1949:576);

It is not really difficult, then, to see why anyone brought up on the austerities of a well-defined science must, if he is to maintain his symbolic self-respect, become more and more estranged from man himself ([1939] 1949:580);

The very terminology which is used by the many kinds of segmental sciences of man indicates how remote man himself has become as a necessary concept in the methodology of the respective sciences... In linguistics, abstracted speech
sounds, words, and the arrangement of words have come to have so authentic a vitality that one can speak of 'regular sound change'...without knowing or caring who opened their mouths, at what time, to communicate what to whom (1939 1949:578, 579);

As we follow tangible problems of behavior rather than selected problems set by recognized disciplines, we discover the field of social psychology... (1939 1949:513);

The social psychology into which the conventional cultural and psychological disciplines must eventually be related is related to these paradigmatic studies as an investigation into living speech is related to grammar. I think few cultural disciplines are as exact, as rigorously configured, as self-contained as grammar, but if it is desired to have grammar contribute a significant share to our understanding of human behavior, its definitions, meanings, and classifications must be capable of a significant restatement in terms of a social psychology which...boldly essays to bring every cultural pattern back to the living context from which it has been abstracted in the first place...back to its social matrix (1934 1949:592-3, 592).

These quotations are from the writings of Sapir's last years, when he began to rethink the nature of language, culture, and society from a standpoint he sometimes called 'psychiatric', or 'social psychology', and which today we might more readily label the standpoint of social interaction, or communicative conduct; the standpoint, as I would see it, of sociolinguistics.

Obviously Sapir's intellectual lead did not prevail, after his death in 1939, although its influence can be traced in many quarters. Such a fact must humble expectation. A decade ago (when the introduction to my book of readings was written) I did venture to predict:

It may be that the development of these foci of interest (semantic description, sociolinguistic variation) will lead historians of twentieth-century linguistics to say that whereas the first half of the century was distinguished by a drive for the autonomy of language as an object of study and a focus upon description of structure, the second half was distinguished by a concern for the integration of language in sociocultural context and a focus upon the analysis of function. (1964:11)

Ten years later, we are, I think, only at a threshold. Whether we pass over and occupy the land will depend crucially upon the commitment
of those who have the essential skills, especially linguists. For a criterion of the field I envisage is that it is a linguistics, a functionally oriented, more adequate linguistics, that has at last realized itself as a social science. Perhaps in this respect there will be in the year 2000 A.D. three main branches of linguistic science: psychological, sociological (these two answering to the two directions of explanatory adequacy), and the traditional and indispensable work oriented toward specific languages, language families, and language areas. With regard to the sociological branch of the three, there are many reasons within theoretical linguistics today why it appears a necessary step. But holes in a scientific pattern, like those in a phonological one, may go long unfilled. Perhaps as much or more will depend on practical as on scientific concern. It may not have been accidental that it was the 1930s that saw Sapir's concern with personal meaning and social interaction. Perhaps socially concerned linguists in the coming decade will discover wisdom in Chairman Mao Tze-Tungs' remark (1952:7—if I may follow the example of our President in quoting him):

If you want to know a certain thing or a certain class of things directly, you must personally participate in the practical struggle to change reality, to change that thing or class of things, for only thus can you come into contact with them as phenomena; only through personal participation can you uncover the essence of that thing or class of things and comprehend them.

Certainly it is a sociolinguistic perspective, uniting theory and practice, of the sort on whose threshold we stand, that is most appropriate to a vision of the future of mankind as one in a world at peace. Comparative-historical linguistics, linguistics oriented toward individual languages and language families, can discover and maintain unities from the past, and indeed has had positive effect in that regard (Matthew Arnold pointed to the Indo-European unity of the English and Irish, Sir Henry Maine to that of England and India, as warrant for overcoming prejudice and recognizing brotherhood). Structural and psychologically oriented linguistics can point to a timeless unity of human nature. Linguistics as sociolinguistics, if it will, can envisage and work toward a unity that is yet to come.

NOTES

1 Fillmore's fine paper became known to me for the first time the morning of our session. To my paper, I prefaced spontaneous remarks to the following effect: if the audience saw me turning rapidly past certain pages in my paper, it would be because things I had thought I should
say had already been said so much better by Fillmore. In his paper Fillmore referred to 'incarnation', and to be sure, at some points during his talk I had a feeling of witnessing something like reincarnation. Nevertheless, it is a remarkable and important paper, integrating in a cogent, effective way many of the considerations which motivate a sociolinguistic approach. I have a few things to propose that go beyond Fillmore's paper; if he will accept those as well, then I can retire from this sort of advocacy altogether.

2I owe this term to Maxine Bernstein, in whose dissertation in progress I encountered it.

3Or, as heard and repeated by one linguist at Georgetown, socially reconstituted linguistics.

4This paragraph was omitted at the Saturday morning session. Two readers of the text beforehand had wished it could go much further into empirical examples; Fillmore's paper marvelously obviated any need.


6Notice that to attempt to handle all modes of meaning on one grammatical framework ends by dissolving structure and making the actual organization of both kinds of meaning difficult and even impossible to discern (cf. Jacobs and Rosenbaum 1971:viii-ix): 'the importance of expressing semantic insights has come to overshadow the criterion of expressibility within a formalizable rule'. The very point of discovering the organization of linguistic features in the service of stylistic and social meaning is an argument for keeping distinct and precise the ways in which language is not organized for such a function. For some linguists it may be sufficient to express semantic insights, rather than to maintain 'the initially indispensable desire for explicit precision that Chomsky inherited from structuralism' (Jacobs and Rosenbaum 1971:viii), but it would be a fatal disservice to the standpoint advocated here if the expression of social and stylistic semantic insights were regarded as warrant for scrapping structuralist inheritance and explicit precision. In this regard the grammatical analyses of Harris, Hiz, Chomsky and some others are more useful than recent attempts that in effect dissolve the many purely syntactic regularities that do exist. The transformationalists are right to reject the extreme to which some American structuralists went, such that one exception could disprove a universal. But have they not fallen into the same fallacy with the principle that one exception can disprove a level? The fact would seem to be that Chomsky is right that there are many syntactic regularities which are independent of the exceptions which motivate generative semantics, and which appear unmotivated from the latter standpoint. It is also true that others are right that there are
many phonological regularities that are independent of the exceptions that motivated 'systematic phonemics', and that appear unmotivated from the latter point of view (witness the awkward efforts to recapture an understanding of canonical forms by 'conspiracies'). The very plain truth is, as Sapir said, that 'grammars leak', and that the major sectors of a language (grammar): phonology, lexis, syntax, comprise patterns and habits which can have rather autonomous histories—in chronological and social time and space.

7I am thinking here of 'The fetishism of commodities and the secret thereof', Section 4, Ch. I, Part I, Book I, of *Das Kapital*.

8The two main facets of 'social' meaning can be identified as 'interpersonal' and 'textual', following Halliday 1970. That is, the facets of meaning involved with nonlinguistic context (the participants in the speech act and their interaction in that setting), on the one hand, the facets involved with the linguistic context, on the other. I put 'social' in quotes because it, like some other common terms for this aspect of language, seems to me to apply to all of meaning. All of meaning is social in basis and may take part in stylistic effect as well. Similarly, 'cognition' and 'ideational', for what is often called 'referential', should not imply that no cognition or thought is involved in expression of social identity, attitude, stylistic consistency or verbal art. Still, 'social' often will conveniently identify Halliday's 'interpersonal', and 'stylistic' either his 'textual', or simply non-referential status of features.

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APPENDIX: REPORTS OF INTEREST-GROUP SESSIONS

SOCIOLINGUISTICS AND RELIGION

Report by William J. Samarin, University of Toronto

The explicit goal of this session was to consider how religion could be used to learn more about how man uses language. The session prospectus that was sent to possible participants contained the following statement:

The approach of this session is linguistic, and its goals are linguistic. Religion, for the participants of this session, is only a domain of human behavior where understanding of man's use of language may be found. Contributions by sociolinguistics to the scientific study of religion are inevitable, but they are not the session's explicit goal.

It is religion in practice that this session will examine, 'practical religion' as E. R. Leach called it. Philosophical religion is not its topic, but sermons and song, prayers and chants, liturgy, rhetoric, styles of language, translations, and all other ways that language is used through voice or writing in the religious here and now. But that can be in the past as well as the present, in non-Western as well as Western societies, in private as well as institutional religion, in 'low' as well as 'high'.

Religion is studied, because it constitutes a 'sociolinguistic domain' par excellence: a sphere of human behavior where linguistic variables signal and symbolize overt and covert structures, functions, values, attitudes, etc. In many societies religion is a cultural given, thereby simplifying the work of the sociolinguist. Where it is a problematic, something to be identified, the investigator is challenged to use language as a
diagnostic tool. Religion is therefore studied, because it requires interdisciplinary research. Linguists will benefit from the competence of, for example, anthropologists, sociologists, and theologians. And religion is also studied, because it provides vast amounts of data, much of it of relatively easy access and most of which has never before been studied at all. In a cliché, religion is made to order for sociolinguistic research.

Participation from a wide range of scholars was sought: linguists, anthropologists, sociologists, psychologists, folklorists, and theologians. Breadth was also desired with respect to variety of religions (Christianity, Islam, Buddhism, etc.) and variety of speech forms (multilingualism, styles, written vs. spoken, etc.). It was hoped that there would also be represented an interesting sample of speech functions (e.g. symbolizing ethnicity, marking sacred as opposed to profane experience, etc.). A small number of the participants were asked to prepare papers for the session based on their experience and interests but bound by the goals of the session. They were the following:

Robert Cromack, State University of New York, Cordland. Social Constraints on Bible Translating.
Charles A. Ferguson, Stanford University. The Collect as a Form of Discourse.
Dale K. Fitzgerald, University of California, Berkeley. The Language of Ritual Events: An Analysis of the Structure of Anti-Structure.
Don Yoder, University of Pennsylvania. Religion and Language in the Pennsylvania German Culture.

The papers were summarized in ten minutes under the following rubrics: general and theoretical notions, the part of language that is functionally salient, genres of discourse, language as an independent variable, language as boundary marker, speech performance in religious contexts. Discussion followed each of these 'chapters'.

This session revealed that there is a lively interest in the field of religious sociolinguistics but that the number of people who have developed a competence in this field is still low. This would suggest
growth. It would appear that for a while longer we must look to anthropologists for most of our data.

SOCIAL AND ETHNIC DIVERSITY IN THE AMERICAN SPEECH COMMUNITY

Report by Ralph W. Fasold and Roger W. Shuy, Georgetown University

The session originally planned as a conference on the linguistics of social dialects developed into a 'language attitudes festival'. After proposing the social dialects topic, it occurred to us that there was relatively little ongoing linguistic work in social dialects besides Vernacular Black English, with some additional work on Puerto Rican English and Canadian French, and some incipient research on Chicano speech. Furthermore, the work on Vernacular Black English had already received a great deal of attention and it appeared that there was not a great deal to be gained from presenting yet another battery of papers on the same topic. At the same time, we were aware that there was a considerable amount of unpublished and ongoing work on attitudinal reactions to socially influenced variation. It was decided to retain the title for the session—after all, the attitudes being researched were based on socially relevant language variation—but to assemble scholars and papers in the area of subjective reactions to variation rather than on the syntax and phonology of varying features themselves.

It seemed that reactions of this sort were potentially important for linguistic theory with possible spinoff in the applied areas of language planning and social justice. Labov (The Social Stratification of English in New York City, Washington, D.C., 1966) showed that language attitude was one kind of evidence which is useful in detecting language in progress. While it seems overly simplistic to assume that only changes which are socially favored will be ultimately adopted in a language, it is equally obvious that attitudinal reactions play a role in the progress of language change. Attitudes are, or should be, a direct factor in making such language engineering decisions as selecting official languages, selecting languages or dialects of instruction in public education, and deciding who should be required or encouraged to learn what languages and dialects (see the papers by Ryan, Wölck, Taylor and, for an opposite view, the paper by Macnamara). To the extent that negative values are unjustifiably imputed to minorities by majority group members on the basis of their speech, attitude studies can be a first step in dealing with social injustice (Taylor, D'Anglejan and Tucker, Williams, Shuy and Williams). Other papers are groundbreaking studies on the mechanics of how language judgments
work (D'Angeljan and Tucker, Fraser, Palmer, Ryan, Sachs et al., Wolfram, as well as the papers mentioned above). The paper by Smith is a programmatic conceptual framework for understanding attitudes about language.

With the extension of the social dialects topic to a conference on language attitudes, the definition of American was extended to include North and South America, and not the continental United States alone. The papers by D'Anglejan and Tucker and Macnamara are based on research in Canada, the contribution by Wölck on work in Peru, and Wolfram's paper deals with Puerto Rican immigrants to New York City.

The number of papers given and the number of people who registered for the interest group necessitated holding two sessions. Even so, each speaker had to be limited to only ten minutes of presentation time which severely limited the speakers in doing justice to their topics. This limitation is compensated for by the projected publication of the collection by Georgetown University Press under the title *Language Attitudes: Current Trends and Prospects*.

The following papers were presented in the two sessions on American Social Dialects:

D'Anglejan, Alison and G. Richard Tucker. Sociolinguistic Correlates of Speech Style in Quebec.

Fraser, Bruce. Some 'Unexpected' Reactions to Various American-English Dialects.

Macnamara, John. Attitudes and Learning a Second Language.


Ryan, Ellen D. Subjective Reactions Toward Accepted Speech.

Sachs, Jacqueline, Philip Lieberman, and Donna Erickson. Anatomical and Cultural Determinants of Male and Female Speech.

Shuy, Roger and Frederick Williams. Stereotyped Attitudes of Selected English Dialect Communities.


Taylor, Orlando L. Teachers' Attitudes Towards Nonstandard and Black English.

Williams, Frederick. Some Research Notes on Dialect Attitudes and Stereotypes.

Wölck, Wolfgang. Attitudes Toward Spanish and Quechua in Bilingual Peru.

Wolfram, Walt. Objective and Subjective Parameters of Language Assimilation Among Second Generation Puerto Ricans in East Harlem.
SPEECH PERFORMANCE AND REPERTOIRES

Report by Roger D. Abrahams, University of Texas

This session was given over to reports on recent research by John Szwed, Barbara Kirshenblatt-Gimblett, Morton Marks, Richard Bauman, and the chairman. These were given seriatim and each was followed by a lively discussion by the participants. The initial discussions were primarily concerned with the relationships between code switching and the self-consciousness of performance. Dick Bauman discussed the notion of performance and the importance of understanding self-conscious performance styles of language use in the perception of the separation between the casual and the non-casual speech event. Barbara Kirshenblatt-Gimblett discussed code switching among Jewish Canadians and Americans between Yiddish, Yenglish (an intermediate code) and English. Morton Marks commented on the switching of codes in music and song and speech among various Afro-American populations during festival occasions for the purpose of asserting a regressive motive—that is, becoming more old-fashioned, more 'African'. Roger Abrahams discussed how different varieties of language use in specific performances in Afro-American communities were useful in an understanding of aspects of social segmentation. He surveyed the distinction made in many parts of Afro-America between the household and the street worlds, the private and the public domains, and the notion of the value of public performance of 'bad' motives. John Szwed, finally, discussed a number of theoretical issues which the study of performance had encountered in the past but which, because they had not been articulated effectively, had not been argued against successfully.

All of the speakers drew upon field experiences in making their remarks, and this direction was maintained in the remarks by the other participants. There was special interest shown in these remarks in how traditional performances provide a means of getting at distinctions between casual and non-casual varieties of speaking. There was also a good deal of animated comment on how these different varieties were paralleled by behavioral switching on the motor, spatial, temporal, and musical levels as well.
THE ETHNOGRAPHY OF INTERROGATION

Report by Lindsey Churchill, Graduate School and University Center, CUNY

The session on the ethnography of interrogation was organized quite informally. A background paper written by the chairman entitled 'The Grammar of Questioning' was distributed to help orient persons attending the session to one way of viewing the topic. Because many persons attending the session reacted negatively to the word 'interrogation', the focus of the topic was changed from 'interrogation' to 'questioning'. Then a number of persons were invited to speak informally, for fifteen or twenty minutes, about their research on questioning or how their research sheds light on the topic of questioning. The persons who agreed to talk were Aaron Cicourel, Mary B. Black, Emanuel Schegloff, Victor Yngve, Harvey Sacks, Charles Fillmore, and Allen Grimshaw.

The style of the meeting turned out to be that of an informal seminar. Speakers invited questions throughout their presentations, and these invitations were eagerly taken up. Discussion during the course of the presentation and following it was so extensive that only four of the scheduled speakers were able to present their materials.

Generally speaking, the various presentations revealed that research in this area was just beginning. All of the speakers brought forward interesting yet theoretically troubling examples of questioning behavior. It was apparent that each speaker, and many of the audience members, had been forced to consider formal properties of questioning because problems in questioning had intruded themselves onto them while pursuing other issues.

In my background paper I propose that 'hard' methods in sociology, organized around survey analysis and statistics, are inadequate to solve sociological problems in more than an approximate, practical manner. Sociologists should learn the largely non-statistical methods that linguists have developed over the last forty years, as a new starting point for developing methods. For example, a sociological topic of interest to me is interviewing. I believe that interviewing can never really be understood through survey methods or experimental methods based on 'variables'. Those methods simply cross-cut the rules by which interviewing gets done and therefore are unlikely to prove themselves adequate in the long run. I propose that we think in terms of a 'grammar' of questioning, so that we concentrate on how questioning-and-answering behavior is generated by rule. Categories of questions and of responses to questions, and the rules for
using those categories can, at least loosely, be conceived as a grammar.

But the same problem arises in this formulation that has arisen in linguistics when one asks how performance relates to competence in the grammar of a language. Actual questioning behavior outruns any rules devised to contain it. That is, persons do questioning at times in ways that break any rule I have considered, but they appear to understand one another perfectly well. One is faced with two unpalatable possibilities: (a) a grammar that contains an infinite number of rules (and perhaps an infinite number of categories as well), or (b) meta-rules that appear to put a great deal of autonomy in the person. An example is Harold Garfinkel's 'etcetera' instruction which says: 'Use any rule appropriately, even if that means deviating from the rule.' The question that I am left with is the following: How can we characterize this fundamental problem of usage?

Aaron Cicourel has been teaching a sociology course for medical students at the University of California, San Diego. One of the problems that he has been addressing is how physicians make a diagnosis. In most cases the physician must ask the patients questions to accomplish this task. To learn how they do questioning and to provide case material for the course, Cicourel tape recorded diagnostic interviews between physicians and patients at the university's outpatient clinic. He found that in many cases the interview was unsatisfactory. The physician would unwittingly antagonize the patient or otherwise not 'handle' the patient well. On occasion he would not discover significant information that only the patient could tell him. These difficulties were particularly acute with patients who did not speak standard English, for example, Mexicans who had been in the San Diego area a relatively short time.

The medical students in Cicourel's class found these tapes highly instructive, but only in a suggestive way. Cicourel found that he could only offer good advice to the students: 'Try to avoid doing this or that in your questioning practices as doctors.' He was not able to offer them a solid theory of how questioning is done, that would show how these difficulties arise in specific ways and thus how they might be repaired or prevented.

Mary B. Black found similar difficulties in her research on the Ojibwa language. Most of her information had to be gathered by questioning members of the local society. She speaks Ojibwa and could do her own questioning of native informants, but found that that was not a sufficient preparation. She quickly discovered that there was an etiquette governing the proper asking of questions, by unknowingly asking improper questions. She learned that it wasn't really nice to ask questions in Ojibwa society, particularly direct questions. For example, she might ask, 'Where are you going?',
elicit information about the language through the answer, only to re-
ceive the response, 'You wouldn't want to ask that, really.' Etiquette
demanded that she rephrase that question to: 'I wonder if you're going
somewhere', though that formulation might not yield the desired lin-
guistic information.

It soon became clear that Black was being tutored by her Ojibwa
informants in how to acquire information appropriately. She had to
learn more about these rules of etiquette than she really wanted to
know, in the sense that the study of etiquette was not her primary
concern. She proposes that all ethnographers are in a similar situ-
ation since much of their work must be done by asking questions.
Therefore, the topic of questioning behavior is much more important
in anthropological field methods than has previously been appreci-
ated.

Victor Yngve discussed his long-standing interest in deriving a
scientific understanding of how people communicate. He proposed
that linguistic grammars are only of heuristic value in studying what
persons do when they communicate. Linguistic grammars are ori-
ented to written texts, and, as is well known, writing texts and com-
municating orally are quite different tasks, involving different sets
of rules. Theoretical linguistics is simply not relevant to socio-
linguistics and the other 'hyphenated' linguistic fields.

To learn more about how persons actually do communicate, Yngve
arranged a situation where subjects would have to make conversation
with one another. He immediately ran into phenomena for which tra-
ditional linguistic grammars were of no help. Taking turns in the
conversation was one such phenomenon. Who the first speaker in
the conversation would be was a second phenomenon of this kind.
He discussed some examples from his tapes involving these phenomena,
showing how questions enter in. He is presently trying to devise a
theoretical way to account for phenomena of this kind.

Emanuel Schegloff took sharp issue with much of the orientation of
the other talks. He proposed that the category of questions may be of
no more than passing interest for researchers in this area. We are
presently fixated on it because it comes to us from our common-sense
upbringing in the culture. But that is no reason why it should become
an important category in a strong theory of questioning. Questions
can only be a starting point. Then let the theory flow wherever it
may, perhaps moving entirely away from the category with which the
theorizing started.

Schegloff's research interests are in how people produce conver-
sation. For him questions are indeed produced in conversation, but
have no necessary relevance to a theory of the production of conver-
sation. Questions are simply something to be explained, in the sense
of being reproduced by the theory.
For Schegloff the theoretical place to begin is in the sequential organization of conversation. Question-answer pairs are a major sequence in conversation, but not the only one. The best way to think about questions is in terms of the function they play in helping to produce whatever activity is going on at the moment in the conversation. And persons are no respecters of conventional grammatical categories when it comes to producing activities in the course of conversation. Hence, one can have questioning without questions being involved, and one can have questions that have nothing to do with questioning. Persons simply use the typical grammatical categories as necessary to do whatever they need to do.

One obvious conclusion reached by participants is that questioning is a much more subtle and puzzling phenomenon than we have previously suspected, and essentially new theories may have to be developed to explain such behavior.

PIDGINIZATION AND CREOLIZATION

Report by David DeCamp, University of Texas

The special interest group on pidginization and creolization met on March 18, 1:00-4:00, and then reconvened at noon the following day for an additional lunch meeting arranged by Charles Ferguson. More than sixty scholars attended these meetings. This was not only the largest group of creolists ever convened but also the most diverse: in nationality, in fields of research, and in opinions about what pidgin-creole studies are and should be.

Ian Hancock presented a survey of current pidgin-creole studies in England, distributing a name and address list of sixteen creolists at seven British universities (Birmingham, Exeter, Lancaster, Leeds, London, Oxford, and York), and discussing their work on creoles ranging from Mauritius to Belize. The presence of so many recent immigrants from creole-speaking territories has given great impetus to these studies in England, Hancock reported, and journals such as the New Beacon now regularly publish articles by African and West Indian authors written in Sranan, Krio, and other creoles.

Other reports described research (recent, current, or projected) on Neo-Melanesian (G. Sankoff), Sango (W. Samarin), Chinook (I. Hancock, P. Kay), Yiddish (R. and B. Hall), Papiamentu (E. Bendix, R. Wood), Haitian (R. and B. Hall), Sranan (R. and B. Hall), San Andrés (B. Bendix, J. Edwards), St. Croix (R. Di Pietro), Hawaiian English (W. Labov, R. Day), and Black English (W. Stewart). These reports and the discussions following them revealed basic disagreements in the definitions of terms like ‘creole’ and ‘creolization’ and
in the meaning and even the relevance of questions such as 'Was Yiddish earlier a creole?'

There was even less agreement on priorities for future research. One opposition was between formal and ethnographic approaches; i.e. some were interested primarily in the linguistic structures of creoles, including the variations in those structures (e.g. C.-J. Bailey, E. Bendix, D. Bickerton, B. Robson), while others (e.g. J. Edwards, W. Labov, W. Samarin) would stress the social and cultural functions of those structures. Labov lamented that only one of the papers presented at the 1968 creole conference at Mona dealt with the use of language. Creolists are highly competent and aware of the use as well as the structures of their languages, he said, but they tend to publish nothing but formal linguistic papers.

About half of the creolists present had a serious interest in historical studies, but they differed in assigning priority to the study of particular stages in the pidgin-creole life cycle. Some (e.g. the Halls, E. Haugen, R. Wood) considered all historical stages equally important. Some (e.g. W. Samarin, G. Sankoff, P. Kay) were particularly interested in the earlier stages, especially the transition from pidgin to creole status. Kay said that he looked at the creation of pidgins as a natural experiment from a psycholinguistic point of view of the creation of language de novo in a minimal functional context which then expands. Other creolists, however, agreed with Stewart in stressing the later, post-creole stages. Discovering the ultimate origin of Black English, he held, is less important than determining whether the changes now going on in Black English are the same decreolization processes observed in other creole contexts.

The division between theorists and empiricists, common in all areas of linguistics, was very apparent among the creolists. Labov drew applause for his plea that since there is now no viable theoretical model which permits us to integrate our knowledge of the use of language into our grammatical descriptions, we should now concentrate on simply reporting the facts, providing secondary reports that will be reliable and useful to theorists in the future. The theorists (e.g. C.-J. Bailey, D. Bickerton, W. Stewart), however, were equally vehement in insisting that objective empirical reporting is impossible except within the framework of a theory, and that distortions in the perception of data resulting from a lack of theory are as damaging as distortions in theory resulting from lack of data.

Sankoff and Kay perhaps came the closest to a synthesis of these opposed positions by calling for neither abstract theoretical models nor the collection of raw data, but rather for hypotheses which make empirical claims and so can be refuted with data. The proper use of data then is for other creolists to confirm or shoot down these hypotheses. They outlined four tentative hypotheses: e.g. a true pidgin
has a trivial phonology consisting only of shallow underlying forms and phonetic rules which vary according to the native language of the pidgin speaker. These hypotheses were hotly debated, but the very fact that they evoked examples and counterexamples from the other creolists indicated that the approach is productive.

The Friday meeting was a work session devoted to planning a pidgin-creole newsletter. It was agreed that this newsletter would be a simple mimeographed quarterly, distributed free to anyone who requests it. One issue per year is to contain a roster of names, addresses, and research specialties of creolists throughout the world. Estimates of their number ranged from two hundred to five hundred. Other topics in the newsletter will include current bibliography, notices of meetings, new projects, and research in progress.

The group expressed regret that John Reinecke had been unable to attend the meeting. Reinecke had mimeographed for distribution, however, a report on the annotated bibliography which he is preparing with the collaboration of David DeCamp, Ian Hancock, Stanley Tsuzaki (Hawaii), and Richard Wood. This work, which will contain approximately five thousand annotated entries and a survey introduction to each pidgin and creole, is nearly ready for publication. Reinecke's report, together with several reports of research either distributed at the meeting or alluded to in the discussions, will be published in Pidgin-creole studies: current trends and prospects, edited by DeCamp and Hancock, which will appear as a monograph in the George-town University sociolinguistics series.

PARALINGUISTICS, KINESICS, AND TEACHER TRAINING

Report by Bruce Fraser and Hugh Mehan, Indiana University Language Research Foundation

The use of language and gestures by teachers and children in the classroom was the topic of discussion for this interest group. A particular concern was to suggest ways of making teachers and teacher trainers aware of the sociolinguistic and kinesic issues raised during this discussion so that more effective classroom communication can take place. Fraser and Mehan began the session by suggesting how the topics of sociolinguistics and kinesics relate to school situations. This introduction was followed by general discussion, and discussion of a videotaped classroom lesson. The following summarizes the points made in those parts of the session.

In the conventional classroom, the teacher asks questions and the child is expected to answer them; the teacher gives instructions, the child is to follow them. Often prospective teachers are told in the
quiet of their education classes that the way to teach children is to capture their attention and then to present clear, complete, and distinct instructions to the children. This recommendation usually means the teacher adopts a formal instructional style and talks in standard (classroom) English. However, examination of classroom interaction shows the question-answer sequence in which one speaker is to speak at a time does not operate. The teacher has simultaneous demands on her attention because many children have different problems at the same time. Likewise, the child has a number of information sources competing for his attention: the teacher, his classmates, his memory of past lessons and experiences, his plans for after school. In addition, the teacher’s instructions are accompanied by nonverbal cues which can tell the child about expected answers. The teacher, for example, can stand beside or look at the desired answer displayed on a chalkboard, or she can accent the expected answer, as in: ‘Does this block belong in the big group, or the little group?’

A critical problem for anyone trying to understand classroom interaction (whether child, parent, teacher, teacher trainer), is that of determining the basis of the child’s responses to teachers’ requests. That is, how do aspects of teachers’ instructions (their syntactic organization, semantic content, pitch, tone, etc.), the teachers’ gestures and bodily orientations and aspects of the classroom surroundings provide information to the child who is trying to carry out routine instructions? The same question can be asked of the teacher’s behavior: what is the basis of the teacher’s evaluation of the child’s performance? How does what the child says, how he says it, his style of clothes or talk, skin color, and eye contact, for example, contribute to the teacher’s evaluation of the child?

Appropriate modes of nonverbal behavior, including ways to sit and stand, when to talk, run, when and where to play are taught to the child in school. The middle-class child has already begun to learn that specific situations require different gestural configurations. Critical questions for understanding the classroom are: Do children from different backgrounds share these rules for nonverbal behavior? Does the teacher recognize that children from different backgrounds have different modes of nonverbal expression? For example, some teachers have complained that American-Indian children are inattentive and disrespectful because they do not look them in the eye. These teachers do not recognize the cultural value attached to lowered eyes as a sign of respect for adult authority. Similar examples can be drawn from the school experiences of Puerto Rican, Chicano, and Black children.

Initial discussion centered on the advisability of combining the two interest groups. Those who favored separate groups said the
theoretical and practical issues associated with kinesic and socio-linguistic behavior were too distinct to be discussed in one session. Others said these fields were not sufficiently advanced to offer concrete proposals to educators. Those who favored combining the sessions cited the cross-fertilization of ideas and the recognition of the problems facing practitioners and theorists as advantages.

Attempts to arrive at a working definition of nonverbal behavior were made. It was considered advisable to study nonverbal behavior in a context of verbal and situation features rather than in isolation.

Mehan showed a videotape of a first-grade teacher engaged in a language development lesson with a small group of children. Initial discussion centered on the nonverbal relations between the teacher and the children. Some said the teacher seemed rigid and stiff in her demeanor and observed she crossed her legs so that her body leaned toward a blond Anglo child on her right, and away from a Chicano child on her left. Some members of the discussion group interpreted this activity as the teacher's conscious or unconscious exclusion of the Chicano child. Others felt the teacher might have adopted that sitting posture so that she could keep an eye on the remainder of the class which just happened to be situated over the Anglo child's shoulder. Others suggested the teacher may have been forced to sit in that position because of the difficulty of sitting in primary-school chairs or because the recording equipment hampered her style. Others offered the interpretation that the teacher was oriented toward the Anglo child because she needed encouragement while the Chicano child needed to be restrained from dominating the lesson. The need for relevant background material to support any such interpretation obtained from detailed field observations was pointed out.

Mehan then analyzed the instructions and questions in the lesson. He pointed out that their vagueness and ambiguity required the children to consult other sources of information in the setting in order to carry out the instructions. Sources identified included the teacher's and other children's hints and cues, the teacher's paralinguistic and kinesic behavior, and instructions given in previous lessons. The verbal and nonverbal nature of teachers' instructions suggests it is necessary to study the child's interpretive abilities and the teacher's educational decisions. It seems the child may have the ability to go beyond the information given in verbal messages, to be able to recognize that understanding the teacher requires invoking additional features, linking utterances to contextually relevant information, filling in missing information, and calling on previously given material.

Instances were pointed out where the children interpreted the teacher's instructions differently than the teacher expected. These differences in interpretation were said to be the result of the inherent ambiguous nature of verbal instructions.
Bruce Fraser was originally scheduled to lead an interest group entitled 'Sociolinguistics and Teacher Training', while Hugh Mehan was to lead one entitled 'Sociolinguistics and Kinesics'. When we realized the similarities in our plans for presentation, we decided to combine the two sessions into one. This decision prompted considerable discussion in the interest group, the substance of which is summarized here.

**LANGUAGE SOCIIALIZATION**

Report by Courtney B. Cazden, Harvard University

Since the term 'language acquisition' has come to refer primarily to the child's acquisition of language structure, it seems useful to assign the term 'language socialization' to acquisition by the child of all aspects of communicative competence. It would thus include at least all of the following:

1. acquisition of the structure and vocabulary of one or more languages or dialects, referential meaning of all forms;
2. learning to use language for any and all expressive and communicative functions;
3. acquisition of awareness of, and attitudes toward, language and language differences;
4. the nature of the social context and environmental assistance for all the above.

The session on language socialization was an opportunity for people doing research in these areas to define their questions, discuss methodological problems, and share some results. Following is the current research of some workshop participants.

Maggie Bruck, Department of Psychology, McGill University. The influence of kindergarten experience on the language acquisition of children from different socioeconomic backgrounds. Tests of grammatical and communicative abilities were given to middle-class and lower-class white children at the beginning and end of kindergarten. In a factor analysis, these two sets of abilities were statistically independent. There was an initial social-class gap in certain communicative skills, and in grammatical production but not comprehension. During the year, that gap lessened more for grammar than for communication, suggesting where school language programs should concentrate.
Betty Bryant, Harvard Graduate School of Education. An analysis of spontaneous mother-child interaction to determine how Black mothers with different degrees of experience with written language socialize their preschool children in skills of explicit referential communication.

Polly Caskie, Department of English, Florida State University. An experimental study of the influence of age, race, and social class on children's ability to understand and produce certain negative and interrogative constructions.

Catherine Garvey, Department of Psychology, Johns Hopkins University. Exploratory study of the growth of social speech by analysis of video tapes of the activity context and verbal antecedents of utterances of dyads of preschool children.

Jenny Cook Gumperz, University of California, Berkeley. An attempt to isolate sociolinguistic skills involved in communicative competence as part of an evaluation of the influence of an elementary science curriculum (SCIS). Experimental tasks required pairs of fifth graders to role-switch, that is to consider the perspective of the other in giving descriptions and instructions. The aim was to elicit speech during the children's pragmatic involvement in activities where talk was essential for 'getting the task done'. While most of the children were able to accomplish the tasks, they differed in the sociolinguistic strategies used; for example, they used different ratios of verbal to nonverbal signals and differed in the extent to which they relied on contextual clues to carry their meaning. The children's practical experience of science increased their ability to adapt their speech to the communicative needs of the other person.

Sara Harkness, Department of Social Relations, Harvard University. The relation between social behavior and language style in Spanish-speaking three-year-olds in rural Guatemala—including style shifting among behavioral contexts, and style differences which may depend on the predominant context for individual children. (These two variables seem to correspond to Basil Bernstein's distinction between 'speech variant' and 'code'—CBC.)

Laura Lein, Department of Anthropology, Harvard University. Ethnographic study of the language of social control used in Black migrant families at home in Florida and in migrant camps in upstate New York, and the sociolinguistic discontinuities their children face in adapting from home to school.

Loren Nussbaum, Center for Applied Linguistics. Development of a communicative curriculum using game-type activities as analogs to language use outside the classroom.

Marilyn Rosenthal, Program in Sociolinguistics, Georgetown University. An experimental study of age, race, and social-class correlates of preschool children's discrimination among dialects and value judgments about them.
Roger Thompson, Department of English, University of Florida. Language loyalty in Austin, Texas: a study of a bilingual neighborhood. Locality of childhood of heads of household was a more important determinant of the extent of their present use of Spanish at home than age, generation, occupation, or education: those raised in rural areas now speak more Spanish at home than those raised in Austin. Austin second-generation families are not teaching Spanish to their children; without the continuing influx of rural Mexican-Americans, use of Spanish would cease within one generation.

LANGUAGE PLANNING

Report by Joan Rubin, Tulane University

This session was organized with a view to trying to establish how much common core of theory there was among scholars interested in language planning, to giving scholars engaged in language planning research an opportunity to report on their progress, problems and findings, and to trying to assess what sorts of research the agencies faced with national development and language problems felt was most called for.

It became clear during the discussion that even the most commonly used concepts would need more consideration and clarification; such often-used terms as codification, standardization, standard language, flexibility, stability, and even language planning proved to have different meanings for the participants. Such differences have important theoretical and practical implications. In attempting to further elucidate Prague School definitions of standard language terms, Paul Garvin kindly offered to translate a passage from the 1932 volume Standard Czech and the cultivation of good language (which now appears in the separate volume of the papers of this session). Throughout the discussion the term language planning was given different meanings ranging from that of individuals who give some thought and perhaps action to language problems (their own and others') to the more normative position which excludes everything but national government planned attention to language problems. Full attention to this discrepancy was not given because of a lack of time but serious need to do so still remains.

Another question which was raised in various ways was the role of the language planner and language planning theory in decision making. Some participants pointed to the responsibility of the language planner in helping, where possible, to bring about ‘rational’ decisions given the particular language problems and the particular socio-cultural
setting. However, the definition and elaboration of such 'rational' criteria (both linguistic and socio-political) remain to be further elaborated.

Several research reports were given; unfortunately, due to the shortage of time these had to be quite brief. These reports have all been modified and expanded into formal papers and are included in the separate volume of this session.

Finally, two oral reports were given by representatives of a private and a government agency. These persons were invited to indicate the language problem research priorities which they felt their organization had. In the case of the U.S. Office of Education, a basic need was expressed for an assessment of existing language resources and personnel and utilization of language resources; whereas in the case of the Carnegie Corporation of New York the focus was upon basic research in child language development and on minority group language problems. (The Ford Foundation representative was unfortunately unable to attend.) In general while there was great enthusiasm for the topic several of the participants expressed regret that this section of the session was so brief since they had hoped that more concrete statements about research priorities would have resulted from the discussion.

Perhaps another meeting could devote a discussion session solely to clarification of a theoretical core so that greater progress could be achieved. It was clear from this session and from other expressions of interest that the field of language planning has already posed many challenges for social science theory and offers potential assistance for the more practical field of language and national development. The discussion of such theoretical problems has only begun.