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Raimonde Dallaire
William Gage
Paul L. Garvin
Paul M. Postal
Eric P. Hamp, Moderator
The Transformation Theory: Advantages and Disadvantages
A. Richard Diebold, Jr.
Code-Switching in Greek-English Bilingual Speech
Einar Haugen
Schizoglossia and the Linguistic Norm
Colman L. O’Huallachain, O.F.M.
Bilingualism in Education in Ireland
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Indian and Ladino Bilingualism: Sociocultural Contrasts in Chiapas, Mexico
Fred W. Householder, Jr.
Greek Diglossia
William G. Moulton
What Standard for Diglossia? The Case of German Switzerland
William A. Stewart
Functional Distribution of Creole and French in Haiti
Charles A. Ferguson
Problems of Teaching Languages with Diglossia

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GEORGETOWN UNIVERSITY
INSTITUTE OF LANGUAGES AND LINGUISTICS
REPORT
OF THE THIRTEENTH ANNUAL
ROUND TABLE MEETING
ON LINGUISTICS
AND LANGUAGE STUDIES

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FROM time to time the Institute of Languages and Linguistics, Georgetown University, publishes manuscripts intended to contribute to the discipline of linguistics and to the study and teaching of languages. Manuscripts should be addressed to:

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Foreword

The 13th Round Table Meeting brought a new feature to its program: the first session was a true Round Table discussion involving four panel members and a moderator, with the Transformation Theory as the topic. The arguments and counter arguments became lively at times, and the editor has attempted to retain as much as possible of the vivid nature of that first session. Nevertheless, the account had to be edited and now contains verbatim renderings of the proceedings only to a limited degree. None of the linguistically-significant remarks have been omitted or altered except where it was necessary to make them more conformable to the standards of the Monograph Series.

The papers presented in the other two sessions as well as the edited versions of the discussions which followed, are reproduced in full.

The editor is happy to take this opportunity to thank the speakers who made the meeting a success—both the panel speakers and members of the audience who contributed questions and remarks. She also is indebted to the President of Georgetown University, The Very Reverend Edward B. Bunn, S.J., and to Dr. Robert Lado, Dean of the Institute of Languages and Linguistics, for their confidence, support and encouragement. She wishes to express her particular gratitude to her colleagues on the faculty of the Institute of Languages and Linguistics and to staff members and students who helped loyally and generously to make the 13th Round Table Meeting worthy of its predecessors.

Elisabeth Delorme Woodworth, Editor
Introduction

Welcoming remarks by the
REVEREND FRANK L. FADNER, S.J., Regent
Institute of Languages and Linguistics

Ladies and Gentlemen, Colleagues, Participants and Guests at Georgetown University's yearly Round Table Meeting on Linguistics and Language Teaching:

When your Chairman, Dr. Elisabeth Woodworth, to whom we are all indebted for the arrangement of this year's meeting with its captivating programme, wrote me in February reminding me of the forthcoming event, she began her remarks with a sigh: "A year passes so very quickly! Much faster than most of us like!"

I agree with Dr. Woodworth's note of dismay at the rapid passage of time which keeps us breathless in a race to keep from being overtaken unprepared. But I take issue with the implication that the flight of years necessarily brings regrets. Certainly this is not the case with the history of these most fruitful Round Table Meetings sponsored every year by our Institute of Languages and Linguistics. This is the thirteenth such venture, and looking back we cannot help but be impressed by the quantity of things we have learned here. The subjects have ranged from the practical evaluation of our Country's need for language study, the relation of Linguistic Science to educational psychology and classroom guidance, anthropology and the humanities, valuable speculation on meaning and language structure, language and culture, to the technicalities of Machine Translation. Certainly, it has been salutary for us to grow increasingly aware of our human smallness and our effectiveness in the awesome expanding universe of the Renaissance in which we now live.

And certainly, this year we look forward to the next few hours which will be given over to the aspects, implications and possibilities of bilingualism. Some may complacently reflect on the present-day universality of the English language to which we were born—a tongue employed by a mere handful of a million and a half a bare nine centuries ago. Some may come up with the objection that the
winning genius of our mode of speech makes a consideration of bilingualism and diglossia an impractical speculation, until they realize that each of us as a human being, in the very pursuit of universality itself, is called, somehow or other, to be another man, to know another world, to become "all things to all men."

Your Chairman asked me for "a few welcoming words." They tell me that this great word welcome is the old Anglo-Saxon compound meaning literally a guest who comes so as to please another's will. In the name of the President and the Board of Directors of Georgetown University I call all of you here on these premises this morning just that. By way of an invocation, if you want, I think we should all ask the Almighty Father of us all and the source of all our wisdom to inspire us in these days of thoughtful deliberation. That is the enthusiastic wish of your hosts in these halls.
PANEL I

THE TRANSFORMATION THEORY: ADVANTAGES AND DISADVANTAGES
Mr. Hamp: Mr. Postal is going to give us a presentation which will deal with the aims of linguistics and the rationale of generative grammars. And then, after that, we will have a free discussion on the part of the panel.

Mr. Postal: I should like to begin by trying to embed the discussion of the ideas of this session within the matrix of the development of grammatical studies in recent years as I see it. This description is necessarily skeletal and oversimplified.

Two questions have been systematically confused in almost all critical discussions of the topic of this panel. One is the question of whether adequate linguistic description must take the form of a generative grammar. The other is the question of whether an adequate theory of generative grammar must necessarily utilize transformational devices. One cannot really appreciate the second question unless one realizes that the answer to the first question must be 'yes'. Since this is not generally accepted, I should like to devote the following remarks to this point.

We can note historically two different traditions of grammatical description. Traditional grammar had a significant aim, namely, to correctly describe the construction of properly formed sentences in the language under description. To this end it utilized various forms of rule, implicit and explicit, the best known of which are the example inflectional paradigms. Methodology was given almost no attention. From the point of view of serious linguistic theory, traditional grammar was unsuccessful because of two fundamental flaws. First, traditional grammars do not in fact begin to tell one how to construct all and only the sentences in the language being described. Instead, they provide a few analyzed examples, a large
set of sample paradigms, and a collection of typical word patterns often supplemented by many example-sentences. Most of the work of actually constructing sentences is left to the linguistic abilities of the intelligent reader. But it is just these abilities that we are interested in explaining in linguistics. And by depending on these in an essential way, traditional grammar fails to characterize or explain them, and thus the ability of speakers to produce and understand sentences is left a mystery. This failure of traditional grammar can correctly be characterized as a lack of explicitness. By failing to make clear the types of rules involved in linguistic description, and therefore automatically failing to show how such rules describe the structure of sentences, traditional grammars simply do not present a testable description of the sentences of the language.

A second, and a little less severe flaw, is that traditional grammatical writings provide no justification for particular grammatical descriptions. Furthermore, no reasons were given as to why languages should be described in just the way traditional grammar describes them, that is, with the primacy of the word, example paradigms, semantically defined categories, and so on.

The development of modern descriptive grammars of the 'item and arrangement' variety provides a second major tradition of grammatical description. Where traditional grammars have a significant aim but fail in carrying out this aim, modern item and arrangement grammars have, I think, to a large extent lowered their aims below the point where the results of linguistic research can have much import for serious linguistic theory. The chief goal of such grammars appears to be little more than the provision of an inventory of elements, phoneme, morpheme, constituent, etc., with which utterances may be represented. The notion of structure assigning rule is even less developed in modern linguistics than in traditional grammar. An emphasis on inventories of elements as over against rules is, I think, the major flaw in contemporary linguistics. Even where some attention is given to the attempt to state how the elements may be combined, or how structure may be described in terms of them, these attempts come to very little because the emphasis on the units and putative methods for their discovery prevents taking seriously the character of the descriptive devices used. Hence, these are almost never made clear. Even on the lower levels of description, when listening can, in principle, cover all the facts, item and arrangement grammars have a built-in inability to formulate any of the real regularities of the language, because they treat everything
as if it were an exception. Such grammars are simply sets of utterance classifications. These classifications may be quite explicit, but this explicitness is of little value, because such grammars do not provide any basis for an account of how speakers produce and understand an endless class of sentences, the overwhelming majority of which they have never heard before. That is, where traditional grammar fails by not providing explicit structure-assigning rules which can carry out its aims, modern item and arrangement grammars fail by reducing their aims through a substitution of the notion of lists for the notion of grammatical rules. The explicitness of the modern grammars is the explicitness of a library catalogue and can provide no more explanatory insight. This aspect of modern descriptive grammars has not been overlooked by all its supporters. Indeed, Joos \(^1\) has gone so far as to see this as an important virtue of modern linguistics, and he has asserted that it is entirely correct that descriptive grammars provide only classifications and not explanations.

Similarly, although Hockett noted in 1953 \(^2\) that the widely accepted principles of phonemic analysis are arbitrary, he failed to develop this correct insight into the obvious conclusion that there are radical inadequacies in the formulation of the goals of a field whose methods are arbitrary.

We noted that traditional grammar provided no justification for grammatical description. This aspect of linguistic description has not been overlooked in modern linguistics. Indeed, it has been given a central position. Modern item and arrangement linguistics has adopted the extraordinary position that it is possible to justify gram-

\(^1\) "An older term for the new trend in linguistics was 'structural'. It is not idle to consider how the term 'descriptive' now came to replace it, even if not all the reasons can be identified. The Sapir way of doing things could be called structural, but the term was more often used for the stimulating new ideas that were coming out of Europe, specifically from the Cercle Linguistique de Prague. American linguistics owes a great debt to that stimulation; but in the long run those ideas were not found to add up to an adequate methodology. Trubetzkoy phonology tried to explain everything from articulatory acoustics and a minimum set of phonological laws taken as essentially valid for all languages alike, flatly contradicting the American (Boas) tradition that languages could differ from each other without limit and in unpredictable ways, and offering too much of a phonological explanation where a sober taxonomy would serve as well. Children want explanations, and there is a child in each of us; descriptivism makes a virtue of not pampering that child." Martin Joos, *Readings in Linguistics*, American Council of Learned Societies, 1957, page 96.

mars, which in its terms means listed elements, by showing how these lists can be derived from the data by applying explicit classificatory procedures. This idea involves the necessary conclusion that impact upon the simplicity of grammatical rules can never be used as a basis for a choice of linguistic elements. Under this view, the discovery of units by classification is necessarily prior to the statements of their distribution—such statements being the closest that item and arrangement grammars come to the notion of grammatical rule.

What has been overlooked here is that there is an endless number of procedures which can be applied to linguistic data to turn classifications into units. Hence, we cannot, in principle, justify linguistic description by showing that it follows from some procedure. This ignores the crucial question of how one justifies one's choice of procedure. It is common to justify procedures by arguing for their rigor, but this is irrelevant. A rigorous procedure for arriving at linguistic elements is to group every sequence of three phonetic segments into a unit: the tri-eme. This procedure is maximally rigorous and suffers only from one flaw, which it shares with most other suggested linguistic procedures, that it yields results of little obvious value.

To sum up, modern descriptive grammars of the item and arrangement variety fail on essentially the same grounds as traditional grammar. They fail to provide an explicit characterization of the sentences of the language under description and they fail to offer any rational justification for particular descriptions, even to the degree to which a list may be considered a linguistic description.

However, the failure of modern grammars is due to different reasons than those of traditional descriptions—this being the idea that a grammar can be a list of elements, and the notion that a grammar can be justified by showing how it was discovered.

The development of generative grammar has been based on the desire to combine the significant goals of traditional grammar

3 This may be considered as simply a special case of the general problem of how one justifies a general theory of language. Presumably, as in other fields, this can only be done by showing that it is true. It is symptomatic, perhaps, of how curiously the goals of linguistics have been stated in recent years that it sounds quite strange to ask whether such principles as complementary distribution are true.


5 For an extensive bibliography of this field see the appendix to the second edition of Syntactic Structures, by Noam Chomsky, The Hague, 1962.
with the explicitness of modern grammar, in order to provide a description of language which can provide the basis of an explanation of the production and understanding of sentences. The first step in such a program would appear to be a precise characterization of the linguistic knowledge which mature speakers have of their language. In particular, a grammar must characterize the ability to distinguish sentence from nonsentence, an ability which, in principle, serves to define an infinite class of well-formed utterances, only a small number of which have been previously experienced. Furthermore, an adequate grammar should not only account for the above feature, but also for the enormous amount of intuitive knowledge which speakers have about the structure or composition of the sentences of their language. Thus a grammar of English should account for such facts as the following:

1. *John eats meat* is a sentence, *eats meat John* is not.
2. *They are flying planes* is ambiguous.
3. *John eats meat* and *meat is eaten by John* are closely related.
4. *John's house, this meat, glorious hero, and the boy whom I saw* are expressions of the same type as opposed to such forms as *kills* or *runs beautifully*.
5. While neither *tfrit* nor *slig* is an actual English word, the latter but not the former is a possible English word. 

The number of sentences or grammatical utterances of a natural language is infinite. Hence the number of facts like 1-5 above is also infinite. Therefore, an explanation of these facts cannot be a mere list of sentences, regardless of how structural or classified. But since our explanation of these facts presumably must take the form of some posited entity which the language user can internalize and since the user must surely possess only a finite amount of storage.

The manner in which a generative grammar explicates facts like 1-4 is explained in some detail in *Syntactic Structures* and in many of the other publications by Chomsky and R B. Lees. The explanation provided for facts like 5 is considered by Morris Halle in his *On the Reality of Generative Grammars*, forthcoming in Word. Facts of this latter type are of particular relevance when we contrast an approach based on generative rules with one limited to utterance classifications. For since neither possible nor impossible non-English words will ever appear in an actual corpus, an approach to linguistics which insists that a grammar is some classification of the data in a corpus can never provide any insight into the difference between forms such as *tfrit* and *slig*.

*This possibility is apparently seriously entertained by Dwight L. Bolinger, *Syntactic Blends and other Matters*, Lg. 37. 381, 1961.*
space, it follows that our explanatory device or grammar must nonetheless be finite. To accomplish this, a generative grammar is conceived of as a precise formal device, a finite set of rules which can enumerate, generate, or list the full set of sentences of the language and automatically assign to each generated sentence its correct structural description or grammatical analysis. These are the minimum requirements which a scientific description of a language must meet. Failure to generate all the sentences means that the grammar does not describe the complete subject matter. Failure to automatically assign structural descriptions of generated sentences is failure to make clear the claims which the theory makes about each sentence and thus robs the theory of testability. The ultimate failure here is of course, to not explicitly assign the structural-description sentence, in which case the grammar is completely empty. Failure to assign correct structural descriptions simply means that false claims are being made about part of the domain of study. It is evident that both modern and traditional grammars fail on all these counts. The question of justification of grammars is handled in generative grammar as in other sciences. We justify a grammar by showing that it is the simplest theory, capable of explaining all the facts. An adequate scientific description of language thus involves, as a minimum, an account of the possible types of linguistic rule, an account of the way these rules enumerate both sentences and their structural descriptions, and an account of the way simplicity of grammars may be determined. Modern linguistics has given much attention to the notion of grammatical analysis or structural description, but it has without justification identified this notion with the notion of utterance classification and concentrated on the idea of arriving at analyses of sentences by manipulations of phonetic transcriptions. At any rate, it has given no attention at all to the finite device or set of rules which the speaker must have in some way internalized and which can assign an analysis to an infinite class of properly formed utterances. By failing on account of such device, descriptive linguistics cannot begin to account for the behavior of speaker or listener, which is surely its task if it is to have any significant function beyond the preparation of catalogues of facts.

MR. GARVIN: I think that it is useful to make one point at the very outset—that in a critique of task approaches to linguistics, whether "traditional" or "item and arrangement," (or for that matter "item and process," which has had some very significant representatives), it is useful to distinguish between two orders of critique. One
is a simple critique of competence. On this ground a great many traditional grammars, as well as a great many item and arrangement grammars and, possibly even some transformation grammars, can be rejected. This is the first point. The second point is the one which is to be considered in this discussion, whether or not with a traditional, or non-transformation, or non-generative orientation, we could produce a grammar at all, irrespective of competence. It strikes me that many of the criticisms leveled against the item and arrangement grammar are simply matters of technical competence, or lack of thoroughness, or lack of proper classificatory criteria for the data, rather than a lack of merit in the taxonomic or classificatory approach to linguistics. I do not think it is useful to set up a straw man and say, “now here is a grammar which is item and arrangement and it is not valid.” (I will also say that it is not valid, but for the simple reason that, in my opinion, it is a very poor representative of a given language.) I don’t know how many really good item and arrangement grammars there are. There are one or two really good articles, and those I would debate more readily than some of the less successful item and arrangement attempts.

The second question is whether it is legitimate to take for granted, as a basis for discussion, that the purpose of a grammar is to account primarily for sentences. It might perfectly well be possible to say that a grammar might account for, let us say, utterances. This is especially so, if the grammar is alleged to be an explication of the behavior of the speaker, since the only definable behavioral units one can find in the literature are utterance units.

The third question, which to me is not at all clear, is whether or not it is the purpose of a grammar to explicate behavior at all. I always thought that the purpose of grammar was to describe structure, and that behavior was to be described by psychology. If you wanted to explicate the behavior of the native speaker to the extent to which this is possible without looking at utterances, you would really be in the area of psycholinguistics. I do not think that the aim of linguistics need be either to account for sentences, or to account for the behavior of the native speaker.

8 Some item and arrangements monographs:
MR. POSTAL: It is of course possible to say that linguistics does not have to explain behavior, or that linguistics does not have to explain anything. It is also possible to say that linguistics should describe the structure of utterances, which in this instance means that linguistics should classify utterances. It is even possible to say that linguistics should just gather data and not even bother to classify it. The question is whether anything can be learned about language from proceeding in this way. One can say that all the explanation will be obtained from a non-existent field called “psycholinguistics” which will then contain, it seems to me, everything of interest in this area. But why do this? Why not try and expand the ends of linguistics proper so that it might have some results of conceivable interest.

MR. GARVIN: I would lodge an objection against the adjective ‘non-existent’ before the term ‘psycholinguistics’. It is an existent field, at least to the same extent as any other trend in linguistics. I, personally, am not particularly sold on it, but it does exist. There are people who have published books on it, and debates, discussions, and conferences have been held on it. Without getting into the ontological problem of existence, I think, at least administratively, this is proof of existence.

MR. POSTAL: The question is, does psycholinguistics as a field provide the apparatus for explaining those features of language that we are interested in explaining. You can take Joos’ position that we are not interested in explaining anything, in which case, of course, there is nothing to talk about. One cannot argue with someone who wishes only to classify utterances. People have a right to do what they want. We can ask, however, whether this has the right to be called ‘linguistics’; whether it has the right to claim to be a significant field of inquiry. I say ‘no’ because there is an infinite number of ways of classifying utterances and I see no rational basis for choosing among them if there is not the aim of providing a significant explanation. Furthermore, it seems to be quite generally accepted that a science is supposed to explain something, that it is supposed to provide insight into the subject matter it deals with. How a classification alone provides this insight is not clear.

MR. GARVIN: If in some way the classification that you make is based on information you get from the way speakers behave, it seems you’re finding something out about the way people use language.
MR. POSTAL: But this is not, in fact, the way classifications are generally obtained. Take for instance the principle of complementary distribution. How does this derive from speakers' behavior? It is simply imposed by the linguist on the data. The important questions, ones very seldom raised in the literature, are how the classification is relevant to an explanation and how one classification may be shown to be more relevant than another?

MR. GARVIN: If you say that there is an infinite number of ways of classifying, you should also say that there is an infinite number of ways of explaining, and this then raises the problem of deciding which, if any one, is the best way. You mentioned a criterion of simplicity. This then raises the problem of deciding what is a simple explanation. There is an operational test for this. You can simply decide, that which a moron can understand is simpler than that which a moron can not understand. Therefore the test of linguistic theory will now be intelligibility by morons, by an operational test of simplicity that can be measured by the Binet-Stanford intelligence test. I am sure you don't want that. Therefore, there must be a slightly better criterion for simplicity which entails preferability. This is just to answer the matter of infinite numbers of classification. When you introduce some kind of judgmental criterion, you obviously will then prefer one classification over another, or one explanation over another. And if any theory makes that flat statement that it is indeed the one and only Santa Claus, I should like to see whether or not the beard is genuine and whether there is or is not a pillow standing for the tummy. I would also like to see whether the red suit is dyed fast or whether the dye will fade the moment you sprinkle criticism on it. The burden of proof really is on him who makes a strong claim. I would merely say that the fact of multiplicity of possible classifications is not in and of itself a criterion for rejecting it. You could simply say, "there are many possible classifications, but I prefer classification 'A' for such and such reason," or you could say, "there is a multiplicity of explanations and I prefer explanation 'A' for such and such reasons."

MR. POSTAL: Then the really important issue is what are the reasons for choosing one classification over another. I think that in classificatory linguistics as it has developed in this country, the reasons usually given for classifying in some way follow from some classificatory principle which has been previously accepted by the investigatory. But the real question for linguistic theory is why one should accept this principle in the first place. Presumably what is
meant is the following: if one classifies according to a certain set of principles proposed by some author, one will get the right results. That is, the sentences of the language contain certain elements standing in certain relations to each other, and these procedures are presumably designed to discover them. But we must distinguish carefully the results of some procedure from the justification of these results. Very often we are given the impression that any explicit procedure is its own justification. But clearly the only possible justification for a procedure is the fact that the structural descriptions which it assigns to sentences are in fact correct, that is, match the intuitive knowledge of native speakers in cases where this is clear. I know of no procedures which meet this requirement. This requires a characterization of the notion 'right results'.

MR. GARVIN: What you have just said, however, is not radically different from the thesis which states that if you explain according to the principles of a certain author, you then have the right explanation. I think the problem of selecting from a variety of classifying principles, that which is presumably preferable to another, more correct, or more operationally viable, can be formulated on two grounds: one, whether this classification is based on a reasonable set of assumptions, (and this is obviously debatable); and two, whether this classification will hold up under some kind of an empirical test.

MISS DALLAIRE: We are getting away, I think, from the main point which is that we must be able to generalize and interpolate from given data. Let us say we have 15 items. With item-arrangement you can classify 15 items and you know you have 15 items. If you want item 13 you can get item 13. The generative approach gives us rules by which to generalize. Maybe, with 20 rules, one can take care of 13 or 14 of, let us say, 15 items. One additional rule allows us to take care of all 15 items. We are doing more than just aligning 15 items in a list. By accounting for 15 items with, maybe, 3 rules, we follow a criterion of simplicity. Generative grammars are against collecting data, but we must also be able to generalize the 15 items.

MR. POSTAL: In any corpus there is a finite amount of text of a certain sort. The question is, how a statement about this text characterizes the domain of study, which is the full language, which includes an infinite number of sentences not in this or any other corpus.

This is what is meant by a generative grammar. Such a grammar not only characterizes those facts which have already been
gathered, but ideally also the infinite set not yet observed. It is not at all clear to me how it is possible for a classification to do this. If, however, we are not interested in explanation why not just present the corpus. Why even bother to classify it? If one is interested in explaining linguistic abilities which obviously are not even fractionally exemplified by any finite corpus then we must consider whether the so-called classificatory approach or the generative grammar approach can best extrapolate the finite corpus so that we come to a description of the full set of sentences of the language. This is something which can be discussed only if we agree that such is the correct goal. If, however, we say that anyone can present his corpus, classify it some way, and have something of linguistic interest, then there is no basis for further discussion. Such an approach makes no truth claims and cannot be shown to be either correct or incorrect.

**MR. GARVIN:** There is one point on which we all agree: that a grammar ought to be a good grammar. I believe you'd like to replace the adjective 'good' by the adjective 'generative'. So far you have said that a grammar should be exhaustive, that it should be consistent, and that it should account for all possible sentences or utterances. A good classification should obviously contain sufficient open lists to account for things that are not in the corpus. It should contain enough distributional statements so that they can, by an easy slight of hand, be rewritten into any sort of rules you please, both transformation rules and finite state rules, Yngve rules or Hockett rules. This is always possible given a good description, in the sense of having a sufficient number of lists and a sufficient number of statements about how the elements of each list relate to the elements of the other list. If these statements then are given a particular form they become transformation rules.

**MR. POSTAL:** It is always possible to say that any classification is an unformalized generative grammar. We can then take the classification and try to formalize it in some way. What we can't know, however, is if the classification can be formalized in the right way. Before this can be determined, it must be shown explicitly what sort of formal systems underlie the classification. It is easy to see, I think, that certain things are necessarily excluded. For example, if one insists that the units of language all be derivable by classification of utterances, it follows that no deletion rules can be permitted in the grammar. Therefore, someone who claims that the taxonomic approach is correct implies that deletion rules are not part of language. On the other hand, it is not at all obvious what precise sorts of formal
systems correspond to particular classificatory statements. This is just the trouble with imprecise classificatory grammars. There may be rules, but it is difficult to determine their precise character. Thus we cannot go very far in determining whether they are true or false—that is, whether they can in fact generate natural languages, assign the correct structures to the enumerated sentences, do this in a simple way, etc. Unformalized grammars have all the troubles that imprecision always has. One cannot find out exactly what is being claimed.

MR. GARVIN: That is only true of incompetent classificatory grammars.

MR. POSTAL: No, it is true of any description which provides only a classification. Without showing how the description can be made an explicit generative grammar, one has not shown how it can enumerate sentences and assign them structure. In other words, the classification as such is untestable. It is only some proposed formalization of the classification into an explicit generative device which has testable consequences with respect to sentencehood, structure assignment, etc. To be sure, the intelligent reader has the ability to use an unformalized grammar for sentence description. But this is of no linguistic interest. It merely provides an instance of the exercise of those very linguistic abilities which require explanation. The reason for providing formalized grammars is to present a testable description of those linguistic abilities which permit the child to recognize and understand an infinite set of sentences on the basis of exposure to a small finite set, and which permit the adult to use an incomplete and unformalized grammar to learn a set of sentences which is not in fact explicitly described by this unformalized statement.

MR. HAMP: Perhaps a clarification would help here. The word 'rule' has been used quite frequently, and it has been alleged that in some sense or other, traditional grammars of both sorts have "rules." I am assuming, however, that things that are called "rules" in a generative grammar and things that we have perhaps called "statements" in other grammars are not the same things. I think this point could bear some explanation.

MR. POSTAL: Traditional grammars do have "rules." In fact they have things they explicitly called "rules," as the "rules of agreement" like tense agreements in Latin, etc. Sometimes they are called "laws." In phonology, for example, Sapir spoke of "mechanically rigid
I presume he meant what we call "rules." The trouble with traditional grammars is that it is impossible to determine precisely the types of rules which are intended. We cannot determine the character of the descriptive devices in traditional grammars. Because of their imprecision and lack of formalization, it is also difficult to determine the type of rules which underlie modern taxonomic grammars. It can however be done, even though it has not been done by the authors of such grammars. I should like to illustrate this with an example and show how this relates to the question of classification in grammars. Many writers have claimed that the proper way of arriving at constituents is on the basis of substitution procedures which preserve complete equivalence. That is, two elements will be assigned to the same class if and only if they share every single environment. But now it is clear that this necessarily reduces all rules to the form $A \rightarrow B$, where $A$ is a single symbol and $B$ is some non-null string distinct from $A$. The interesting thing about this form of grammar, which has variously been called Type 2, or context-free phrase structure, is that it is possible to prove that natural languages cannot be enumerated by it. In particular, Mohawk cannot be. That is, it is simply impossible to generate all Mohawk sentences using exclusively such devices. Hockett in *Grammar for the Hearer* recently proposed that constituents be defined in terms of complete substitutibility and thus in fact claimed that the grammars of natural languages were context-free phrase structure devices. This is a rather striking example of what is wrong with not formalizing linguistic descriptions and the apparatus which underlies them. If this is not done, it is not possible to determine exactly what is being claimed about the subject matter. Thus, one may very well be using, as in Hockett's case, a model of language which someone else has shown to be incorrect. The important thing about linguistic rules is then that their properties be precisely and explicitly stated so that given any rule we can tell whether or not it is countenanced by the particular theory of grammar whose truth is in question. Linguistic rules must be formulated in such a way that there is a precise account of how they enumerate an infinite set of sentences.

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9 Edward Sapir, "On the Psychological Reality of Phonemes," in the *Selected Writings of Edward Sapir (in Language Culture and Personality)*, edited by David G. Mandelbaum, University of California Press, 1951, pp. 50-51. The exact quote is: "For the phonetic forms result from the phonologic only by the application of absolutely mechanical phonetic laws of spirantizing."

10 *Proceedings of the 12th Symposium in Applied Mathematics; Providence, R. I., 1961.*
as output and how they automatically assign fully a structural description or grammatical analysis to each sentence.

MR. HAMP: Could you clarify another point? You mentioned that deletion rules are not possible in an item and arrangement grammar. How do deletion rules differ from, for example, morphophonemic zeros if not a question of other kinds of zero allos?

MR. POSTAL: The formal apparatus would presumably be the same.

MR. HAMP: In what way is this not permitted in item and arrangement?

MR. POSTAL: Of course there are item and arrangement grammars which do use zero elements. The question is, whether there are any explicit procedures by which these can be discovered. I claim not.

MR. GARVIN: If you compare two utterances, it is perfectly possible to state that one has fewer elements than the other, and then, that which has fewer elements may be interpreted as having a zero. This to me is possible and it’s not always clearly enough defined, but I don’t see why this cannot be the result of classification. Zero is, after all, the first of the integers in some mathematical systems, and the classification is an extremely simple arithmetic.

MR. POSTAL: Ad hocly, sentence by sentence, we can say that anything contains a zero anywhere we wish. This is clearly not in question. The question which I tried to raise was whether there is a general mechanical procedure for classifying utterances, which has as output, structural descriptions in which some sentences are provided with linguistic elements unrepresented by actual phonetic strings. In particular, are there procedures which can show that John was seen and John was seen by Mary are related by deletion in a way in which John saw the cow and John saw the cow in the field are not. Clearly there are no such procedures at present and I think very probably there can in principle be none. It seems to me that one who maintains that the taxonomic approach can utilize deletions must be making the claim that such procedures exist. The reverse appears to be the case.

MR. GAGE: At the present stage, we don’t seem to have said much that could be considered very clear for distinguishing between two rather ill-defined things. That is, we have said that there is
classification of utterances which gives you lists of elements that might be in sentences; and then there is generative grammar which gives you a list of sentences that might have elements in them. It has also been said that it is very difficult to know about something called 'right results' in classification, which are obviously of some importance; and we have something just about as ill-defined called a 'correct structural description', which a sentence is supposed to have and which obviously must correspond in some way to a right result of classification. We have, furthermore, a statement that traditional taxonomic linguistics is like a library catalog where you could look up things that might build sentences, and obviously, if you look at it the other way, generative grammar is like having some sort of linguistic cook-books with enough recipes in them so that you have a recipe for every sentence the speaker of the language may want to serve up. We're supposed to decide somehow between the merits of library catalogs and cook-books.

MR. POSTAL: But there is an important difference, because a cook-book can be a recursive; that is, it can in principle enumerate an infinite set, whereas a list can never do this.

MR. GARVIN: It can have an etcetera in it.

MR. POSTAL: Of course it is always possible to be imprecise.

MR. GARVIN: It's not an imprecise notion in a large number of mathematical systems. I understand even the strictest logicians admit the existence of the notion of an open set. After all, you can always say, "my daddy's logician is better than your daddy's logician," which is what linguists apparently seem to do.

MR. POSTAL: The important point is how a finite list can explain the speaker's ability to produce and understand an infinite set of sentences. The claim that this can be done by saying "etcetera" is not very illuminating.

MR. GARVIN: It's at least as illuminating as the lists which are at the beginning of transform grammars. Householder gave a paper on lists where he said that the beginning of transform grammars is lists. First you have lists, next you select elements and then put them into the rules. These are lists, and the only difference is that Householder's lists are followed by statements that explicitly are call-

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ed rules and are alleged to have a more rigorous formal structure than anybody else's rules. I say alleged to, because there are obviously ways in which it could be shown that they are not completely rigorous. The rules of transformation grammar are alleged to be not at all \textit{ad hoc}. But it would be very difficult for a person who is not a native speaker of English, armed with only a set of kernels and a set of transformation rules and nothing else, to produce all and only the sentences of English. It could be said that this is because nobody has yet written a complete grammar by transformations, but the same argument could be brought in favor of any other approach, saying that if this approach were carried to completion we would then have an entire explanation of everything. This can be asserted of anything if you carried on in the same vein, but it doesn't seem to me that this is convincing.

\textit{MR. POSTAL}: There are two different questions here. One is the question of whether the precise formal systems of generative rules which have been proposed for English and other languages can be completed, that is, whether enough further rules of this type can be added to provide a correct description of the full language. The other is the question of whether taxonomic grammars can be "completed," but here "completed" means formalized, put into precise enough form to be capable of an empirical test. The question of "completing" grammatical descriptions of languages in terms of describing all sentences cannot even arise in the case of non-explicit taxonomic grammars. The problem with these grammars is not that they don't describe \textit{all} sentences, but that they don't describe \textit{any} sentences, if "describe" is taken seriously.

\textit{MR. GARVIN}: But all notions that we work with in linguistics, in those terms, are by definition unclear, because they are not dealing with the arbitrarily defined systems of logic and mathematics. In mathematics you have eminently precise notions, because any natural object that does not fit these notions is not usable. In linguistics you have only what is in the data and the data are not quite that precise. Therefore you have to have a certain fuzzy edge somewhere and you can either mask it by pretending that it does not exist or you can admit it. I think that a set of lists with a set of distribution rules can account for all the sentences of the language, provided that some of the lists are open and some of them are closed. Any grammar in the item and arrangement style can do this. You could then presumably take such a grammar and take any random text, as tape-recorded or looked up in a book, and find the correct
description in the particular portion of the grammar, provided the grammar is complete enough. If it is not complete it will not lay claim to completeness. If you reverse this, you can call it ‘generation’. I don’t see any great difference between a generative grammar and a recognition grammar from the stand-point of the rules. It all depends on how you apply the rules.

**MR. POSTAL:** The point is that taxonomic grammars do not have explicit rules.

**MR. GARVIN:** That’s a statement which remains to be proven.

**MR. HAMP:** This brings us back to the question of a little while ago. I am not sure whether it has been made clear in what way generative grammar rules are or are not comparable to statements we have seen in other kinds of grammar. It seems to me this argument will not proceed very far unless we can discriminate something along this line.

**MR. POSTAL:** Let me try and characterize what a typical descriptive grammar consists of today. First it will contain a list of all phonemes with a statement that such and such allophones occur in such and such environments. Then there will be an incomplete list of morphemes with some way of stating the arrangement of these in higher order elements. This information will be given by morpheme order charts, substitution classes of morphemes, etc. Obviously such descriptions cannot say anything about an infinite set of sentences unless there is some way that one list can be embedded in another, that is, unless there is explicit information given about how noun phrases can be embedded in verb phrases, verb phrases in noun phrases, etc. But unless the grammar says precisely how these can be embedded, it does not explicitly describe the recursive features of the language. It must tell us, in other words, whether ‘a’ can be embedded in ‘c’ when ‘d’ follows or when ‘c’ precedes, or when ‘a’ contains a possessive phrase, etc. It is this type of information which is never explicitly provided in modern grammars. And it is, I think, only the fact that there have been few attempts to actually describe the full range of syntactic phenomena in taxonomic terms which leads to the gratuitous assumption that this type of description can be “completed.”

**MR. GARVIN:** I agree. I think this is a lack of technical competence rather than anything else, or perhaps a lack of completeness.
MISS DALLAIRE: I would like to state the difference between a formal rule and a formalized rule. Let us take Harris, for example. He states very rigorous formal rules using mathematics and all kinds of formulae, but he does not include content, nor does he tell us when to use his rules. A formalized rule, on the other hand, does include content. It shows when and how to use rules.

MR. POSTAL: In a theory which contains formal rules all the properties of these have been explicitly described in advance. It is thus possible, in principle, to determine whether certain kinds of phenomena can or cannot be described with such rules and if so, in what way, with what degree of complexity, with what structural descriptions, etc.

MR. GARVIN: In that case I can make a flat claim that since I have described in detail a program for the automatic classification of English words for a computer, which the people in the computer field consider reasonable, I then have a set of formal rules for classification.

MR. POSTAL: No doubt! The question is, would that provide an output of any interest? No one doubts that it is possible to state all sorts of completely formal procedures; the one I gave before, classifying every three segments, is a formal procedure. But one has to show, given a set of formal procedures, that they have an output of some interest.

MR. GARVIN: The aim of linguistics is to have an output of some interest, and everything hinges on how you define what is interesting. This is the area where it is hardest to reach any agreement whatsoever, because, obviously, since I have one orientation and you have another, to me your output will be uninteresting and mine will be to you.

MR. POSTAL: You are not stating what particular criteria you impose on an output. That is, you say you want to classify English words, but not the end that you wish to accomplish by this.

MR. GARVIN: I pursue an old fashioned end, namely to detect certain hitherto unknown properties.

MR. POSTAL: Several questions have been mixed up. One is the question of a rule in the sense of a procedure for classifying utterances. This is not the sense in which generative grammars have rules. The rules of such grammars are not devices for analyzing
texts, that is, for taking sentences as input and providing the structural descriptions of these as output. The rules of generative grammars are designed to enumerate sentences and their associated grammatical descriptions and thus to define or characterize the language from which texts may be drawn. A grammar in this sense provides a mechanical enumeration of a set of structured objects, namely the sentences, and thus provides a partial explanation of the speaker's ability to distinguish sentence from non-sentence and to implicitly recognize the structure of sentences. I do not see how a classification of utterances can do this.

MR. GARVIN: It's very easy to show you. In the semi-annual report No. 2 of Librascope Division of General Precision, Inc. 12 is a detailed discussion of the operation of a computer program called The Autobeatnik. This program not only generates English sentences, but these sentences sometimes rhyme and they have certain particular properties. They were even read in a coffee-shop in Long Beach, and were considered very "cool." If you want it as a test, whether or not you can have a set of rules which by means of a computer will produce English sentences, I will say "yes you can." I will also say, with the authority of output behind me, that you can order it by mail and that there need not be any transformation rules. Furthermore, Yngve has programmed a random sentence generator, 13 which I understand does not use transformations. His sentences are much less amusing than the Librascope sentences, but I think this a matter of personalities rather than competence.

MR. POSTAL: There are two questions which need to be distinguished. One is whether a computer can enumerate some sentences, given some rules. Of course it can, and these can be rules of practically any sort. If the output set of sentences is to be finite, the "rules" can simply consist of the sentences. The computer will then give out sentences. The other question is whether a computer can be programmed in such a way as to give out all English sentences and only English sentences and at the same time assign to each sentence a structural description which can provide insight into the way that sentence is used and understood. In this case, the computer program would be nothing less than a grammar of the language.

Note, incidentally, that the fact that a computer can enumerate sentences when given a program containing a grammar is completely trivial. The non-trivial element is the grammar itself. From the point of view just discussed, it is easy to immediately see a crucial objection to any taxonomic constructional or phrase structure grammar. These cannot properly describe so called "co-ordinate constructions" like those in English with "and." In these there are strings which can become endlessly long. In order for a phrase structure grammar to describe this, it must provide incorrect subordinate structures. That is, to describe a and b and c in a recursive way, it must provide a tree description like:

```
NOMINAL
/   \
/     \
NOMINAL NOMINAL
/   \
/     \
NOMINAL NOMINAL
/   \
/     \
a   and b and c
```

where the correct tree is:

```
NOMINAL
/   \
/     \
NOMINAL NOMINAL
/   \
/     \
NOMINAL NOMINAL
/   \
/     \
a   and b and c
```

Phrase structure description which is simply a formalization of the taxonomic approach necessarily provides subordinate trees for co-ordinate constructions. This is the only way in which such a grammar can provide for co-ordinate strings of infinite length. The correct co-ordinate trees can only be provided in such grammars by giving a separate rule for each length. But this implies an infinite number of rules.

**MR. GARVIN:** But there are inadequacies in transformation grammar too.

**MR. POSTAL:** This is not a question of inadequacies. This is a matter of whether or not we will use precise or imprecise notions. A priori the term 'phrase structure grammar' is unclear. If we are going to seriously discuss phrase structure we must specify precisely
what sort of rules we mean. Chomsky has discussed a type of grammatical system which he referred to as phrase structure grammars. These consisted of rules which expanded a single symbol into a non-null string of symbols distinct from the original. He and others then considered various sub-types of this general type depending on whether contexts were allowed, how ordering was utilized, whether permutations were permitted, etc. About these various systems we can argue intelligently because we know exactly what form of descriptive apparatus is in question. When I claimed that phrase structure grammars cannot enumerate the correct structures for coordinate constructions, I meant phrase structure in this sense. If someone wishes to claim that there is a ‘phrase structure grammar’ which can enumerate these structures correctly, he must of course explicitly present such a grammar so that this claim may be tested.

MR. GARVIN: It is not incumbent upon me to do so. It’s incumbent upon Chomsky to examine not only those things which he knows beforehand will prove his case but also those things which may turn out to be reasonable counter examples. Proof is a mathematical term, and if it is not, it is meaningless.

MR. POSTAL: Does it have anything to do with linguistics?

MR. GARVIN: It obviously doesn’t in my opinion, because we don’t have enough factual information about any language to be able to build mathematical proof, and any mathematician will tell you that a proof is an extremely conclusive, incontrovertible-type statement. There are proofs that were stated by Euclid, which to this day are valid proofs. I don’t think that you can do anything but assert the conclusiveness of a proof in linguistics. Furthermore I don’t think that that assertion is accepted by anybody in the field of linguistics, except by those who agree with it, whereas nobody in mathematics disagrees with the conclusiveness of certain proofs. I question whether in a field where we know relatively little with the precision required for real proof, the term ‘proof’ is meaningful at all. We obviously don’t know enough about all the languages of the world, nor do we know enough about all the possible approaches to phrase structure grammar to be able to say that we have proof.

MR. POSTAL: It seems that there is an insistence here that the term ‘phrase structure’ be used in a maximally unclear sense. It is impossible to come to any conclusion at all about ‘phrase structure’ in this sense, because it has no determinable reference.
MR. GARVIN: That's a perfectly valid point to arrive at, because there is no 'one and only' phrase structure grammar.

MR. POSTAL: To discuss any of them we must have a precise characterization of the form of rules they contain, the type of structures they can describe, etc. Only in this way can we determine their empirical adequacy.

MR. GARVIN: The only way to answer that question is to generalize phrase structure grammar in such a way that you can make statements about any conceivable form of phrase structure grammar. Otherwise you don't have proof; you merely have an opinion.

MR. POSTAL: But this is absurd. This is to assume that the notion of 'phrase structure' is clear when we agreed above that it was not, that is, that there were many different kinds of grammar called 'phrase structure'.

MR. GARVIN: Maybe it does not intend to be clear on the same level of abstraction that Chomsky is talking about, because it is not possible on that level of abstraction to achieve both clarity and relevance.

MR. POSTAL: This is an empirical claim.

MR. GARVIN: It is a very wide one. But we are not dealing with logic, we are dealing with natural language. If we were logicians we would have to answer such questions as logical consistency. But we are not. In linguistics you have to answer questions about natural language, and it is not that logical.

MR. POSTAL: This is, I think, simply a fallacy. It does not follow from the assumption that language is 'unclear', or 'messy', whatever interpretations you may give to these terms, that our descriptions of language must have these properties. Indeed, just in so far as descriptions are unclear we cannot determine at all what properties languages do have, because unclear descriptions are just those which do not make precise, testable claims about their subject matter. And whether a phenomenon is 'messy', 'irregular', etc., often depends on the way it is described. For example, from the ordinary constructional or phrase structure point of view, the behavior of be and have in English is quite irregular. But from the point of view of a transformational grammar, the description of these is quite simple and regular. It seems that the claim is being made here that all notions be kept maximally unclear, maximally inexplicit. I take the opposite
position because I wish to be able to determine the truth value of claims about language and for this purpose it is necessary to know precisely what these claims are.

**MR. GAGE:** I should like to ask you to defend your generative notion. You said you thought generative grammar is the first one that might tell something about the ability of speakers to produce utterances and, presumably, the hearers to hear utterances. Is it not quite clear to me why you can think that you can make this claim. There may be several recipe books that give you approximately the same product. So, when you say that you obtain the output you want, how do you know this has anything to do with reality? How do you know that the simplest assignment of capacitance inductance, and resistance that you can make matches what is, in fact, in the black box once you open it?

**MR. POSTAL:** I am not sure that I understand you correctly. Do you want to know how I know that the rules are right?

**MR. GAGE:** As far as I can see, the only thing that has been said about rules is that they follow a simple form, that the whole recipe should read in a simple way, and that the result should be in accord with the language of which it purports to be a grammar. Maybe the form of the rules should include the ordering imposed upon them. You obtain the output that matches what you wanted it to match. Then, you find that this ordering is as simple as any other that you may have on hand and has been chosen according to preconceived notions of what the results should be. What makes you think that this procedure has something to do with the way users of a language actually do use the language?

**MR. POSTAL:** We notice from a large set of clear cases that people can tell sentence from non-sentence. That is, there is a clear set of utterances that speakers of a language can identify as sentences. Another set they can identify as non-sentences. They may have difficulty with a third group, which is in between sentence and non-sentence. What is demanded of a grammar is that it have, as output, the set of clear sentences, none of the obvious non-sentences, and that it makes decisions about the doubtful ones.

**MR. GARVIN:** Of course, you realize that there are speakers of languages, who have never even heard of the term 'sentence' and they still talk.

**MR. POSTAL:** I have worked with speakers of Mohawk who have, I suppose, never heard of the term 'sentence' and I found them
able to make the distinction between sentence and non-sentence.

MR. GARVIN: Are they able to tell you anything more beyond that it is not Mohawk?

MR. POSTAL: That is all I require for this purpose.

MR. GARVIN: But then that is a distinction between Mohawk and non-Mohawk utterances. It does not define sentences. 'Sentence' is a rather concrete notion. In English spelling, you put a period at the end of a 'sentence'. And when you take down a language such as Mohawk, you must have a place to put your periods. Do you always know where to put your periods? Does your informant always know?

MR. POSTAL: The informant is able to tell whether the string is a grammatical utterance, but there are cases where he cannot, or where he changes his mind.

MR. GARVIN: So then analysis is as difficult for you as for everybody else. If you don't have a clear-cut informant response that will tell you whether this is a sentence, then you can never judge your outcome. What do you do with the unclear cases? Do you pretend they don't exist?

MR. POSTAL: On the contrary, we try to explain them. The grammar must be determinate, that is, it must make an explicit grammaticalness claim about each object which it enumerates. It must say whether or not that object is a properly formed sentence. But the general theory of linguistic structure should be powerful enough to tell us why there should be unclear cases. One answer is that there is a limitation on memory. It may be that in the course of derivations of unclear cases, many complex rules are involved and the informant has difficulty in tracing the path of derivation.

MR. GARVIN: You mean that you forgot a rule?

MR. POSTAL: It may also be that the language is changing to a certain extent; or it may be that there are several informants who disagree because they speak slightly different idiolects. It seems to me that the fact that there are unclear cases does not in any way negate the fact that there are clear cases and many millions of them. What we must say is simply that unclear cases are a poor choice of crucial cases to decide between alternative grammars and more seriously alternative theories of grammar.
MR. GARVIN: I agree, but you have to account for all your data or else you are back to the old saying in language data processing: "A well formed sentence is one which our program will handle!"

MR. POSTAL: This is not a serious criticism as long as we insist on correct treatment of the clear cases. Furthermore, I should note that attempts at systematic grammar construction have led to the discovery that many of the unclear cases cluster about violations of certain low level selection restrictions such as animate-inanimate, etc. For example, in English if we place an inanimate noun like 'metal' in a context like, "— loves John," we may or may not find that this will be accepted. But it is important to note that just such restrictions appear to have certain formal properties in common, chiefly the fact that they cannot be naturally stated with any devices now known. Thus generative grammar itself begins to provide criteria for understanding unclear cases.

MR. GARVIN: I thought you didn't classify in generative grammar. Yet both you and Miss Dallaire said that generative grammar classifies, and you drew upon such classes as concrete and abstract nouns, which to me are extremely precise notions.

MR. POSTAL: No one ever said that generative grammar was not interested in classification. What was said is that no one claimed that generative grammars per se could be discovered by classifying parts of utterances. Certainly generative grammars impose a classification on the sentences.

MR. GARVIN: Then one could make the claim that if you know how to classify utterances you might wind up with a grammar, provided that you do this right. And 'right' is that which is empirically verifiable and which produces an output that you like.

MR. POSTAL: How is the list verifiable?

MR. GARVIN: By going through the text and deciding whether or not upon processing the data you do or don't add things to the list. If you keep on adding things to the list, you know it's open. If the list closes after a certain amount of processing, it is closed.

MR. POSTAL: What about sentences? They will never close that list.

MR. GARVIN: Who said that you are going to have a list of sentences? You don't have to. You can have a list of elements making up sentences, or a list of rules or combinations; you can decide

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which are restricted, and which are unrestricted inventories. And then you must go over a body of text and see whether your rules are applicable, and if they are not, modify them. I am sure transformationists modify their rules. I understand there is more than one project in this country working on the transform grammar of English and they don’t all agree. Why should other people agree if they don’t?

MR. POSTAL: You are now talking about rules. What kind of rules are these?

MR. GARVIN: They are distributional statements and other kinds of statements which, if you so desire, you may call “rules.” And if to you “rules” is a prestige term, I hereby declare them rules. They are obviously not the same rules as you have in mathematics, but that is because we are not mathematicians.

MR. HAMP: We will now discuss the general question of the transformation theory. This will be an attempt to deal with questions of the application or factual correctness of transformation. Miss Dallaire will present the subject.

MISS DALLAIRE: I shall try to show how we get a derived “tree” and a derived “string” in the transformation of generative grammar. In the last few years there has been much intuitive discussion about generative grammars in general, and transformation generative grammars in particular. Concerning the latter, Professor Halle’s work provides the main source of information on the morphophone-mic and phonetic levels, and Professor Chomsky’s Syntactic Structures, provides us with information on the syntactic level. Today I would like to indulge in specific talk and not general talk.

The diagram on p. 30-31 is divided into three sections—blue, red, and green. The blue section is the C-terminal string which will be transformed. The red section is the part that is transformed. And the green section contains the tools needed to transform the blue section into the red section.

To transform the C-terminal string we will use a “simple” T transformation. (A thing to be transformed can also be complex, as when two sentences are put together and transformed to get a third sentence. In this case we use a “generalized” T transformation.)

The red part transformed could have already been transformed by another transformation, that is, it does not have to be simple. The blue part to be transformed, which is called the “terminal string,” is presented in the form of a “sequence.” It is known as the “proper analysis sequence.” It has a fixed number of “terms,” in this case three—X₁, X₂, X₃. The red transformed part also has a “proper analysis sequence.” It has, as well, a fixed number of “terms,” which is the same number as the “terms” present in the blue “sequence.” We have, then, three blue “terms” and three red “terms.” This is an important point to remember, because the “terms,” must be of a fixed number, and the same number for both “sequences.” These “sequences” are called “proper analyses of a terminal string.”

On the last line of the blue section and the last line of the red sections there are “strings.” The blue “sequence” has a blue “string” and the red “sequence” has a red “string.” In the “sequence,” commas (, , , ) unite the “terms;” in the “string” this is done by arches ("""). As was mentioned before, in the “sequence” there must be a fixed number of “terms.” In the “string” this is not obligatory (Here we have an important distinction, because “sequence” is not equatable to “string.”). On the last line of the blue section and the last line of the red section are found the “strings” which correspond to red and blue “sequences.” The fact that these “strings” are connected with arches and not with commas is not simply a notational convention. It has structural importance as well. Whereas the “terms” connected with commas must be of a fixed and same number for both sides, the “terms” connected with arches need not be. The “terms” connected with arches, i.e., the “strings” are “concatenated.” In the procedure which follows, we will not be working on these “strings,” but on a constituent structure known as a “tree,” which is here represented by the “sequence.”

I will now illustrate how capital T transformation (there can also be a small t transformation) works on a blue “tree” or “sequence.” A T transformation is composed of one or more elementary transformations. The green section of the diagram gives the elementary transformations which will be used in order to obtain the T transformation. The T transformation is a concatenation of elementary transformations, in other words, transformations connected with arches. In the green section we have two types: singular elementary transformations, and compounded elementary transformations. The asterisk next to the compounded elementary transformations refers to the asterisk at the bottom of the diagram which shows that there
HOW TO GET A DERIVED TREE AND A DERIVED STRING IN A TRANSFORMATIONAL GENERATIVE GRAMMAR

**BLUE TABLE**

<table>
<thead>
<tr>
<th>CP TA</th>
<th>1</th>
<th>2</th>
<th>3</th>
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<tbody>
<tr>
<td></td>
<td>X</td>
<td>X</td>
<td>X</td>
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**Proper Analysis Sequence**

<table>
<thead>
<tr>
<th>CP TA</th>
<th>1</th>
<th>2</th>
<th>3</th>
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<tr>
<td></td>
<td>X</td>
<td>#</td>
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**C-terminal String**

<table>
<thead>
<tr>
<th>CP TA</th>
<th>1</th>
<th>2</th>
<th>3</th>
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<tbody>
<tr>
<td></td>
<td>X</td>
<td>X</td>
<td>X</td>
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**DERIVED**

<table>
<thead>
<tr>
<th></th>
<th>U</th>
<th>U(^a)X</th>
<th>X</th>
</tr>
</thead>
<tbody>
<tr>
<td>CP TA</td>
<td>1</td>
<td>2</td>
<td>3</td>
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<table>
<thead>
<tr>
<th></th>
<th>U (^a)X = U (_1)</th>
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<tr>
<td>CP TA</td>
<td>1</td>
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Transformed String

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<tr>
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<th>X</th>
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<tr>
<td>CP TA</td>
<td>2</td>
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**RED TABLE**

<table>
<thead>
<tr>
<th></th>
<th>U</th>
<th>U(^a)X</th>
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<tbody>
<tr>
<td>CP TA</td>
<td>1</td>
<td>2</td>
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</table>

Proper Analysis Sequence

<table>
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<th>U</th>
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<tbody>
<tr>
<td>CP TA</td>
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<tr>
<th></th>
<th>U (^a)</th>
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<tr>
<td>CP TA</td>
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<tr>
<th></th>
<th>U (^a)X</th>
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<tr>
<td>CP TA</td>
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<th>X</th>
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<tbody>
<tr>
<td>CP TA</td>
<td>3</td>
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# Transformations—Simple or/and Generalized

## Underlying Elementary Transformations

<table>
<thead>
<tr>
<th></th>
<th>( d )</th>
<th>( b )</th>
<th>( \pi )</th>
<th>( n )</th>
<th>( i )</th>
<th>( d(n) )</th>
<th>( d(n)^2 )</th>
</tr>
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<tbody>
<tr>
<td><strong>Singular</strong></td>
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</tr>
<tr>
<td>Elementary</td>
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<tr>
<td>Transformations</td>
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<td></td>
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<tr>
<td></td>
<td>deformation</td>
<td>rearrangement</td>
<td></td>
<td>affects CP=attaches like ( d )</td>
<td>affects CP=attaches like ( d )</td>
<td>SUBSTITUTION</td>
<td></td>
</tr>
<tr>
<td></td>
<td>affects CP only—either 1 or 2</td>
<td>affects TA only</td>
<td></td>
<td>affects CP=attaches like ( d )</td>
<td>affects CP=attaches like ( d )</td>
<td>d affects CP: deletes only* here</td>
<td></td>
</tr>
<tr>
<td></td>
<td>1. deletes ( \text{U}^*X=\text{U} )</td>
<td>1. permutation</td>
<td></td>
<td>affects CP=attaches like ( d )</td>
<td>affects CP=attaches like ( d )</td>
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<td></td>
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<tr>
<td></td>
<td>2. attaches ** \ldots</td>
<td>2. non-permutation*</td>
<td></td>
<td>affects CP=attaches like ( d )</td>
<td>affects CP=attaches like ( d )</td>
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<td></td>
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<tr>
<td><strong>Compounded</strong></td>
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*restrictions on set of elementary transformations in order to secure UNIQUENESS OF ROOT

CP = Constant Part

TA = Term Arrangement
are restrictions imposed by the theory of the language. Each transformation affects either a "constant part" (CP) or a "term arrangement part" (TA) of the blue and red sections. For example, in the blue section we have X's and numbers under the X's. The X's are the "constant parts," and the numbers are the "term arrangement parts." When we have to rearrange parts, we rearrange the numbers, or the "term arrangement part." Each elementary transformation will affect either the X's or the numbers, or both.

Each "term" of the blue "sequence" is taken care of by an elementary transformation, either singular or compounded. If there are three "terms" on the left-hand side "sequence," there will be three elementary transformations underlying the \( T \) transformation. The number of "terms" in the "proper analysis sequence" is important for the number of elementary transformations involved, because each blue "term" has what we call a "root term" on the red side. The notion of "root" plays a central role in the discussion of the derived constituent structure, or the derived "tree," or sentence. Each "term" has an outer structure, or "node" and an inner structure, or "content" of that "node." A "root" refers only to the outer structure of a red "term." In other words, a "root" refers only to the "tree node" which this red "term" occupies.

Let me illustrate this. We will take the "content" of blue \( X_1 \) and put it in position 2 of the red \( X \), without changing position 2. The transfer is made by trying to get a red "tree" or "derived constituent analysis." In order to work on "root" 2, which is the red "term" 2, we will need the compound transformation \( d(n) \). Notice that we call this a "substitution." The \( d \) part will affect the "constant part" only, or the blue \( X \) only. The \( n \) transformation will affect both the "constant part" and the "term arrangement part." Here we are dealing with a "generalized" transformation, which means that we have more than two sentences, or parts of sentences, on the blue side. Therefore, we have to place sentence boundaries, and this is done by placing a \( \# \) before \( X_1 \), another after \( X_1 \), and another after \( X_3 \). This means that the first part is one "term," and the second part is divided into two "terms," or two "branches" of a "tree."

To have the "contents" of the first blue \( X \) in the position of the second red \( X \), or the internal structure of the blue \( X \) and the external structure of the red \( X \), we will have to work in three stages. **STEP ONE**: Elementary transformation \( d \) will affect the "constant part" of the second blue \( X \), by deleting the "constant part" while keeping the "term arrangement part" intact. This is represented by
We have kept position 2, but it no longer has a "content." **STEP TWO:** Elementary transformation \( n \) will affect the "constant part" of the second red "term" \( X \) now represented by \( U_2 \); it will attach "something" to it. The second red "term" position now appears as \( U_2^* \). Position 2 is now empty. In the next step we will concatenate it with the "content" of \( X_1 \), to get the "content" of \( X_1 \) in red position. **STEP THREE:** Elementary transformation \( n \) will affect the "term arrangement part" of the first blue "term" by moving \( X \) from first position to second position on the red side. This first blue "term" is the "something" referred to in step two. The second red position now looks like this \( U_2^*X_1 \). The transfer is now effected; we have the internal structure of blue \( X_1 \), and the external structure of the second red position. The second red "term," or "derived term" in second position, is now characterized by elementary compound transformation \( d(n) \). If we want to complete a "generalized" transformation, we have to attend to the first and second blue "term." Let us choose to delete the "constant part" of the first blue "term" by using elementary transformation \( d \). We now have, thanks to our elementary transformations, a derived "tree," or red "derived constituent structure." \( (U_1, U_2^*X_1, X_3) \). This is the same thing as the "proper analysis sequence." We do not have a "string" yet, we only have a "tree." Finally, we get \( X^*X \), which is our "string."

To make this more concrete, let us suppose that the first blue "term" is: "that he is sick." The second blue "term" is "it," and the third blue "term" is "is obvious." By following the above procedure we get, "that he is sick is obvious."

**MR. GAGE:** This argument for generative grammar is like arguing that Ptolemy was a better astronomer than Ticho Brache, because Ptolemy used all sorts of epicycles to explain the movements of planets, while Ticho Brache just went out and measured where stars were. And this was not very interesting or powerful.

**MR. POSTAL:** It is difficult to appreciate the complicated formalism involved here, unless one takes seriously the goal that a grammar should be a specified formal device which can not only enumerate the correct set of sentences, but automatically provide each with its correct grammatical analysis on all levels. What Miss Dellaire has tried to show is how in one particular case transformational rules can assign a certain structure. She thus tried to illustrate
the claim that transformational rules are devices which can assign structures to sentences. The case used is interesting because it is a substitution transformation. Substitution transformation can, in fact, assign the correct structure to indefinitely long co-ordinate strings, something impossible for phrase structure rules. But it is difficult to appreciate why anyone would bother with such complicated formal apparatus unless one agrees that a grammar should be an explicit set of rules which make a certain claim about each sentence. Since transformations must be iterated and since transformations apply to whole phrase structure trees rather than strings, it follows that the output of a transformation must itself be a tree rather than simply a string. This must be the case also because we know that sentences have a certain derived constituent structure.

*MR. GARVIN:* Is there a countable number of transformation rules at present? Or are they all designed to fit the particular debate that is in progress?

*MR. POSTAL:* The term transformation has been used in two different senses which Chomsky has distinguished in an unpublished work. In the most technical sense there is an infinite number of transformations. But this does not matter, because all the rules in actual descriptions are technically 'families of transformations' and there is always a finite number of these. A 'family of transformations' is a set of transformations, meeting some specified formal property. When it is claimed that the grammar is finite, what is meant, technically, is that there is a finite number of families.

*MR. GARVIN:* But then you are right back to the business of open lists, because instead of having infinite lists of elements, you now have infinite lists of transformations.

*MR. POSTAL:* No. This is a mistake. The actual rules of the grammar are families of transformations of which there is a finite number. The set of transformations associated with a family is mechanically determined by the finite statement of the family.

*MR. GARVIN:* But in a list the actual defining criterion is finite, and just as the family of transformations is finite, so the class-defining criterion in a good phrase-structure grammar, or IC grammar, or item and arrangement grammar is finite. If it isn't, it is a matter of a lack of competence rather than an inherent weakness of the theory.

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You can say, for instance, that nouns in English are those gadgets which end in an 's' in the plural, and I am deliberately phrasing it very vaguely to allow for precision according to the tastes of the individual proponent. This is a finite defining criterion, and with a couple of allomorphs thrown in, it defines an infinite number of items. You have said that you have a finite set of rules which define a family of transformations in which there is an infinite number of particular transformations. I don't see how much better this is than to have a class criterion with an infinite number of items in a class. I don't see how much less finite the latter is than the former.

**MR. POSTAL:** No one ever claimed that a classification couldn't be finite.

**MR. GARVIN:** I think you did.

**MR. POSTAL:** What I said is that one has to show how a finite classification can enumerate an infinite set. This can only be done if one provides for some embedding. The usual way to provide for embedding in procedural approaches is on some sort of substitution basis. But here it is necessary to make precise just how one can substitute. There is only one way of doing this that has ever been presented in literature, and that is to make it precise by 100% substitutibility. This can be proven wrong.

**MR. GARVIN:** I agree. The only claim that I want to make is, that to me, a finite number of families, each of which contains an infinite number of items, each called an individual transformation, is no less finite, really, than a finite number of classes.

On the matter of substitutions, if you say that they are poorly defined, I agree with you. It has yet to be shown that transformations are much better defined than other linguistic concepts. It may be necessary in a transformational statement to list an infinite number of certain characteristics in order to be sure that in a particular language, under particular circumstances, the little x's and y's tell us really what piece of the data was assigned to which x, or y.

**MR. POSTAL:** Again there is a misstatement here. The fact that there is an infinite number of transformations in one sense (the most technical sense) is of no more interest than the infinitude of the set of sentences. The rules of the grammar are not transformations in this sense; they are families of transformations whose finitely stated properties determine the set of transformations in the narrower
sense. There is no listing of an infinite number of characteristics. Indeed, this notion is self-contradictory.

MR. GARVIN: I agree. But the real problem then, is, how do you assign a particular datum to a given rule? Suppose I were not interested in the sentence, 'that he is sick is obvious', but rather I picked a sentence at random from the Washington Post and Times Herald, and asked somebody to give it its transformational history. How would he do it?

MR. POSTAL: This is another matter entirely. You now raise the question of how, given an arbitrary sentence, its structure may be determined. A transformational grammar as such does not answer this question.

MR. GARVIN: What question does it answer? Given a sentence whose structure you already know, you then can determine the same structure?

MR. POSTAL: We should distinguish three sorts of devices. One is a grammar which enumerates sentences and their structural descriptions. Another is a 'recognition routine’ which takes as input arbitrary sentences and gives as output their structural descriptions. The third is a decision procedure which takes as input arbitrary utterances and gives as output a decision as to whether they are sentences or not, while providing a structural description for the sentences. I cannot tell whether you are speaking of a recognition routine or a decision procedure in this sense. A generative grammar by itself is certainly neither of these. But I think that the following claim can be made: no rational decision procedure or recognition routine can be based on anything except the right generative grammar, because what recognition routine must do, is to provide you with the correct structure of the input. This is a notion defined by the generative grammar. For example, if it is decided that no sentences have any structure beyond, say, being concatenations of words, this will lead to a quite different recognition routine and decision procedure than a decision that phrase-structure trees are necessary. The notions of recognition routine and decision procedure are, it seems to me, quite subsidiary to the notion of generative grammar. They can't be seriously studied unless you study generative grammar.

MR. GARVIN: I would disagree for one very serious reason. One way of verifying the validity of a theory is by writing a recogni-
tion routine based on this allegedly correct, and allegedly only correct grammar, and then by seeing whether it indeed does "recognize." I deliberately mentioned the Washington Post and Times Herald, because to a large number of speakers of English, it contains grammatical sentences.

**Mr. Postal:** Most of the sentences would not be sentences at all.

**Mr. Garvin:** That is a preposterous claim! On behalf of the Washington Post I protest! This is a very common brand of English.

**Mr. Postal:** I would say it is a very common brand of non-English, that is, not complete English sentences.

**Mr. Garvin:** Then, of course, you are in the marvelous position where whenever you can't analyze something you simply say, "this is not English."

**Mr. Postal:** Again you ignore the fact that there are clear cases, cases on which both you and I and everyone else agree. The only rational course is to choose among grammars on the basis of the clear cases, and to let the status of unclear ones be determined by the character of the grammar itself. Incidentally, this provides even more support for the claim that the grammar must be precise. If it is not, it will of course make no decisions for unclear cases.

**Mr. Hamp:** Could we perhaps assume that this brand of non-English is something else which, for example, might have its own transformational grammar?

**Mr. Postal:** No, I don't think we want to assume that at all. I think we ought to distinguish here between language and speaking. This is a distinction that De Saussure made, and which linguists have traditionally followed from the beginning. A linguist works with edited texts; he works with sentences, not with utterances.

**Mr. Garvin:** But if the linguist is permitted to edit his own text, then obviously he will only accept those sentences that fit his scheme.

**Mr. Postal:** Suppose we take for example the opposite position, that the linguist actually studies utterances, that is, that he actually takes the approach of a tape recorder. I think it would be fair to ask, which particular utterances does he study? Is it to be all
possible utterances? If this is the case, it is quite easy to see that linguistics is at an end, because it is easy to write down right now the grammar that would enumerate every possible utterance. This is simply the set of all possible combinations of morphemes. This is exactly what a possible utterance is. On the other hand, if the linguist studies the actual utterances in some corpus, this would be of no interest, because it is an arbitrary corpus and we are interested in the language. The only possible way to succeed here is to use the grammatical judgments of the native speaker, because he knows clear cases of “sentence” and “non-sentence.”

MR. GARVIN: I would just like to say that a corpus is as much of a representative sample of all possible utterances as a segment of a population is a sample of the total population. With both, the problem of sampling techniques arises. You can reject a corpus as being non-representative. You can also use native informants' judgments of a corpus to establish its representativeness. If you are going to use the native informant to test whether each individual sentence is grammatical, why not use the informant to ascertain whether a large corpus is representative of his language, and then use the corpus as being a good sample? If it is big enough, it will be a good enough sample. If it is too small, you will become aware of it.

MR. POSTAL: This is not a question of representativeness of the sample. The question is how you can judge when the grammar produces one sentence not in the corpus. How can you decide whether or not the grammar should produce such a sentence? The question can't be answered on the basis of corpuses.

MR. GARVIN: No, I agree, but then you have an informant.

MR. POSTAL: So we seem to agree that the linguist does not study the actual speech, because he cannot do that. He has two possibilities: he either studies a finite corpus which is of no interest, or else he studies a set of all possible utterances which is also of no interest. Somewhere in between lies the language, and it is that which traditionally has been studied.

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Editor's note: This statement by Mr. Postal concluded the formal part of the discussion among the invited speakers. The moderator, Professor Hamp, invited remarks and questions from the floor.
DISCUSSION

GARY MARTINS: (Georgetown University student) I have two questions to address to Mr. Postal. The first one comes to my mind every time I hear somebody talk about generating all and only English sentences or for that matter sentences of any other language. It would seem to me that any program which claims to be of proven validity would have to be validated in logical terms on its own internal consistency. Therefore I would like to ask you, just what kind of logic you use to justify the fact that every sentence which is going to come out of your machine, from here to eternity, will be English, and that eventually all the sentences of English will be generated? That is my first question. It is a question in logic.

My second question deals with your machine. The question of the indecisiveness of the informant in identifying Mohawk vs. non-Mohawk on different occasions, and the variability between idiolects and informants has been raised. To begin with I will assume that your generating machine has no “Monte Carlo process”* within it. Therefore, inasmuch as a machine cannot run, unless a yes-or-no decision is made at every step and process, I assume that your machine today generates the same sentences that it will generate tomorrow. Therefore, the output of your machine would be unique, generating all and only Mohawk sentences. Is this to say then, that your program is a better speaker, in the sense of Mohawk, than the native informant? Then, on the other hand, let us say we include a “Monte Carlo” process in your machine, will it generate the same sentences today that it will generate tomorrow? If, in the latter situation, we get different outputs on different occasions from what we get from the informant, how then do we prove that we are generating today all and only Mohawk sentences, and prove the same thing tomorrow when we are getting a different output?

MR. POSTAL: There is no more problem in testing the theorems of a generative grammar than there is in testing the theorems of any other science which would also be infinite. Obviously, all you can do is test a finite number. You cannot prove that it generates all and only sentences. You can show, however, that the things that it does generate seem to be sentences, and that the things that it does not generate seem to be non-sentences. The general problem of con-

*A “Monte Carlo Process” is like a dice-throwing technique in a machine, i.e., a random decision procedure derived from the roulette wheel.
firmation of scientific theories that you have raised is, I think, of no particular relevance to linguistics.

MR. MARTINS: You just said you can show that what the machine does generate is English, and what it does not generate is not English. Do you mean to say then that one cannot conceive of a sentence, in good English, which will not come out of your machine?

MR. POSTAL: No one has ever claimed that we have a complete grammar of English. This is set up as a rational goal.

MR. MARTINS: If you cannot prove that it is possible, it is an irrational goal. You were talking about I.O. grammar and phrase structure grammar as being inherently logically incapable of analyzing language, and now you are trying to apply the same thing in your grammar.

MR. POSTAL: I did not say anything quite like that. Let me repeat what I did say about phrase structure grammars. I said that they do not assign the correct structural descriptions to coordinate constructions. That is a much more limited statement. For some types of phrase structure grammar you can prove that they cannot enumerate the correct set of terminal strings, that is, the correct sets of sentences, even if you let them produce the wrong structural descriptions.

Secondly, there has been some talk of machine. This is an ambiguous notion in the sense that a determinate theory can be thought of as a machine, in that at each point its operation is determined by specifications that came before. This should not necessarily be thought of as a machine in terms of hardware.

EINAR HAUGEN: (University of Wisconsin) I must say that when I first began reading about generative grammar and about transformation procedures, I was extremely sympathetic to it, because, it seemed to me, that here was a wonderful corrective to the one-sided synchronic item-and-arrangement grammar which had dominated American linguistics for the past generation, which I had not been sympathetic with, and which seemed to me to be missing some very essential elements of the structure of language. The opening of Mr. Postal's paper indicates to us that, in a sense, this is carrying on an older tradition, a tradition of grammar in which no one hesitated to say that you add something to something in order to produce something else. For a long time now, everyone has been attempting to eliminate this kind of process terminology, which was
said to be historical, in favor of statements of purer synchronic distribution. This seems to me a great advance. It represents the structure of language better, simply because we know that language is a living thing. People learn it and people change it. But once one begins to have discussions with the people who advocate this new approach, one discovers a certain dogmatism. Every discussion I have had or heard, including, so far, this discussion today, seems to me unprofitable. All that we get as answers to our questions are assertions, and not any of the kind of proof which most of us would be willing to accept as evidence. I have lived long enough to have heard a number of dogmatisms. I can remember when some of the workers in synchronic linguistics came up with the item-and-arrangement grammar, and some of us objected to the rigidity of it. Now again, we find a new rigidity, and I wish that somehow the people who are so enthusiastically pursuing this new form, would understand some of the problems in presenting their ideas to other people, so that those others could accept them willingly.

My own opinion about this grammar is that like so many previous grammars, it is a partial one. It appears that any time a question is asked like the one Paul Garvin asked, namely how do you analyse this or that sentence, the answer is, “This is immaterial.” It seems to me that this is a grammar for the speaker, not a grammar for the hearer. Since both of these are essential to any form of linguistic communication, it seems to me that a grammar is inadequate from this point of view unless it can be reversed in some way. A phrase structure grammar has the advantage over the transformation grammar as a process of understanding. Of course, in the end we will not accept this doctrine unless it can prove itself; it will not prove itself by assertions. It will have to do so by producing grammars. So far, it has not produced a grammar of any single language, and only fragments of the grammar of any particular language, chiefly English. When it does produce a grammar, we will, of course, be ready to evaluate it in terms of the traditional procedures.

MR. POSTAL: Let me try to answer the substantive issues at the end of your remarks first. No one ever said that decision procedures, analysis routines, etc., were of no interest to generative grammar. What has been said, is that the discussion and study of generative grammars is logically prior to the study of recognition routines and decision procedures. Certainly a complete account of linguistic behavior must include these. But before we can study devices which will give a structural description for any arbitrary ut-
terance, we certainly must know just what a correct structural description is and just how a finite organism is able to impose such on an infinite set. These are the questions studied in generative grammar. Professor Haugen claims that generative grammar takes the point of view of the speaker. In fact it takes the point of view of neither speaker nor hearer, nor anyone else. It is simply a device that enumerates the sentences of the language. The speaker does not enumerate sentences, he produces them one at a time in particular contexts.

MR. HAUGEN: You mean, 'generates'.

MR. POSTAL: No, he produces them; 'generates' refers to logical enumeration. A theory may be said to generate its theorems. For example, if there were a completely formalized theory of genetics and human biology, it could very well be said to generate the set of all human beings. But it certainly would not produce them. Speech production involves a physical device with vocal cords, tongue, etc. This is a separate matter from generation. Now as to the claims of dogmatism it is difficult to make a defense until this claim is made more precise. Are there no details you can give, no particular points where there were dogmatic assertions?

MR. HAUGEN: You have characterized the generative method as a whole, as producing correct structural descriptions.

MR. POSTAL: I said that that was its aim and I think every one agrees with this. Now we can discuss the question, can certain types of grammar provide the correct structural description? But what is a correct structural description? We have never even reached this point. If we agree that a grammar should assign the correct descriptions to the full set of sentences and if we agree in what constitutes a 'correct' description, then we can consider the types of formal device, that is, grammar, which can accomplish this task. But first we must agree that it is a rational aim.

MR. HAUGEN: On this point no one would want to disagree with you. I mean, this is what we want to discuss, but we get the impression from your statements that you feel you have the answer and the only correct method, as compared with the one Mr. Garvin was advocating.

MR. POSTAL: I do not know what point you are referring to here. I would certainly claim that transformational grammars are the only known devices which can assign correct structural descrip-
tions to a wide variety of natural language constructions. Today I mentioned only co-ordinate ones. But only a small part of my discussions with Mr. Garvin have been about this. Much of them have been concerned with whether this is a reasonable goal. There are two questions: first, whether a grammar should be generative, and second, whether it should be transformational. I think the answer to both is YES. But the first is a methodological question roughly equivalent to should a grammar be a testable theory of its subject matter. The second is a linguistic question proper.

CARLETON HODGE: (Foreign Service Institute) A few weeks ago, Professor Zelig Harris, among others from the U. of Pennsylvania, presented their project on transformational analysis at the National Science Foundation. At that meeting, Professor Harris said that certain facts about a language are discovered by transformational analysis, which are not discovered by other means. This, I think, is important. He did not say, however, that this is the only way to approach language. He also said that he has nearly finished a grammar of English. This will answer one of Professor Haugen's questions. I would much like to see transformationalists concentrate on bringing out the things which transformational analysis can do better than any other method of analysis.

MR. POSTAL: There is an unfortunate ambiguity here, in that Professor Harris does not use the term 'transformation' in anything like the sense Professor Chomsky does. For Harris a transformation is a relation between sentences and their parts based on the notion of co-occurrence. In his sense all transformations are 'reversible' and the notion of 'obligatory' transformation is meaningless. For Chomsky, a transformation is a kind of rule which may be obligatory or optional, and which may, but need not, reconstruct sentence relations of the co-occurrence type. In using the term transformation today, I, of course, was using it in Chomsky's sense. The question of the 'only' way to approach synchronic grammar arises only in the context of methodology. What I have claimed today is that the correct goal for synchronic linguistic description is the formulation of explicit, precise, formalized, in short, generative grammars. This requires discussion today because grammatical descriptions do not, in general, have this character. And, as a number of comments today have shown, it is not generally agreed that they should have it. The question whether the correct form of generative grammar requires transformational rules is one which I suppose should properly have been discussed on this panel, but in fact very little was said about it.
The chief claim I made today was that transformational rules were required for the description of co-ordinate constructions. But there is ample evidence in the literature that they are needed for reasons of simplicity and correct structure assignment in such cases as common selections in reverse order, agreement, discontinuities, structural ambiguities, etc.

MR. HODGE: I do not see that the aim of linguistics is that of producing either a generative grammar or any other specific type of grammar. Rather I would agree with Paul Garvin that we are trying to discover something about language while we describe it. If we can discover something about language through the item and arrangement approach or item and arrangement process, good! If we can discover something through the Harris transformations or your transformations, fine! But I would not want to say that any one approach was definitive.

MR. POSTAL: I don't disagree with that; it is simply that we must consider what generative grammar means. A generative grammar is simply a theory which makes testable claims about the sentences of a language. Anything less than that, it seems to me, does not say anything about language.

MR. GARVIN: If you say that a generative grammar is a grammar that makes claims about accounting for a language, then, of course, this is just another way of saying that a generative grammar equals a good grammar, a claim made also by any descriptive grammar. It accounts for the total population on the basis of a sample, or the total population of possible utterances on the basis of the behavior of an informant that has been observed, or some other justifiable basis. If it cannot be justified by formal logic, then it can be justified by sociology or Newtonian experiments in physics, or anything else you want to use. The second point is that I believe that it is indeed possible to use transformations as a discovery procedure. The third point is that I don't see any significant distinction between a relation and a rule that expresses this relation. This to me is a kind of sophistry that the logical positivists have been trying to force upon the scientific world, and it is out of fashion now among physical scientists. In other words, I don't see much difference between Harris who talks about transformations as if they were existing things and Chomsky who talks about transformations as if they were rules about existing things. This to me is really not a fundamental distinction.
MR. POSTAL: Again you seem to insist that all terms be used in a maximally unclear way. As I noted before, there are types of transformations in Chomsky's sense which cannot be utilized by Harris. For Harris all transformations are reversible, while Chomsky makes the claim that they are all uni-directional. In failing to distinguish these notions you simply involve yourself in contradictions and other forms of confusion.

MR. GAGE: Without being generative at all, it seems there is a certain type of stable claim which theories may make, namely, that they have the taxonomic categories into which anything that turns up will fit in some natural way. They have, just like ornithology, enough categories to identify any bird that happens to fly in the open window, and qualitative analysis has enough categories to name any item that turns up in an unknown. This is a claim about a certain type of adequacy for description of speaking behavior.

MR. POSTAL: Yes, but this is really a weak claim. Naturally, the weaker the requirements for explanation are made, the more different and incompatible theories will be compatible with this data. In view of this we should try and make our requirements for linguistic theory as strong as possible.

CHARLES BIDWELL: (University of Pittsburgh) I have a question about phonemes that I would like to address to Mr. Postal. Do I understand correctly, that you feel that phonemic analysis by conventionally practiced methods is no more valid or interesting than any random throwing together of forms into units by whatever other means you might choose, say stargazing, or casting dice, or something like that?

MR. POSTAL: No, that's not true. It has more validity than that, but much less than another phonological approach which was followed by Sapir and which is now advocated by Halle. In phonemics certain quite unexceptionable moves are made and certain real redundancies are eliminated. But there are a very large number of phonological predictabilities which can be expressed in rules which are simply unstatable in phonemic terms. That is, there are all sorts of features which are in fact predictable, and which pho-
nemics is forced to treat as distinctive. The transcriptions required by these rules would now generally be called morphophonemic. This is more than a matter of labels. It is possible to show that adding phonemes to a grammar which contains morphophonemic and phonetic representations has the result of complicating the rules. This was shown by Halle. I also showed this for Mohawk in a paper at the LSA meeting in Chicago. Many similar examples can be found.

MR. GARVIN: I should like to make a very strong statement in this connection and say very flatly, that after discussing the matter with various and sundry engineers, I am convinced that without keeping phonemics and morphophonemics strictly apart you can never solve the problem of mechanical speech recognition. This to me is very strong empirical engineering evidence for the superiority of one approach over another, comparable to the engineering superiority of the theory of arches, if you wish, over the theory of just blocks. I think that when it comes to the question of simplicity and neatness, the description by Halle which throws together phonemics and morphophonemics, is far inferior to the description by Trubetzkoy where the two act peculiarly separate, and in an extremely elegant way morphophonemic symbols are used together with a phonemic transcription to represent a structure of Russian with, what I would call, maximum simplicity.

MR. POSTAL: It is one thing to make strong claims. It is another to support them with evidence. I know of no single piece of evidence to support the view that phonemic representations in the post-Bloomfieldian sense are required for an adequate theory of speech perception.

MR. GARVIN: There are many experiments in speech recognition, and the ones that even have a modicum of success operate with phonetic distinctive features of one kind or another. For instance, the very elementary speech recognition equipment being built at IBM in San Jose works with features of voice and friction, and so on, without including any kind of morphemic information.

MR. POSTAL: But these features are an integral part of Halle's approach.

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3 Morris Halle, Ibid.
MR. GARVIN: I agree. All I am saying is that unless you keep morphemic and phonemic concepts separate, the problem will not be met.

WESTBROOK BARRITT: (Washington and Lee University) Mr. Garvin, couldn’t the machine be inferior? After all, mechanics construct machines, and I think we have something here for which a scientist is needed, not a mechanic. I agree with Mr. Haugen that there are some basic misunderstandings. For example, terms such as “behavior,” “rules,” “interests,” and “generation” will have to be explained to us. I think also, that part of the misunderstanding stems from the literary, or philological training that most of us who are interested in languages and linguistics have had. I would say that at this point we have a chance to put linguistics on a scientific basis. The basic claim that Chomsky makes is one of simplicity, and I think that this is carried out. Even if phrase-structure solves a great many problems, transformation, as practiced by Chomsky, cuts the process in half. It is hard to imagine cutting infinity in half; nevertheless, this is what happens. Let me read what he says: “in making use of phrase structure and transformations, we are trying to construct a grammar of English that will be simpler than any proposed alternative; and we are giving no thought to the question of how one might actually arrive at this grammar.” 6 I think that here the conception of ‘machine’, a word, is throwing us off. A ‘machine’ is not necessarily a gadget that any good mechanic can put together. “We are giving no thought to the question of how one might actually arrive at this grammar in some mechanical way from an English corpus, no matter how extensive. Our weaker goal of evaluation instead of discovery eliminates any fear of vicious circularity in the cases discussed above.” 7

MR. GARVIN: Machines are as good as the people who build them. They have one advantage though, and that is they have no intuition so that they do follow the rules that you lay down. In this sense I think machines are a very significant verifying device. Engineering, in a very real way, is a verification of the underlying physical theory, because if the theory is wrong the bridge collapses. Consequently, if indeed it is true, and I will make the flat assertion that it is, that speech recognition has to differentiate between phonemic and morphemic elements and therefore between phonemic and morphophonemic, then this is as good an evidence as the evidence

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of bridge-building is for the mechanics of stresses. In linguistics, the basic problem is not theory construction, because this has been going on with varying degrees of success since before Panini. I think the basic problem is method and the construction of techniques for accurate measurements by means of which theories can be verified. Until this is done, no theory is going to be more than a flat assertion. This is the basic aim of linguistics as I see it.

**MR. POSTAL:** What sorts of measurements do we need in linguistics that we do not have? After all, there have been several thousand years of data collection in linguistics. Your idea that theories are abundantly present, needing only to be tested, is it seems to me, fundamentally mistaken. The whole question of data collection is relevant to what kinds of questions are being asked, that is, to what kinds of theories are being proposed. But before a theory can be tested, that is, before any data can be collected which will bear out the truth of a theory, it must be presented in a precise form. This is, I think, the chief need in linguistics. A generative grammar is simply a linguistic theory in a precise form.

**JAMES REDDEN:** (Foreign Service Institute) Assuming you have your generative grammar completed, to what extent would random access nullify or modify your rules, (especially the order of your rules) if you have to have random access of any sentence for verification? I grant you, your grammar would generate it, but is it a sentence or is it not a sentence?

**MR. POSTAL:** That is a matter of recognition routine. The grammar alone is not enough. Given any random utterance, you cannot tell, given a grammar, whether it is a sentence. The grammar enumerates a certain output, and you can check that for sentencenhood.

**MR. REDDEN:** Mr. Garvin said that you can build a machine that can grind out perfectly good sentences forever. Is that all you are going to do,—just grind out sentences?

**MR. POSTAL:** It is not a question of 'grinding out sentences'. It is finding that simplest function which not only enumerates all and only the sentences, but which simultaneously provides each with a meaningful structural description. For instance, there is no interest in a grammar which tells us that 'John eats meat', and 'The boy drinks water' are sentences, but which fails to tell us that they are sentences of the same type.
MR. GARVIN: The machine can do that, too.

MR. POSTAL: Does anyone else have a device that, given a random sentence, will correctly analyze it?

MR. GARVIN: Librascope has a machine which will produce random sentences and the program can, if given certain exits, also give the description. You will probably say that this is the wrong description.

MR. POSTAL: No, I won't say that before I look at it. The question is, do the people who have such a system make the claim that the grammar of this device—I presume it has a grammar built in—will enumerate all the sentences, and that their recognition routine will recognize all of them.

MR. GARVIN: The Librascope people had a marvelous idea. They thought that if they had a store of English words and a set of frames and a program that would select randomly from the store and put the selection in the correct place in the frame, they would get English sentences. These sentences would be initially very peculiar, and if they did not like them, and if they wanted to get better sentences, they would just juggle the frames and the lists and the rules and thus gradually improve the program. Now this is commonly done in crude engineering applications. You start out with a first approximation and you work on to something better. The only thing that I know is that their latest output is better than their prior output, which to me is a sign of progress. Whether they will be able to ultimately generate all and only, is impossible to foretell.

MR. POSTAL: Let me answer Mr. Redden's question. The question is, whether given any arbitrary utterance we can guarantee an analysis. Given many utterances we may be able to find out whether the grammar generates them. The point is that the grammar itself cannot guarantee this. For any input x you can run through the rules and see if you can get x as output. But the grammar is not designed to automatically provide the analysis of any presented sentence. This is a matter of recognition routine.

JOSEPH A. REIF: (Foreign Service Institute) Can this grammar take one of its own sentences, any sentence at random that it has generated, and recognize it?

MR. POSTAL: Not necessarily. It depends on many complex factors, for instance, on how the rules are set up, and on the kind
of rules. Anyhow, which kind of grammar are you talking about, a phrase structure grammar or the kind that Chomsky has? A transformational grammar is not designed to recognize sentences, it is designed to enumerate them.

**MISS DALLAIRE:** It is designed to “produce” them.

**MR. POSTAL:** Certainly, the grammar by itself cannot recognize a single sentence. Whether you can use the grammar in some non-formalized way to analyse that sentence, I don’t know, and I cannot guarantee. Whether it could, in fact, by various manipulations tell whether it generated it, is another matter.

**MR. REIF:** Then it is quite narrow.

**MR. POSTAL:** No more so than anything else.

**MR. HAMP:** We have to call a halt to the discussion at this point. It is clear that a great deal remains still to be done in this field. It seems to me that several rather important points were brought out towards the end, apart from the substantive discussion all along, and that, as Prof. Haugen has eloquently stated, there is much explaining to be done for the benefit of those of us who are less practiced than others who have been privileged to study and comprehend these new approaches and ideas. On the other hand, it seems to me that there is a certain amount of learning incumbent upon those people who need explanations. I hope very much that these discussions will at least be another step along the road to an understanding and a meeting of minds.
PANEL II

BILINGUALISM
The German linguist Albert Thumb, who worked extensively with the various modern languages spoken in the Greek homeland, early noted, as one must, that there is "ein heftiger Sprachkampf" between the collection of modern dialects which (together with an emergent colloquial standard based on some of them) has been called δημοτική /dimotiki/ (hereafter Demotic), and a literary standard called καθαρέωσα /katharevusa/ (hereafter Katharevusa). The relationship between the two systems and their histories are not unlike other instances of diglossia, paralleling, for instance, the situation which Kucera has recently described for Literary and Colloquial Czech. Later during these same meetings, I understand that Professor Householder will inform us in greater detail concerning this interesting case of Greek diglossia, which is euphemistically referred to in Greece as the γλωσσικόν ζήτημα /glōsikon zítima/, the 'language issue'. My only purpose in further mentioning it now is to indicate

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1 The research upon which this paper is based was supported by a research grant from the Laboratory of Social Relations, Harvard University. I acknowledge with thanks comments offered me by Eric P. Hamp, Charles E. Bidwell, and Fred W. Householder.

2 Albert Thumb. 1895. Handbuch der Neugriechischen Volkssprache. Strassburg; Verlag Karl J. Trübner; and also his 1926 Grammatik der Neugriechischen Volkssprache (revised by J. E. Kalitsunakis). Berlin and Leipzig; Walter de Gruyter & Co. Thumb (1895:vii,viii) succinctly describes the Greek diglossia: "... man versteht bekanntlich unter 'Neugriechisch' zwei Sprachformen, einmal die lebendige vom Volke gesprochene, in Zahlreiche Mundarten gegliederte Sprache, welche eigentlich allein den Namen neugriechische verdient, dann die Schriftsprache, die καθαρέωσα d.h., 'reine Sprache', welche eine literarische Wiederbelebung der mehr oder weniger modernisierten altgriechischen Gemeinsprache ist, also ein Kunstprodukt, allerdings nicht der neuesten Zeit, sondern das Ergebnis jahrhundertelangen, bis über Byzanz hinausreichenden Schriftgebrauchs: wie weit man in dieser erstarrten altgriechischen Sprachform, der Volkssprache entstammende Elemente zulässt und zulässt, was nicht nur in verschiedenen Zeiten verschieden, sondern wechselt auch jeweils nach Autor und Gegenstand."

3 Henry Kučera. 1961. The Phonology of Czech. 's-Gravenhage; Mouton & Co., see especially pp. 11-20. The so-called spisovná čestina 'literary Czech' or spisovny jazyk 'literary language' is an analogue to Katharevusa; there is a colloquial "standard" (the hovorová čestina) which, like its analogue Demotic, is based on a multiple background of colloquial dialects such as the obecná čestina, and yet is influenced markedly by the literary language. Charles A. Ferguson has outlined four similar cases (including Modern Greek) in his 1959. "Diglossia", Word 15:325-340.
how it has obscured aspects of the subject which I want to discuss, namely, the nature of recent English-derived interference as this occurs in the bilingual speech of American Greeks, a variety of Demotic which Greek nationals are wont to label "Greek speak American".

The language contacts which the Greek dialects have experienced during the past several centuries are many, and it is not unreasonable to assume that some of the observed linguistic changes in Greek, especially in the phonology and the lexicon, derive from those contacts; comparative procedures demonstrate, in fact, that many are. For the linguist restricted to written materials, however, there exists the problem of getting behind the facade of Katharevusa; the problem is similar to conjuring up an image of the ancient colloquial speech obscured behind a lattice of literary Attic, by examining, for example, Aristophanes's comic dialogues. Perusal of modern bilingual Greek dictionaries show considerable mixture at many points between Katharevusa and Demotic. The mixture, however, is not without a measure of order: when ever there is an actual rivalry between synonymous Katharevusa and Demotic members of a doublet, and the fact of rivalry is seldom reported, the problem of correct entry is cavalierly resolved by omitting the Demotic form. In these cases, the Demotic form is frequently a recent borrowing. We cannot welcome too eagerly, therefore, the appearance of dictionaries such as Swanson's *Vocabulary of Modern Spoken Greek* since the Demotic entries contained therein provide an infinitely more reliable index of recent linguistic interference. It is difficult, to be sure, to assess the full effect of Katharevusa not only in fending off on-going linguistic interference but also in weeding out products of recent contact-induced changes. It is now known that there are a larger proportion of Katharevusa-derived loanshifts and induced creations than earlier realized. These are of the sort which enabled German-language sentimentalists to hold on to *Fernsprecher* before the onslaught of *Telefon*; and, like German, these forms especially abound in learned and technical vocabulary.

Loanword phonology has brought about a number of syntagmatic changes in the native phonology, both in the distribution and in the native phonology, both in the distribution and in the sequence cont-

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straint rules of the phonemes involved. For example, the distribution of the voiced stop series /b,d,g/ has changed as result of diaphonic identifications with various foreign models which had similar sounds in word-initial position; prior to this phonic interference, /b,d,g/ never occurred in word-initial. A brief examination of dictionary entries for /b,d,g/ will quickly betray their non-native provenance. French and Italian loanwords certainly have been more influential in this syntagmatic change, but the Demotic forms μπούνι /búsi/ 'bus', ντόνατ /dónat/ 'doughnut', and γρέιπφρουτ /gréipfrut/ 'grape-fruit' are all replicas of English models. For another example, it is easy to reconstruct a morphophonemic code in many Greek dialects, for a period antecedent to the impact of French-Italian-English contracts, in which, between successive junctures, (1) nasal plus voiceless consonant becomes nasal plus voiced consonant, or in some cases, becomes voiced consonant alone (thus e.g. /n + t > nd, m + p > mb/); and (2) /n before labial stops becomes /m plus voiced labial stop, i.e. /n + p,b > mb/. Syntagmatic changes in loanword phonology have altered these rules: e.g. /nk (earlier > ng) occurs in English-derived loanwords such as τάνκς /tánks/ 'tank', /np (earlier > mb) in τίπον /típón/ 'ping-pong', /nt (earlier > nd) in τζέντελμαν /dzéntelman/ 'gentleman'.

Loanword phonology has incurred little paradigmatic change from an analytic point of view; there have been no major changes in the status of distinctive features as opposed to determined or conditioned, i.e., redundant features in the phonological code. Minor exceptions include the change from conditioned to distinctive for the feature 'grave' (as opposed to 'acute') in the non-labial nasal /n/, such that phonemic split occurs between the acute allophones [η] [n] and the grave velar [η]. Prior to the introduction of loanwords such as πίνον /pínón/ ['pi.ŋ.pɔŋ], [ŋ] was in non-contrastive distribution with [ŋ] [n], occurring only before velar consonants (/n + k,g > [ŋ]g; n + x,y > [ŋ]γ); they now contrast in syllable-final position. Phone-types novel to the earlier phonology have been

5 The orthography acknowledges the special status of these entries and reflects one of the morphophonemic rules discussed later in this paper: initial /b is written μπ (mp, but m + p > mb, or sometimes > b); initial /d is written ντ (nt, but n + t > nd, or sometimes > d); g is γκ but note that — γκ — (as well as — γγ —) is — ng — (and n + g sometimes > g); in word-medial —μτ — = — mb — and — ντ — = — nd —. This is an interesting example, manifest orthographically, of the "psychological reality", not of phonemes (in the traditional sense), but rather of morphophonemes.

imported: in some dialects, one regularly hears the fricatives [ʃ] [ʒ] and the affricates [tʃ] [dʒ], which have come in with loanwords such as σιρό /šeri/ 'sherry', σάπ /ʃap/ 'shop', τζέρκ /jérk/ 'jerk', not to mention in many non-English loanwords, e.g. γκαράζ /garázh/ 'garage' ( < French garage /garaz/). In other dialects, however, diaphonic identifications have substituted similar native sounds rather than importing these non-native ones; thus /séri/ /sap/ /dzérk/ rather than /séri/ /sap/ /jérk/. Retroflexed vowels and English-like diphthongs have been imported also, but their frequency is low. Analytically, there is no compelling reason to set these importations up either as new phonemes or as constituting a separable coexistent system, a point to which I will return.

These changes, already integrated in the Demotic phonological code, are the common property of all American Demotic speakers, quite apart from their subsequent contacts with English. For those who do learn English, however, these integrated changes have paved the way for the assimilation of new loanwords with relatively little distortion of the phonological shape of the original English models. An extreme of distortion involving recent loans might be the replica ['pekədzdɔrn],* but the shape of the English model package-store ['pekəj, stɔ:]. is nevertheless readily identifiable.

The generalizations which I want to make about the phonology of code-switching are based on the study of the diaphonic identifications which coordinate bilinguals make in their rendition of English words and phrases in the context of Greek discourses. By ‘code-switching’ is usually meant the successive alternate use of two different language codes within the same discourse; it implies that the speaker is conscious of the switch. Although the research on which this report is based included the study of Greek forms in the context of English discourses, I will restrict my remarks here to the examination of English forms in the context of Greek discourses. The majority of idiolects which made up my sample was those of speakers who were coordinately bilingual in Greek and English, i.e., in whose Greek and English there were no features of foreign accent in one such as we might attribute to the dominance of the other. The sample did include, however, subordinate bilinguals who had obvious accents in English, their secondary language. Without excep-

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* In the phonetic transcription, [r] = apico-alveolar flap; [r] = any of the typical allophones of American English /r/.
tion the speakers had been born into families whose primary lan-
guage was Greek. The coordinate bilinguals either acquired their 
Greek and English together as primary languages in childhood, or 
subsequent to having learned Greek as a primary language achieved 
perfect mastery of English as a secondary language. The descrip-
tion of the phonic interference which I observed, essentially listings 
of diaphonic identifications which account for the shape of Greek 
replicas of English models, is too lengthy to present here. The gen-
eralizations which the description yielded are more interesting.

One of the prevalent types of change in American Demotic is 
that which Haugen has called 'reborrowing', a process which some-
what resembles dialect borrowing in the bilingual community and 
by which a replica-loanword becomes ever more alike in shape to its 
original model in the donor language. Reborrowing presumably 
operates on the basis of increasing proficiency in the secondary 
language and consequent greater familiarity with the models therein. 
The process is strikingly apparent in the case of those English-
derived loanwords which are already integrated in Demotic (some 
so even before the 20th Century immigration to the United States), 
forms such as: (various written forms) /oloráit/ ‘alright’, μπάρ /bár/
/βαρ/, κέκ /kék/ ‘cake’, τσέκι /tséki/ ‘check’, (various written forms) 
/gágzder/ ‘gangster’, πζάτζ/ /dzáź/ ‘jazz’, σάντσκι /sándwits/ ‘sand-
such as these (I estimate that there are well over 100, some with 
high frequency of occurrence) are learned by the monolingual 
speaker as well as the bilingual during the normal childhood 
acquisition of his speech; they are a part of the Demotic 
langue. But many younger Greek speakers, coordinately bilingual in Greek and 
English, shortly acquire, as the Greek forms, not ['bar] but ['bar], 
and ['eik] for ['ke:k], ['ček] for ['tsék], ['gænˈstər] for 
['gægzdan], ['jæ.z] for['dza.z], ['sænwič] for['sandwits], ['še·
ri] for ['sɛ:ri], and ['šák] for ['sɔk].

It would be very interesting, working with a large sample of 
speakers, to ascertain whether there is a stratigraphy of replica-
shapes from least to most resemblant of the model, co-varying with 
the proficiency of the speaker in the secondary language which con-

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8 ‘Reborrowing’ is discussed in Einar Haugen. 1950. “The analysis of 
linguistic borrowings”, Language 26.210-231 (see especially section 12); see 
also Haugen's 1953. The Norwegian Language in America. 2 Vols. (Philadelphia; 
and Chap. 16 'The Phonology of Loanwords', pp. 383-458, for many well-
described parallel cases in American Norwegian.
tains the models.\(^9\) I have noticed chains like (for \(\rho\alpha\delta\omega\) ‘radio’) 
\[\text{[}'\text{radio}'\text{] — [}'\text{re:dio}'\text{] — [}'\text{re:dio}'\text{] which are thus correlated,}
\[\text{[}'\text{radio}'\text{] occurring in the speech of immigré subordinate bilunguals}
\[\text{who learned English later in their lives, and [}'\text{re:dio}'\text{] in the}
\[\text{speech of young American coordinate bilunguals. There are a few}
\[\text{loanwords which may continue to resist the effects of reborrowing;}
\[\text{these are mainly place-names e.g., N\text{é}a \text{'}Yo\text{pki}/n\text{é}a i\text{ó}rki/ ‘New York’,}
\[\text{Bo\text{\textae}t\text{\textae}nis} /\text{vost\text{\textae}nis}/ ‘Boston’, which were widely known and deeply}
\[\text{integrated before immigration.}

Examining the Greek of the coordinate bilunguals brought to
light the following significant but not unexpected finding: that, with
the exception of a very few forms, phonic interference always makes
for perceptible differences between the replica and the model. This
difference obtains independently of whether the speaker (1) produces
the model without accent when speaking English or (2) is aware
of the relationship of the English model and the Greek replica. I
collected a short text in English from one coordinate bilingual, the
subject of which concerned Boston weather reports. A detailed
phonetic analysis failed to reveal any of the properties which might
implicate a Greek accent, e.g., unaspirated phase-initial voiceless
stops, substitution of \([\varepsilon]\) for \([\varepsilon]\), flapped for trilled \(r\) substituted
for syllable-onset semi-consonant \(r\), \([ts]\) for \(\varepsilon\), \(s > z\) before voiced
consonants, etc. A subsequent text in Greek was elicited, and the
English-derived loanwords or code-switchings contained therein were
subjected to detailed phonetic analysis. These included the follow-
ing examples: [\text{ug\text{o}ri,\text{p}ort], cf. his English [\text{u\text{e}d\text{a}r+-ri,\text{port}]
\text{weather-report}, [\text{bek,\text{b}et\text{\textae}st\text{\textae}san}], cf. his English [\text{b\text{e}k,\text{b}et\text{\textae}st\text{\textae}san}]
\text{Back Bay Station}, [\text{mr\text{\textae}m z\text{\textae}n}], cf. his English [\text{mr\text{\textae}m z\text{\textae}n}]
\text{more damn snow}. The last example is a telling one: hearing
the tape-recorded phrase in context of the Greek text, the in-
formant was convinced that he had merely switched briefly into
English, explaining that that had indeed been his intention. Hearing
the two phases, one from the English text, the other from the Greek
played in isolation, he was obviously astounded by his “accent.” The
informant’s naive observation is partly true. The changes which
affect the shape of the English models when they appear as replicas
in Greek contexts, closely match the very changes in the English
spoken by subordinate bilunguals which characterize a “Greek accent.”

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\(^9\) This is a recurrent phenomenon in the American Norwegian described by
Haugen 1953, op. cit.; e.g. cf. the older Norwegian speaker’s [\text{tavan}]
\text{tavern} [\text{t\textae}v\text{\textae}rn], and the younger
speaker’s replica [\text{t\textae}v\text{\textae}rn].
Much of the distortion derives from disturbance in the lowest level rules of the phonology, i.e., to various changes in the occurrences of redundant features. Such phonetic distortions can make for alarming changes in the phonetic substance of a discourse, but without altering the basic formal properties of the phonology. It will be the purpose of a future paper to demonstrate that the phonology of Greek bilingual speech can be (must be) analyzed without establishing a coexistent phonological system.¹⁰

One conclusion which seems to emerge from this research has more relevance to the psychology of language: It has been my impression that code-switching, strictly defined, obtains only as a hypothetical endpoint in a typology of bilingual behavior. One of the linguistic correlates to the coordinate bilingual’s purportedly being inputted with two separate codes, is his ability to keep the two language systems distinct with respect to linguistic interference.¹¹ Greek-English code-switching has convinced me that, even with a speaker’s conscious efforts to offset interference, that Greek-English coordinate bilinguals cannot switch to one language in the context of the other without incurring phonic interference of the sort described in this paper. This suggests to me that we need to review our still imperfect notions of what is involved in the separability of two language codes in the same speaker.

¹⁰ See the procedures suggested by Charles C. Fries and Kenneth L. Pike, 1949. “Coexistent phonemic systems,” Language 25.29-50. It is curious that an earlier statement by Pike (1947. “Grammatical prerequisites to phonemic analysis”, Word 3.155-172) to the effect that “one should hesitate to allow a small residue of words of foreign origin to prevent a general formulation” (p. 171) appears to be contradicted by the Fries and Pike 1949 paper.

¹¹ The psychological attributes of the coordinate bilingual have been discussed by Uriel Weinreich, 1953. Languages in Contact: Findings and Problems. (=Publications of the Linguistic Circle of New York, No. 1) [see pp. 8-10, 71-82], and by Charles E. Osgood and Susan Ervin in C. E. Osgood, ed. Psycholinguistics: A Survey of Theory and Research Problems (=Indiana University Publications in Anthropology and Linguistics, Memoir 10) [see pp. 139-145]. More recent literature is reviewed by Einar Haugen 1956. Bilingualism in the Americas: A Bibliography and Research Guide (=Publication of the American Dialect Society, No. 26) [see pp. 69-86].
DISCUSSION

EINAR HAUGEN: (University of Wisconsin) Dr. Diebold said that code-switching implies conscious switching. I originally used the term "code-switching," and I had not necessarily thought of it as conscious. In fact, I was trying to exclude the problem of whether it was conscious or unconscious, since in practice we do find a great deal of switching back and forth among naive speakers who are obviously quite unaware of the fact that they are changing languages. I found numerous examples of the kind of layering of interference which Dr. Diebold here reported among my Norwegian-American informants. One of my favorite ones is where the 'r' was actually borrowed in two different ways within the same word, namely the word 'hardware store'. In a community in western Wisconsin, people whom Dr. Diebold has called 'coordinate bilinguals' use English 'r' in the word 'hardware' and Norwegian 'r' in the word 'store', the latter being presumably an older borrowing. Norwegian spelling is used for the word as a whole, and the two parts are combined according to Norwegian rules, so that there is a different degree of interference or of integration in the two. Another word which I shall quote agrees closely with the material that was presented here, namely the word [weather-riporten] which means the weather report and has English phonemes all the way through, except for the intonation and the definite article at the end.

CHARLES BIDWELL: (University of Pittsburgh) I would like to comment on the etymology of several of the items which Dr. Diebold used as examples in his paper. For instance, 'bira' is certainly not borrowed from English but rather from Italian, and the Italian word, of course, goes back to some Germanic root. This is a case not of borrowing of two English items in different forms, but of borrowing of a word first from one language and then later in America from English. Secondly, the word 'tšai'. This is probably, I believe, from Turkish rather than from Bulgarian inasmuch as Greek has borrowed far more vocabulary items from Turkish than from Slavic. Thirdly, the question of 'en dăksi'. I don't think it would be correct to say that this is a loanshift legislated by the advocates of Katharevusa replacing English "O.K.," because this expression exists in Greece itself and it seems to parallel similar expressions in various languages of East Central Europe such as the Serbo-Croatian 'u redu',
German 'in Ordnung' and so forth. Therefore, again, the influence may not come from the English speaking world, but perhaps from a cause of common Europeanism. Lastly, the question of the phonemes /ʒ/ /ç/ /ʝ/ and so forth. There are Greek dialects in northern Greece where these phonemes occur, and I believe they occur as a result of internal development rather than influence of other languages. Of course, a conceivable influence would be from Turkish or from Slavic, but I think without knowing too much of the phonological development of these dialects, that they do result from internal phonetic development of the Greek dialects concerned. And, since people from all regions of Greece emigrate to the U. S., this must be taken into consideration.

**MR. DIEBOLD:** Thank you first of all for the 'bira'. I'll make sure I correct that. I disagree in the case of 'tšai' though. I think that this word definitely comes from Bulgarian. As to the Greek dialects, the immigration from the particular northern mountain regions has not been to the U. S., nor have the innovations in the phonology in these dialects contributed much to the rise of the so-called new standard colloquial Demotic. Therefore I do not think your point is relevant. Rather, I would say that the reason why these affricates exist in the Demotic that was brought over to the U. S., is because of the various English and Romance loan words which we have received during the last century. And finally, I doubt if there ever can be a successful loanshift, in the real sense of the word, for "O.K.," mainly because obviously you can not translate this into Greek.

**MR. DIEBOLD:** I would have to look it up in Andreetis' Etymological Dictionary of Modern Greek, but my guess is that Turkish is a much more likely source for 'tšai' than Bulgarian.

**MR. DIEBOLD:** Some of the etymologies of course, are quite questionable. There is a problem of the ultimate source of all loan words in the Greek dialects which date from the last century. Many English loan words which have been related to French and to Italian present a special problem which hasn't been susceptible to precise analysis, and I think the same thing may hold true for Turkish and Bulgarian. Another possible source, although he doesn't go into the matter very deeply, is Karl Lokotsch's study of the various Oriental loan words in European languages.¹ But as you may realize his listing for Greek is quite brief.

ERIC HAMP: (University of Chicago) I think that we need a great deal more control of the internal Greek situation before we can make a rational judgment on many of these things. First of all, the internal Greek dialect set-up is rather complicated, not only because of some of the developments, for instance in North Greek dialects, such as Mr. Bidwell referred to just now, but also because of the fact that phonetically different things come out in phonemically similar positions in different dialects. The dialects of Crete, for example, have our type of sibilants and affricates but they are phonemically identical with the /s/ type ones in the more familiar Peloponesian and Athenian Greek. Immigrants have come from the various islands as well as from the homeland of what is essentially present day standard Demotic. Concerning the 'tsai' word, I too would like to see proof that it is Bulgarian. Almost all food terms of any long date in modern Greek are Turkish loans, whatever their ultimate source may be.

I would like to comment on another item. The internal history of the genesis of voiced stop phones regardless of their phonemic position, is a very long one in Greek. It has been going on since early Byzantine times and the accretion of Venetian loans. The latter, for example, could have been a good source for the 'bira' word which certainly ante-dates more recent contacts. Those Venetian loans were tremendously important. Some words were inherited, for instance the verb to enter, 'beno', which I think is, phonemically initially /mp/ even though we don't hear a nasal preceding it. This kind of thing has arisen ever since changes began to occur in post-classical Greek. Since I think all this has to be controlled so much within the chronology of Greek itself, I don't see how you can make a judgment on many of these items right now.

MR. DIEBOLD: There is an interesting piece of evidence from orthography in a case of the /bdg/. It is interesting for two reasons. First, because it throws some light on the problems which you have just raised, and second because it is a very interesting case of the psychological reality, not so much of the phoneme, but of the morphophoneme. This is illustrated by the custom of writing, for instance, for word-initial /b/ the graphemes usually written for the cluster /mb/. And this holds true for all the initial voiced stops, one of the morphophonemic rules being that, for instance, /m/plus/p/ becomes not only /mb/ but occasionally /b/.
When I received the invitation to participate in this panel on bilingualism, I decided to take as my topic the kind of bilingualism which exists in every complex civilized community, and which I decided to call schizoglossia. While we are all familiar with it under various other names, it has not usually been considered in this connection. This led me on to ponder the conflict which arises within the individual speaker when he becomes uncertain as to what he ought to say and write because the same linguistic item is presented to him in more than one way. This raised the question of a linguistic norm and its place in society, a question which has occupied me recently in connection with a book I am writing on language planning in Norway. Not until I got the printed program of the conference did I discover that a whole section was being devoted to such problems. By this time I was stuck with my title, and if this paper would seem to belong rather in tomorrow’s than today’s panel, you will now understand the reason why.

Schizoglossia may be described as a linguistic malady which may arise in speakers and writers who are exposed to more than one variety of their own language. Under favorable or more precisely, unfavorable conditions, the symptoms may include acute discomfort in the region of the diaphragm and the vocal chords. If the patient refuses to “leave his language alone,” we are assured by Robert A. Hall that he may also be afflicted by general insecurity, which expresses itself as “false humility” and “needless self-depreciation.” The damage to his character, we are told, may be “incalculable.”

Pursuing this thought, I may add that the victims of schizoglossia are often marked by a disproportionate, even an unbalanced interest in the form rather than the substance of language. In extreme cases they may even turn into professional linguists, just as schizophrenics sometimes become psychoanalysts in order to study in others the symptoms of their own ailment.

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Schizoglossia is endemic in American society, which was founded by speakers from various parts of the British Isles. It flares up at times, especially when new editions of standard dictionaries are published. Sufferers are especially common in a society where most people are socially mobile and very few know exactly where they stand. Since its germ or virus has not yet been isolated, its cure, like that of the common cold, has mostly been undertaken by quacks and other well-meaning but financially interested persons. A flourishing industry exists for the purpose of supplying the country with remedies, ranging from pocket handbooks to improve your English through evening courses in diction to huge and costly tomes of scholarship. Dr. Noah Webster diagnosed the malady to his own interest in the early years of the Republic, and set himself to rescue the populace from its Babylonic babble. With patriotic devotion this Noah of our linguistic deluge followed in the footsteps of Dr. Johnson, who in 1755 had expressed the hope that by his dictionary he might “retard what we cannot repel, palliate what we cannot cure.”

The malady which Dr. Johnson wished to cure was not precisely schizoglossia, but linguistic change in general. He was realistic enough to see that codification would not inhibit it completely. The medical metaphor which I have been developing was anticipated in his Preface, where he wrote: “When we see men grow old and die at a certain time one after another, from century to century, we laugh at the elixir that promises to prolong life to a thousand years; and with equal justice may the lexicographer be derided, who being able to produce no example of a nation that has preserved their words and phrases from mutability, shall imagine that his dictionary can embalm his language and secure it from corruption and decay, that it is in his power to change sublunary nature, and clear the world at once from folly, vanity and affectation.” Nevertheless, he stoutly declared his desire to make an effort: “Life may be lengthened by care, though death cannot be ultimately defeated: tongues, like governments, have a natural tendency to degeneration: we have long preserved our constitution, let us make some struggles for our language.”

This point of view, so typical of the eighteenth century, is one which few American linguists would endorse today. In fact, the prevailing attitude is rather the opposite, that not only is schizoglossia not a problem, but that it is positively harmful even to try to eliminate it. Bloomfield’s remarks in the last chapter of Language pointed the way, which has been well-trodden by his followers, including
such eminent scholars as Charles C. Fries and Robert A. Hall, Jr. Hall exhorted his readers in the previously cited book to "abandon entirely the old dogmatic, normative, theological approach of traditional grammar and of social snobbery; and to substitute the relativistic, objective approach of scientific study and analysis." In another passage he makes an even more sweeping condemnation of the concern of the rhetorican with what is "right" and "wrong:"

"The merit of what a person says or does is not in any way affected by the way in which they say or do it, provided it is the most efficient way of saying or doing it; and to accept or reject someone just because of 'correct' or 'incorrect' speech is to show oneself superficial, lazy, and snobbish."

It will be my contention in this paper that the case of linguistic relativism has here been vastly overstated, and that there is no warrant in linguistic science for the wholesale condemnation here made of normative grammar. In his eagerness to neutralize the ill effects of Miss Fidditch's rigidity, Hall has thrown out the baby with the bath. It takes only a modest amount of discourse analysis to show that in these two passages the word "normative" has been associated rhetorically with pejorative terms such as "dogmatic," "traditional," and "snobbery," while "relativistic" has been associated with favorable terms like "objective" and "scientific." It also seems unrealistic to declare that merit is unrelated to the way in which something is done. Many would hold that the manner of doing may be more important than the doing. "Correctness" may not be synonymous with grace or charm, but together with these it is a significant element in what we think of as civilized behavior. For linguists as a group to put themselves in a position of opposing normative standards is to invite a charge of cultural barbarism. In recent discussion of Webster's Third New International Dictionary it has even been contended that "structural linguistics" is behind its supposed betrayal of standards.

It needs to be clearly understood that "scientific" is not necessarily identical with "tolerant." A plea for tolerance may be laudable from a moral or ethical point of view, but does not of itself follow from the premises of science. A botanist may have private opinions about the plants he studies, such as that some taste better or are more sightly than others, but in expressing these he is not speaking

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4 Dwight MacDonald. New Yorker, 10 March 1962.
as a scientist and his opinions have no more validity than anyone else’s. The problem of linguistic correctness involves dimensions of human behavior that are not provided for in the models which linguists usually build. I venture to go so far as to maintain that when Robert Hall tells the non-standard speakers that “there is nothing inherently wrong with your language,” he is no more scientific than Miss Fidditch. He is doing his bit to eliminate snobbishness, which does honor to his good heart, but he is not being either objective or scientific.

The conflict between the dual role that linguistic scientists are sometimes called to play has been painfully evident in recent years in Norway, where schizoglossia is not endemic, but epidemic. Although there are two official written languages, it is rather a case of schizoglossia than of diglossia, since these are little more than divergent dialects of one language. The official government policy during the past 25 years has been to promote the fusion of these two languages into one compromise norm. In 1952 a Language Commission was established by act of parliament which should advise the government. The directives given to this Commission declared that its function should be to give this advice “on the basis of scientific research” and thereby “promote the mutual approach of the two written languages on the basis of Norwegian folk speech.” The humanistic faculty of the University of Oslo at first refused to nominate representatives to the Commission on the plea that this formulation committed them to a particular linguistic policy which would be inconsistent with their freedom as scientists. In its comment on this stand the Ministry of Education emphasized that “a distinction must be made between linguistic research and linguistic normalization and guidance. The latter has to build on scientific research and take its results into account, but is not in itself a purely scientific problem. It is in equal, or greater, degree a national, social, or practical-pedagogic problem.”

The distinction made by the Ministry of Education’s adviser is one that it would be hard for any linguist to reject once he thinks more closely about it. In countries where he is asked to assume partial responsibility for language normalization, he has to face the same conflict of conscience as the atomic scientist in our country. No matter how well versed he may be in linguistics, he cannot then plead that everyone’s language is equally good or that it does not matter how a thing is said or written. Every dialect or every lan-

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5 Stortingstidende 1950, St. prp. nr. 1, Tillegg nr. 3.
language may be equally entitled to exist in a historical sense and
equally capable of expressing what its users wish to say. But within
the nation Hall's goal of "the most efficient way of saying or doing"
something is precisely promoted by uniformity of code rather than
by diversity.

It is along this dimension that our understanding of linguistic
normalization must proceed. If we do not keep our gaze one-sidedly
turned on the linguistic structure itself, but lift up our eyes to see
the society in which we really live, I think we have a model on which
we can build. Linguists seldom consider the nature of the pressure
that creates the high degree of uniformity among the speakers of
what Bloomfield called a "speech community." Anyone who has ob-
served the process of child learning of language will not fail to note
the numerous instances of mutual ridicule and intolerance on the
part of the still untutored savages. Schizoglossia is rooted out among
them by constant correction, which goes far beyond the minimum
needs of communication and virtually insists on identity of code. If
it were not for this kind of insistence, there would be no language
structure and no language history.

The introduction of writing made possible the infinite extension
of the language community beyond the immediate reach of the voice.
It made possible the building of nations and empires, which in their
turn became extended speech communities. Within these the pres-
sure against schizoglossia was directed above all at the normalization
and standardization of writing, but with the growth of other means
of communication through travel, it came to embrace also speech at
least of those who travelled. Only by reducing what Hockett has
called the noise of the code could the institutions of modern societies
be built. From the prime community of children and savages over
the secondary communities of classes and professions to the great
states of modern times the basic model of communication remains
the same, and the need for uniformity of code wherever communica-
tion is to be rapid and unimpeded is constant. The role of the writ-
ten word in this connection has barely begun to be understood. Per-
haps even the much reviled "spelling pronunciations," which Bloom-
field (p. 501) called "ugly," may be found to have their virtues.

It seems to me that all the activities of rhetoricians and norma-
tive grammarians, from Samuel Johnson to the lowliest schoolmarm
in American rural schools, need to be reevaluated in terms of this
model. Dialects, whether regional or social, have their charms, but
they hamper communication by calling attention to features which
which either are or ought to be irrelevant to the message. They label
their man by his social history, and their maintenance is often advo-
cated precisely by those who wish to maintain a snobbish distinction
of class. If dialects are to be tolerated, the teaching of tolerance
must begin with other and more basic features of inequality in society
than the purely linguistic one. In spite of the evidence piled up in
Fries's American Grammar concerning the failure of the schoolmarm
to impose her norm in certain areas, I am convinced that the compara-
tive uniformity of American linguistic usage has been greatly facili-
tated by her activities. There is no nation in the world where the
dictionary has entered daily life to the extent of ours, or where the
teaching of "correct" grammar has touched as many lives. It is not
difficult to see in this activity a reflection of the basic faith of Amer-
icans, however unrealistic it may have turned out to be, in equality
of opportunity for all. In other countries one could learn the best
usage only by associating with an aristocracy, which generally meant
being born into one; here culture could be learned from a book.

There is an interesting difference of attitude among the Scandi-
navian countries in this respect. In Sweden the firmness of the writ-
ten standard language has led to its dominance over speech. While
the colloquial standard in many ways escapes from this dominance,
the formal standard is strongly marked by it. While there are regional
varieties, a recent investigation of the school practices showed that
they were being reduced. Those pronunciations which come closest
to the spelling are favored. In Norway the opposite condition pre-
vails. The fluidity of the written standard has led to widespread
toleration of dialectal speech on the stage and in private life. But
the effects of widespread communication are nevertheless apparent
in the mutual adjustment of speakers from various parts of the coun-
try to one another. And one may surmise on the basis of various
tendencies that if once the written norm is unified, a concerted effort
will follow through the schools to impose a common standard of
pronunciation.

In conclusion, then, I note that there are two courses one can
follow to meet the problem of schizoglossia in modern society. Lin-
guists who do not regard it as a problem may of course take a laissez-
faire attitude to it. But they cannot claim that they do so because
this is the scientific position. The scientific position is to recognize
that a problem exists, that it needs research and study in terms of
social goals, and that mere toleration is not really a remedy. Nor-
malization, which aims to provide a common code for those who need one, is a remedy, and linguists can make a contribution by seeing to it that it is a good one. It would be nice if we could persuade polite society to accept Eliza Doolittle as she is, but in our heart of hearts most of us would prefer to associate with her after Dr. Higgins has straightened out her aches.
DISCUSSION

J. C. THOMPSON: (U. S. Government) I want to add one more possible way of looking at this. After Henry Higgins had straightened out Eliza Doolittle's pronunciation, he had to correct her dress and her manners before she was acceptable in polite society. I think that is both accurate and illuminating to consider linguistic norms in the same light as we consider norms in dress and etiquette. They are very definitely there and if you don’t observe them you will find yourself with a very bad social black eye. But Miss Fidditch has always considered that the norms of grammar were much more immutable than the norms of etiquette, and it is perhaps an attempt to correct this attitude which led Hall and others to over-correct it, by, as you say, throwing the baby out with the bath water.

CHARLES BIDWELL: (University of Pittsburgh) I think Prof. Haugen has quite correctly stated that the question of normalization vs. laissez faire with regard to regional social variations of speech is not a question of scientific attitude. But I can’t see that he has proven that there actually is a question here. He apparently has an aesthetic preference for normalization. I have an aesthetic preference for diversity. I think this is perhaps a question of individual philosophies.

MR. HAUGEN: I don’t have any comment on Mr. Thompson’s statement, but I do have a comment on Mr. Bidwell’s. I think I said that dialects have their charms, and this indicates my aesthetic preference for diversity, but I prefer it to be, in some ways, an individual diversity, rather than a dialectally determined diversity. In other words, I prefer the inevitable diversity of individuals within a group, rather than an individual’s leadership within a group, which imposes upon him the prejudices of other groups with whom he comes into contact. However, it is evident that this is not just a matter of aesthetics. I was trying to suggest that the development of norms, and whatever fumbling efforts have been made by Miss Fiddich or by Miss Fiddich’s teachers to create such norms, is simply an extension of the normalization that goes on in every speech community, whether a relatively primitive one, or a more civilized one. Where people talk together they talk alike if they continue long enough. This is clearly a result of a basic feature of communication, which

1 R. H. Hall, c. Leave Your Language Alone.
we can study in Hockett's book\textsuperscript{2} by studying the models and the theory of communication, namely that uniformity of code leads to more rapid encoding and decoding than is possible when we are uncertain what the person said because we did not hear it. I have that difficulty, when listening for example, to British stage plays, and I lose a great deal of what is said because there is a deviation, a difference in code between British English and my English.

PAUL GARVIN: (Thompson Ramo Wooldridge) One of Bloomfield’s great contributions was, among others, to have pointed out that sub-standard varieties of a language deserve study. The great virtue of the present paper is that it points out that the study of sub-standard varieties does not exclude normative varieties of the language as a fit subject for study. I think that the role of the linguist in the normalization process, is, perhaps, one of attempting to bring about a certain realism in the sense that the norm which one would like to see taught in the schools should be one which is actually in use and not one which exists merely in the imagination of certain people. This, I believe, is a scientific problem because it is a matter of ascertaining what exactly is good usage in appropriate sources from which the norm is to be taken. It involves more than just linguistics. It also involves, as was properly pointed out, sociology, and anthropology, just to mention two.

WILLIAM STEWART: (Center for Applied Linguistics) It is true that Hall has been criticized time and again, and this is not the first time that he has been accused of having thrown the baby out with the water, in his book, \textit{Leave Your Language Alone}.\textsuperscript{3} However, his comments are more to the point in another book which is less read, but is a good book, that is, \textit{Hands Off Pidgin English}.\textsuperscript{4} It still has the admonishing attitude which is so typical of Hall, but it brings out a point which should be well taken. Namely, that it is one thing to say that, in an area which is already linguistically unified to a certain extent, maximum unification under the norms of a single, prescribed set of linguistic norms may be desirable. But it is another thing again, in a linguistically pluralistic situation, to try to apply indiscriminately a single set of norms to what are structurally fairly autonomous linguistic forms. Attempts of this type have led to attacks on most of those pidgin and creole languages which are lexically related to European standard languages, but which, in fact,

\textsuperscript{3}op. cit.
have grammatical structures which make them different enough from the latter to require separate formalizations. This is, for example, what Hall describes as having happened with Melanesian Pidgin English in the book I refer to.

RALPH ALLEN: (Department of the Army) Prof. Haugen needs no defense from me, but in this matter of normalization, we, the people identified with the Armed Services, have a special problem. That is, the codes of large and sophisticated groups of speakers of English are the codes of British English, not American English, and we find it very definitely necessary to create a reflex response identification in American English as opposed to British English. My associates from the Air Force can bear testimony that when you are in a jet trainer headed for something large and solid at about 450 knots, there is no time to go into an analysis, syntactic or otherwise, to determine what the British English phrase means in American English or vice versa.

MR. HAUGEN: I think this is really an extreme example of what I was thinking of, and I am grateful for this example. If we all are going to be reduced to traveling at jet speeds, we obviously will have to have one common language for the entire world, one code in which we can communicate. Whether this is British English or American English will remain for the future to determine.

I would like to comment on Mr. Stewart's point with regard to Hall's Melanesian book which I will grant that I have not read so that I am handicapped in commenting on it directly. But I accept his statement and I think that in a down-to-earth discussion, Hall and I would not disagree very seriously. It is more the aggressive tone in his other book Leave Your Language Alone, to which I reacted, and which I think should not be allowed to stand as the last word of scientific linguistics' comment on this problem. I think the problem is far subtler than Hall presents it, far more difficult to handle. I was particularly driven to this problem by the review in The New Yorker of the Webster's Third New International Dictionary in which structural linguistics is specifically the villain of the piece in "the betrayal of English," "the string untuned." I agree that Miss Fiddich's norm was not always realistic, as Paul Garvin pointed out. Nevertheless it was a norm, and most people, I think, can be taught anything as long as you teach it to them in simple terms. If you tell them that 'ain't' is wrong, this is a much more effective way to

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Dwight MacDonald. New Yorker, 10 March, 1962.

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teach it than to say, "well, over in Pennsylvania there are a few cultured people who say 'ain't' in the first person but never in the third." This is essentially what Webster's new dictionary says, namely, that a number of cultured people in various parts of the country say 'ain't' in the first person, but in the third person, or second person, it is non-standard or sub-standard. Perhaps it is a large question to decide whether a dictionary should be normative or scientific in such cases and just what the difference is between the two.
Today, when so many new states are emerging—many of them with bilingual problems affecting their educational systems, the case of Ireland has significance broader than might first appear from the position of the country and the size of its population. After centuries, independent native rule was restored forty years ago in three quarters of Ireland, and since then bilingualism has been a major feature of our educational system.

The languages we have to deal with are Irish and English. The Irish language is a Celtic one. It is closely related to Welsh and differs from the Gaelic spoken in Scotland only as one dialect differs from another. Our English varies from the speech of the educated townsman (which is quite different from British English) to a dialect more heavily influenced in phonology and idiom by the Irish it has superseded. Something like what Richard Diebold calls *incipient bilingualism* is, therefore, quite widespread. This, surely, is a factor operative in the attainment of results through our educational system that appear to me to be greater than might be expected considering the methods used and the extent of their use.

When modern nationalism began in England, one of its first results was the destruction not only of the native political system in Ireland, but of the academic system as well. By 1700, the land was ruled by an English-speaking ascendancy. The majority of the population remained Irish-speaking for a century and a half longer. But Irish, which had been the medium of social communication, of law, medicine, history, philosophy, piety, and literature for a thousand years, was left without the professional bodies that had been officially entrusted with its cultivation as far back as historical records go. The centuries of development and expanded use of the other vernaculars of Europe were those in which the most determined effort was made to stamp out Irish. So the language was rendered deficient as a medium for use in the modern world.

It would be naive to imagine that the prolonged operation of a number of forces against Irish ceased to have effect at the mere at-
tainment of political liberty. No matter what national leaders or government officials may have wished to do, they could not, under our political system, ignore the effect these forces have had on the mass of the people and on academic, professional, and higher business sections of the community. Consider what these forces were: the depressed social position of the Irish-speaking population lowered the status of the language; the spirit of nationalism, which arose at the end of the eighteenth century among the ascendancy, spread the new political doctrine of Republicanism through English; the Act of Union in 1800 had brought the country more than ever before under direct rule from London, so that virtually all accepted what was English in speech and manners as the ideal of cultural attainment; before famines and emigration decimated the Irish-speaking districts, when seventy per cent of the people still spoke Irish, the British Government established in 1831 an elementary school system which ignored the native language and compelled the use of English; at the same time both Catholics and Protestants aspired to secondary and university education which aimed at producing worthy citizens of the United Kingdom and the Anglo-Saxon world.

By the end of the last century the districts in which Irish was the language of daily use had become very small. A cultural movement was begun to save the remnants of the heritage of oral and written literature, to teach Irish to those who did not know it, and to restore the language as a medium of communication, of learning and of literature. The shrinking of Irish-speaking districts has been slowed up, but never halted. Today there are only seventy thousand native speakers in these districts. On census forms of the independent part of Ireland half a million people have claimed to be Irish speakers. But the population of the State numbers three million, and there is another million or so in the North-Eastern area still outside the jurisdiction of the Republic of Ireland.

In this paper on bilingualism in education in Ireland my remarks about the situation in that North-Eastern territory must be brief, for want of reliable data. The government there does not favour the restoration of the Irish language. But it may be taught in schools as an optional subject, and there are realistic standards of teacher-training and proficiency in communication to be reached if the courses are officially recognized.

At an early stage in the movement for the restoration of the language, people belonging to that movement became influential in
the long-established movement for political independence. So it happened that with the coming of independence in twenty-six of the thirty-two counties, one of the principal national aims was the restoration of Irish as a means of ordinary communication all over the country. Successive governments have pursued that aim for the past forty years. But note that it was at the beginning a cultural aim, which was later adopted by the political movement, and it is one which was imperfectly understood by many of those most active in military, political, and economic life both prior to and since the foundation of the State. The declared aim of various governments in the matter of restoring the Irish language appears so difficult to anybody acquainted with the factors involved, that it would be expected to require at least as much expense, research, and adjustment of administration as was provided for national defense, electrification, housing, exploitation of fuel and mineral resources, economic expansion. Nobody could seriously allege that such efforts have been made on behalf of the language. So it is that bilingualism is more a feature of the schools than of life outside them, and this is having its inevitable repercussions on the work of the schools.

Before I give details of the use of Irish as well as English in our schools, some consideration must be given to the internal development of Irish as a medium. What has been achieved (by means which can only be described as haphazard) is indicated by this extract from a report on a meeting of Convocation of the National University last June: “All were agreed on the adequacy of the language as a medium of instruction even in technical and scientific subjects.” The worth of this claim may be gauged from the fact that full university courses through the medium of Irish are available in Latin, Greek, Mathematics, Geography, History, Celtic Archaeology, Education, Experimental Physics, Chemistry, Botany, Mathematical Physics, Economics, and Commerce. The reduction of the full philosophy course (which was given through Irish until a few years ago) to certain minor branches of the subject is due circumstances other than the state of development of the medium, because the vocabulary for that area has been published. The students who follow courses through Irish compete successfully with those taught in English. Many of them have won travelling studentships as a result of competitions. Quite a few have studied or are studying here in the United States—there are at least two in the Graduate School of Georgetown at present.
About ten years ago Dr. Tomás De Bhaldraithe, who is now Professor of Irish at University College Dublin, examined a check list of eighty modern terms prepared by Ralph E. Turner of Yale as a test for assessing the modernization of certain Asiatic languages. Only three of the eighty terms lacked corresponding words in Irish—these were demography, neurosis, and ... regimentation! He remarked that most of the new words were not got from the source which has provided 90% of the vocabulary of the language as it is used today, namely the people living in Irish-speaking districts, but from the professional classes (some native speakers of the language, some not). Terms were obtained in all of the following six ways: by giving a new meaning to an old native word (e.g., *rachmas* for "capital"), by the revival of an old word (e.g., *mortlaiocht* for "mortality"), by the revival of an old word which has been given a new meaning (e.g., *turgnamh*, which once meant "preparation," used for "experiment"), by making a new word from native elements (e.g., *bunseacht* i.e., basic law for "constitution"), by making a new Irish word from a foreign root (e.g., *leictreachas* for "electricity"), by giving an Irish form to a foreign word (e.g., *leictreon* for "electron").

Expansion of vocabulary is the aspect of development most easily discussed in a brief sketch like this. Not less important, however, is the greater flexibility of expression which has been attained (by mutual borrowing among the dialects and enrichment from the style of the old literature, as well as by interference from English): this has come about through the use of the language in translations, news-reporting, teaching, and modern entertainment. A simplified uniform spelling system and a standard grammar which draws on all three dialects have been produced and are generally accepted. Recently a standard form of pronunciation has been prepared and proposed for use on television.

When I spoke of university courses through Irish, my intention was to illustrate how much the language has been readjusted to suit modern education. The extent of the use of Irish in universities is small. In the Republic of Ireland there are five institutions at university level. Four of them are associated in the National University of Ireland which, at its foundation in 1908, was compelled by pressure from the Language Movement to list Irish as a compulsory subject for matriculation. In only one of those colleges is Irish used to any appreciable degree—University College Galway, where the courses I have already mentioned are available. Individual professors teach some subjects through Irish in Cork and at St. Patrick's...
College, Maynooth. The largest of the colleges, University College Dublin, completely ignores Irish as a medium and has rejected requests for courses in Irish to facilitate students who do their elementary and secondary schooling through that language. Ireland's oldest university, Trinity College Dublin, made an effort to meet this need some years ago, but the attempt seems to have failed for reasons outside the control of that university.

Certain faculty members and large numbers of graduates of all Irish universities (including the Queen's University of Belfast) have ceaselessly supported the use of the language in education and public life and have produced research work, textbooks, and literature in Irish. Their support is more than counterbalanced by a number of academic and administrative people in positions of great influence who resist bilingualism consistently. This has repercussions on some young academic people and on all the lower stages of education which depend on the universities for training and research.

Secondary school teachers depend entirely on the universities for their training. Those who cannot attend the Galway college are deprived of the experience of themselves being taught and doing directed work through Irish during their university training in the subjects they are later expected to be able to teach through Irish!

Irish secondary schools provide a general education of an academic nature. Religion and varying degrees of English and Irish provide a unifying element in a system which allows a degree of variation between Ancient Classics, Modern Languages, Mathematics, and Scientific subjects.

These secondary schools are private institutions. Many of them, particularly those of religious orders of priests, of some of the congregations of nuns and of Protestant denominations, inherit traditional attitudes from nineteenth century Ireland which may in part explain some of the following facts.

The government has never imposed the use of Irish on these schools. It has been insisted, however, that Irish be taught as a subject in secondary schools which are officially recognized by the government, and the language is demanded as a subject for those receiving State certificates of secondary education. Recourse was had to offering grants to schools teaching subjects through Irish. Fifty-six per cent of the secondary schools teach no subject through Irish.

Almost twenty per cent of the schools teach all subjects through Irish, and the remainder teach some subjects through that language.
Whatever the explanation, the results obtained in competitive examinations, far from showing pupils of these schools to be inferior, rate them at least as high as those from schools using English only as a medium. (Obviously this fact must be interpreted with due consideration to the kind of students attending such schools, the standard of teaching, and so on.) Notable among the educationalists who have adopted bilingualism are a few orders of teaching sisters—the Dominicans and the St. Louis nuns in particular, the diocesan clergy in their seminaries (for layfolk as well as those intended for the clergy), and above all the Irish Christian Brothers who are undoubtedly the greatest single educational force, throughout the past century which has favoured everything distinctively Irish, linguistically and culturally. The significance of the situation in most of the universities and secondary schools is that the attitude of academic people and professional and influential business men is often influenced by the attitudes of their college training at these two levels.

Before dealing with the primary schools, mention should be made of a system of technical post-primary education which was throughly reorganized since the attainment of independence. "Vocational Schools," as they are called are taught completely through Irish in the Irish-speaking districts. About a third of the courses in domestic economy are in Irish. All outside Irish-speaking areas have three hours instruction per week in the Irish language as a subject. A favourable attitude towards bilingualism in this system is somewhat hampered by dearth of linguistically able experts in carpentry, metalwork, engineering, and so on. This can gradually be remedied and at present the various Vocational Education Committees give scholarships to some 500 students each year to spend a month learning Irish in Irish-speaking districts.

The state established in 1922 inherited as an elementary school system that which was planned almost a hundred years earlier to supersede Irish with English.

Our primary schools have a double purpose: to give the instruction usually imparted to children up to the age of fourteen and to teach Irish. Regarding the second purpose this quotation from official directives to teachers is clear enough: "Its aim is frankly and unequivocally to make Irish speakers of the children of the Galltacht (English-speaking areas) so that by the age of 14, they may be able to express themselves freely, fully, and correctly in the new language.
In the Gaeltacht (Irish-speaking areas) its aim is to perfect the vernacular into as adequate an instrument for all forms of self-expression as the English course aims at doing for English-speaking children. . . . (An Roinn Oideachais: Nótaí d'Oidí, p. 2).

The training and standard of primary teachers is uniformly excellent. Many of them are native speakers of Irish. The number of them holding certificates of competence to teach bilingually has risen from 10% in 1925 to 80%.

Irish is introduced as a subject and medium of games, singing, counting, etc.; in the infant classes outside the Irish-speaking areas, English is introduced to native speakers of Irish at about the age of eight. Instruction completely through Irish is continued throughout the primary course in only 8.6% of the schools—and this includes the Irish-speaking areas. Calculations made by the Council of Education ten years ago revealed that less than 5% of the schools in English-speaking areas did all their work through Irish, but that in partially Irish-speaking areas 16% of the schools used Irish only as a medium. In 42% of the schools certain subjects are taught through Irish for the entire course.

Bilingualism in the primary schools has been successful in preparing a percentage of pupils able to pursue full education to university level through Irish, where such has been provided. Frequently, however, the children have not full facility in communication in Irish by the time they reach full fourteen. At the same time, it should be noted that large classes in places and failure to provide planned material for the teachers, make for tremendous variety in what is attained from school to school. Yet efforts so far have succeeded in giving virtually all under the age of fifty in the Republic some acquaintance with a language which remained utterly unknown outside Irish-speaking areas before the movement for its restoration began. There are probably some hundreds of thousands who could conduct simple conversation in Irish and a couple of thousand more who, though unable to speak Irish, could understand it. Poetry, drama, and scientific studies are being published in Irish both in books and periodicals and there is a public for them. It is a curious fact, however, that it is possible to live as a monoglot speaker (English) in Ireland and to be quite unaware that there is any force at all worth considering left either in Irish as a modern language or in the movement for its restoration, while tourists can be easily left unaware that it is truly the daily language of thousands; likewise it is
possible for others to spend their lives in bilingual circles—even in Dublin—and to fail to realize the amount of ignorance and misunderstanding of their aims which exists among the public.

So bilingualism in our educational system is a subject of continual debate between two minorities. On one side there are those who consider the effort wasteful and useless, particularly in the light of the demands being made upon us in economic matters and in general education standards. These people hold a view easily appreciated by the general public everywhere, and as some of those on this side of the debate are influential academic, professional or business men—particularly journalists—and as they write or lecture almost entirely in English (often for the practical reason that they know only English!) their view is the one better known both at home and abroad. On the other side you have equally sincere people who are at least as qualified to offer an opinion—more particularly since they are usually bilingual—who are not convinced that deficiencies in the economic or educational fields are necessary concomitants of efficient cultivation of our specific linguistic and cultural inheritance throughout the educational system. I would describe the attitude of the majority of the population, outside the two minorities, as one of benevolent apathy regarding the Irish language. But this may turn to something less favourable, under the influence of politicians seeking grievances to exploit, if means are not taken to ensure that greater numbers leave the schools with a true ability to use both languages for communication.

My conviction that a key to great improvement within the present system is research by methods of modern applied linguistics, so as to provide planned materials for our teachers, has brought me to the United States. Nothing of this kind has so far been attempted, so far as I am aware. Whether or not the attempt will lead us any nearer to a solution of the problem of bilingualism in education in Irish schools I leave to you, as specialists in this field, to guess, and to some future lecturer on the same subject, to tell.
DISCUSSION

EINAR HAUGEN: (University of Wisconsin) I was very much interested in this paper because of its parallel with what I am working on in the Norwegian situation, but the obvious differences are very great. The phrase that Father O’Huallachain used, “benevolent apathy,” seemed to me to be a very good expression for certain similar phenomena in Norway. It seems that in a country where language is a problem, one could perhaps make the generalization that only a minority of the people are really interested in that problem. The debates are generally carried on by small groups of people intensely interested in the problem, usually not for linguistic reasons, but for cultural, social, historical or patriotic reasons, such as in the case of the Irish, and I suppose the question that is on the lips of all of us is, what is the future of Irish? We can’t help but wonder, since, as you had suggested, the number of speakers of Irish is not actually growing, although the incipient bilingualism seems to be greater because of the schools. I seem to have read somewhere that the school-Irish is not entirely identical with the Irish spoken by the native speakers, that there is some difference of idiom or feeling between those two groups. Could you comment on that?

FR. O’HUALLACHAIN: Certainly there is a big difference between what is taught in the schools and what is found in the native speaking areas. Of course that varies, and it is impossible to give a general picture. Linguists especially find accounts of this language written by other linguists, of a certain interest from a historical point of view, and they lament the distortion of this ancient language in a cause with which they feel comparatively little sympathy. The Department of Education of Eire in its directives for teachers points out that there is a great need for more attention to what they call “phonetics.” But there is no specification given to the teachers about what precisely is needed. There has never been a contrastive study of the phonology of Hiberno-English and the target language. Therefore, the people put the burden on the government, the government puts the burden on the Department of Education, and the Department of Education by these directives has placed the burden firmly on the shoulders of the teachers while doing very little to provide the teachers with materials. The result is that there is certainly an imperfection (the degree of which depends of course on the teaching), on the linguistic ability of the learners, and the length of time they can continue with the language. The great movement that started a few years ago among University graduates, to send their children
to Irish speaking areas for three months, has had wonderful results. This should never be thought of as a class question. That is to say, you will meet fluent speakers of Irish in various branches of industry and you will also find people in academic circles who actively communicate in Irish. It is a very involved question, and the complaints about the destruction of the purity of the language could only be expected as a result of the inevitable interference. Of course, phonologically speaking, all the vowels are already Hiberno-English. We just lack two diphthongs /ye/ and /we/. In the consonants, there are difficulties because there are very important distinctions between palatalized and velarized sounds. Yet there is no system available of drilling the students and there is no study published which would help the teacher pinpoint the trouble. That is the situation with regard to interference.

A. Richard Diebold, Jr.: (Harvard University) Prof. Hagen suggested an analogy of sorts with the Norwegian situation with which he is working, and I believe that there is something similar in the Greek case which I deal with. But this similarity does not lie in the overseas community; rather, it lies in the immigrant community in the U. S. There is a great awareness of the successive loss of the Greek language through generations. Third generation Greek children very often acquire Greek as their secondary language. In the Greater Boston area a number of schools are being established, essentially parochial schools attached to the various Greek Orthodox parishes, where a great debate is going on whether or not to have the curriculum taught in Greek, or at least have it divided up somehow. I think the analogy fits better with this situation than it does with the Norwegian one, since there is a degree of mutual unintelligibility involved here, which I believe is quite different from the case in Norwegian, no matter what the non-reciprocal nature may be between the two.
The data on which the present paper is based were gathered by the field-workers, both linguists and social anthropologists, of the University of Chicago's projects among the Tzeltal and Tzotzil-speaking Indians of Chiapas, Mexico and among the Spanish-speaking non-Indians (Ladinos) of the same area (see Figure 1). There are considerable numbers of bilinguals in both groups, although the incidence of bilingualism varies greatly from place to place within the area. In the southern and southwestern portions of the territory, where there are large numbers of Ladinos, there are many Indian bilinguals and practically no Ladinos who know anything of the indigenous language. In the northern and, in particular, in the northeastern parts of the area, these proportions are reversed. There are very few Ladinos, and these are, for the most part, bilingual. The agents of acculturation in the north, are, in great part, the Ladinos who speak the Indian language. In the south, on the other hand, such agents are to be found among the Indians themselves.

What such general statements fail to specify, however, is why some Ladinos in the north and some Indians in the south are very poor, some very good, agents of culture change, and what are the specific indices, both linguistic and sociocultural, of such differences in individual personality, differences which account for the selection process whereby some are chosen to lead and some are not. It is the purpose of this paper, in the concrete linguistic, sociocultural, and personality data available on five individuals, distributed over this continuum, to attempt to discover such specific indices, and, having discovered them, to try to rationalize the particular roles which individuals so distinguished play in the life of their changing communities. I am especially grateful to the social anthropologists Charles E. Mann of Stanford University, María Esther Alvarez de Hermitte of the University of Chicago, and Marcelo Díaz de Salas of the Mexican National School of Anthropology, and to the linguists Gerald E. Williams of Stanford University, and R. Radhakrishnan, and Harvey B. Sarles of the University of Chicago for having made
MODERN DISTRIBUTION & LOCATION OF TZELTAL & TZOTZIL PUEBLOS
MODIFIED FROM J. BAROCO, 1959

APPROXIMATE LINGUISTIC BOUNDARIES

- MOYOS
- SABANILLA
- AMATÁN
- TILA
- PETALongo
- TUMALÁ
- YAJALÓN
- CHILÓN
- NUEVO SITALÁ
- BACHAJÓN
- SITALÁ
- SIVÁCA
- OCOSINGO
- GUAGUITEPEC
- TENANGO
- SAN MARTÍN
- ABASOLO
- SAN CARLOS
- ALTAMIRANO
- NUEVO SITALÁ
- SITALÁ
- SI VAC A
- GUAQUITEPEC
- BACHAJÓN
- OCOSINGO
- SOYAL6
- SAN GABRIEL
- IXTAPA
- ZINACANTÁN
- SAN CRISTÓBAL
- HUASTÁN
- DE LAS CASAS
- SAN FELIPE
- CHANAL
- CHINAN
- TOTOLAPA
- CHIAPILLA
- AMATENANGO
- TEOTITLAN
- AQUACATENANGO
- NICOLÁS RUIZ
- VILLA LAS ROSAS
- SOVATITÁN
- SOCOTENANGO
- CANDELARIA
- COMITÁN
- HIAPA
- DE CORZO
- SAN CRISTÓBAL
- HUISTÁN
- LAS CASAS
- EL ROSARIO
- TEPICHCA
- AMATENANGO
- INDIANACO
- ROSARIO
- VILLA LAS ROSAS
- SOVATITÁN
- SOCOTENANGO
- CANDELARIA
- COMITÁN
- HIAPA
- DE CORZO
- SAN CRISTÓBAL
available to me the sociocultural, linguistic, and projective test materials on which this study depends. I have made use of taped 200-word vocabularies, both for Spanish and for Tzeltal-Tzotzil, and of the responses (in Spanish) to two types of cultural projection tests (the conventional T. A. T.'s, and a photo-test for cultural perception devised especially for the Chicago Chiapas projects by Murial Eva Verbitsky de Hunt of the University of Chicago). In addition, I have had available to me a variety of socio-cultural census data on the five individuals in question (one Ladino, and four Indians), all of whom are bilingual in varying degrees which will be here carefully specified.

Since the features which I have here isolated as indices of bilingual adaptation are in considerable number phonological, I present first, in brief outline, the phonological systems (Figures 2-11) of the dialects involved, followed by the lists of features, so that the factual selection among the features found in these systems in the usage of each of the individuals under study as well as the rationale of such selection may be made clear.

*Figure 2*

**OCOSINGO TZELTALZ**

\[
\begin{array}{cccccccc}
\text{p} & \text{t} & \text{c} & \text{k} & \text{?} & \text{i} & \text{u} \\
\text{p} & \text{t} & \text{c} & \text{k} & \text{e} & \text{a} \\
\text{b} & \text{d} & \text{g} & \text{s} & \text{x} & \text{m} & \text{n} & \text{a} & \text{á} \\
\text{w} & \text{y} & \text{C} & \text{V} & \text{C} & \text{V} & \text{C} & \text{V} & \text{C} \\
\text{C} & \text{V} & \text{h} & \text{C} & \text{V} & \text{h} & \text{C} \\
\text{C} & \text{V} & \text{P} & \text{C} & \text{V} & \text{P} & \text{C} \\
\text{C} & \text{V} & \text{V} & \text{C} & \text{V} & \text{V} & \text{C} & \text{V} & \text{C} \\
\text{C} & \text{V} & \text{h} & \text{V} & \text{C} & \text{V} & \text{S} & \text{C} & \text{V} \\
\text{S} = \text{Semivowel} & \text{C} & \text{V} & \text{P} & \text{C} & \text{V} & \text{C} \\
\end{array}
\]
The Ocósingo Tzeltal (Figure 2) of Informant No. 1 (Ladino) contains, among others, the following phonological peculiarities which stamp it as non-Indian:

1. absence of glottalization (in all positions);
2. absence of glottal stop (in CV?CVC);
3. absence of stop articulation (in /b/);
4. assimilation of e.g. [s] to [ž] before voiced item;
5. replacement of [V?b] by [Vb]; of [?βφ] by [vf].

The following represent instances of hyper-Indianism:

1. [. 'xaʔ. ka.] instead of normal [. xaʔ. ?a.] ‘agua’;
2. [. t en. 'tsum.] instead of normal [. ten. 'tsun.] ‘chivo’;
3. [. tu. 'mirj.] instead of normal [. tu. 'min.] ‘algodón’;
4. [. ta. 'kin.] instead of normal [. ta. 'kin.] ‘seco’;

On occasion, however, other, genuinely Indian, features do appear:

1. utterance-final glottal stops (regularly);
2. glottalization (properly placed) (sporadically);
3. [ʔφ] where it should occur;
4. OVxC (regularly) (but with a strongly fricative [x] in this position.

One or more of these genuinely Indian features occurred not as reactions to the linguist’s elicitation formulae, but rather as echo reactions to Indian responses to the formulae. In such cases the chain Spanish-speaking elicitation—Tzeltal response in Spanish context was broken, and a genuinely Tzeltal reaction was in order.

Figure 3

OCOSINGO SPANISH

p t č k
b d g
f s š x
i u
e o
a
The Ocoseno (?) Spanish (Figure 3) of Informant No. 1 (Ladino), as evidenced by the text of the informant's reactions to the individual photographs in the PC (hototest for] C[ultural] P[erception], departs very little from the Central Mexican phonological norm, although there are numbers of grammatical and lexical provincialisms and an occasional vulgarism. Among provincialisms, we note:

1. ad-verbial muy (no muy se distingue);
2. ir a without a (se va casar);
3. feminine en -anta (ó alguna acompañanta ó alguna visitanta).

As a vulgarism we observe the pronunciation [. tsek.'tal.] in place of the usual [. tsel.'tal] in de raza tzectal.

Inspection of the content of Informant No. 1's Spanish-language PCP reactions produces clear evidence of his Ladino identifications:

1. his use of the term indito for the human figures in the photos;
2. his use of the term choza for their houses;
3. his use of the term indumentaria for their clothing.

On the other hand, his constant use of the diminutive in -ito or -ita evidences his kindly tolerance and good will (dos inditos, su chocita, este chamaquito, está contentito, se conoce muy apuradito, chamulitas, son inditas) toward Indians, as does his use of señora of an Indian woman. Other words and phrases for identifying Indians are typically Ladino in their avoidance of more direct labels (indígenas, de raza tzectal, de raza indigena, un chamaquito . . . así de la región, es del carácter de ellos). His almost exclusive reliance on clothes as providing the distinguishing line between Indian and Ladino is again typical, although it gets him into trouble when he tries to reconcile Ladino trousers and shirt with bare feet and a kerchief tied around the head. Shift of clothing from the Indian to the Ladino norm is characterized as progressive (más adelantados, una familia bastante
The use of pewter bowls is a mark of progress (por lo tanto, ya están bastante adelantados). The refusal to recognize, among the photos, either of the curing ceremonies as such, on the other hand, indicates Indian-Ladino common sentiment on witchcraft and curing. Full identification with the non-Indian (ahorita si se ve que está muy contento), however, is manifest in Informant No. 1's reaction to the last of the twenty PCP photos.

*Figure 4*

**PINOLA TZELTAL**

\[
\begin{array}{ccccccccccc}
\text{p} & \text{t} & \text{č} & \text{k} & \text{?} & \text{i} & \text{u} \\
\text{p} & \text{t} & \text{č} & \text{k} & \text{e} & \text{o} \\
\text{b} & \text{d} & \text{g} & \text{s} & \text{x} \\
\text{m} & \text{n} & \text{a} & \text{á} \\
\text{w} & \text{y} & \text{CVx} \\
\text{l} & \text{CVp} \\
\text{r} & \text{CVč}
\end{array}
\]

The Pinola Tzeltal (Figure 4) of Informant No. 2 (Indian) contains the following special characteristics which mark it as innovating and hispanicizing:

1. Complete lack of noun-classifying numeral prefixes (with only one exception, the personal *tul*, in only one instance);
2. Almost exclusive use of [b] allophone of /b/ (only one instance of /b/ in utterance final);
3. Use of -/Vl/ final (adjectival) instead of nominal root initial members of the Tzeltal equivalents of Spanish adjective-noun phrases (*čahal lum, tohol beh, sikil ṭakabal, yačil nah*).

Other features may be interpreted as indicating a tendency toward slavish literal translation (*yakat yučil ha?:* estás bebiendo agua, *ha*...
the grasp of the second language is, however, not always adequate (tokal lom ?ip: not cayó una lluvia fuerte but hay una neblina fuerte).

Figure 5

PINOLA SPANISH (INDIAN)

\[
p t \hat{c} k \quad i \quad u \quad pr \quad tr \quad kr \\
b d g \quad e \quad o \quad br \quad gr \\
f s s x \\
m p n \\
w y \quad a \quad á \quad Cía \quad CVx \\
l \quad Cío \quad CVVx \\
r \quad í
\]

The Pinola Indian Spanish (Figure 5) of Informant No. 2, notwithstanding his tendency toward innovation in Tzeltal, departs quite widely from the Central Mexican norms for Spanish:

**Phonetic Features:**

1. the voiceless stops are usually aspirated ([k'] amino largo, [k']i) (but occasionally not; [k']acho;
2. post-nasal voiced (está Usted cor[d]ando zacate) or lenis (tem[pl]lo);
3. the /ɾ/ is fricative (cerro, barriga);
4. the final vowels are followed by voiceless echo vowels (grande [ẽ], zope [ẽ]);
5. utterance final /a/ is occasionally centralized (mi mano derech[a:].)

**Morphophonemic Features:**

15. nasals usually fail to be homorganic with following stops or fricatives (u[n] perro, u[n] piojo) but occasionally do assimilate (nara[n]jas, u[n] caballo);
19. cluster /gr/ replacing /dr/ (una piegra, un cegro);
20. cluster /xu/ replacing /fu/ ([x]uego instead of fuego);
(21) cluster /ie/ replacing /ue/ (carne del pierco);
(22) ia or io vowel clusters instead of illa or illo; (mi rodía, amario, martio, un amario, gargantía) (for rodilla, amarillo, martillo, armadillo, and gargantilla respectively);
(24) loss of [d] (¿onde estabas? se espientan);
(25) loss of initial vowel ('stá seco, 'l ombligo);
(26) loss of final vowel (lech' de vaca);
(27) loss of final syllable (mi mano zur' instead of mi mano zurda).

Grammatical Features:

(35) plural endings are omitted (somo' chiquito', dos pájaro', 'stán buenos, dos mes', cuatrociento'), as are other final s'es ('stá lejo');
(36) plural number in possessive adjectives refers to plurality of possessor rather than thing possessed (sus ojo' Ustedes, sus hombro');
(38) possessive phrases lack both the article preceding the thing possessed and the preposition de which connects it with its possessor (polvo los camino', tronco los palo');
(39) feminine gender concordance fails (no andan los criaturas, un hoja verde, manta blanco, está enfermo su mujer);
(40) special forms of the second person singular of verbs (those compatible with the pronoun vos,) frequently occur (vos sos, no sabés, dormite) but not to the exclusion of the usual Central Mexican forms compatible with the pronoun tú (¿qué quieres?).

Lexical Features:

(60) special lexical items (calca del palo, mi pescuez, un lucero, cayo una helada doble, mi carne, un zope, tzilica, murciégalo) (for corteza, cuello, estrella, fuerte or espesa, cuerpo, zopilote, and chilacayote, respectively).

Nine items (5, 19, 20, 22, 24, 25, 26, 40, and 60) are characteristic of local Indian Spanish, five items represent special solutions to difficult problems of shift from Indian speech habits to Spanish speech habits, and six items (1, 3, 6, 8, 15, and 35) are clearly transfers of Indian linguistic habits into Spanish speech.

Inspection of the content of Informant No. 2's Spanish language T.A.T. reactions produces clear evidence of his distance from the Ladino world:

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(1) he fails totally to recognize the violin and the sheet music in Picture No. 1 and concentrates instead on the alleged sadness and sleepiness of the figure;

(2) he fails to recognize the books carried by one of the two women in Picture No. 2;

(3) the female figure in Picture No. 3BM is identified as a man (perhaps because of the short hair-cut), and its emotional state is identified as drunkenness;

(4) grief in Picture No. 3GF is not recognized as such, but is identified as illness of physical origin;

(5) he fails to identify the resistance of the male in Picture No. 4 to female advances;

(6) and the woman in Picture No. 5 is merely identified as such and her expectant attitude is not clearly specified.

In short, in language which continues to represent consistently Informant No. 2's special brand of Indian Spanish, this informant regularly misses standardized cues to Ladinos mores and morals.

Figure 6

PINOLA TZELTAL

\[ \begin{array}{llllllll}
  p & t & ĝ & ķ & i & u \\
  ĵ & ķ & ī & ĵ & ķ & e & o \\
 b & d & g & a \\
 s & ś & x \\
 m & n & a & ā \\
 w & y & CVx & CVČ \\
 l & CVp & CVpC \\
 r \\
\end{array} \]

The Pinola Tzeltal (Figure 6) of Informant No. 3 (Indian) contains a number of features which clearly mark this informant as fairly conservative:
(1) the use of nominal classifiers in place of (tulát 'sos cristianó') or prefixed to numerals (túlán 'una mujer', kóht ́fi 'un perro', péh té 'un palo', hű́is tón 'una piedra');

(2) the use of classifiers /h/- and /š/- (hkóht š-čúč 'una rana');

(3) clear glottal (/ʔb/ and glottalization (/b/)) in the reflexes of /b/ (hőb[ʔb] čáy 'cinco pescados', čěb mó 'dos pájaros', hkaʔbtik 'nuestras manos', hű́n ha ʔbí 'un año', čín [b]éh 'caminito', ya š-báh ta milél 'lo van a matar, htáʔ[b.ə] 'veinte');

(4) Tzeltal stress in Spanish loans (krisyanóh 'gente', mančanáh 'manzana', hkóht čiwoh 'un chivo', merkadóh 'mercado', pwersáh 'fuerza') (except for: ālagúnaéh 'la laguna', šléčě wáká 'la leche de la vaca', túnah ~ tunáh 'nopal', segúro, 'seguro').

There is, on the other hand, a small number of features which mark No. 3 as innovating or hispanicizing:

(1) occasional straight numerals (without the nominal classifier prefixes) in situations which normally require such classifiers (hoʔèb čáy 'cinco pescados', čěb mó 'dos pájaros', ošèb tomút 'tres huevos');

(2) homorganic nasal (in position where the local Tzeltal does not normally have it) (či[m]béh 'caminito').

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The Pinola Indian Spanish (Figure 7), of Informant No. 3, is, on the whole, even more conservative than that of Informant No. 2, and is characterized by the following special features:

**Phonetic Features:**

1. the voiceless stops are occasionally aspirated (*un* [k']aminito, *se* 'stá hinchando mi *[p']*iie, *el* *[p']*o*too del [k']amino);
2. use of [v] allophone of /b/ (*es* muy *[v]i*[v]*o*);
3. failure of /s/ to assimilate to voiced allophone before voiced consonant (*e*[s] *muy* *vivo*);
4. fricative /r/ (*una rana*);
5. the final vowels are occasionally followed by voiceless echo vowels (*la* sangre*[e]*, *el* hues*[o]*, *lo* 'ist*[e]* *la* ceniz *[a]*, *es* redond*[o]*);
6. very open allophone of final /e/ (*la* sangr *[e]*, *sus* pi*[e]*, *lo* 'ist*[e]*, 'stá cortando zacat*[e]*, *manta* dobl*[e]*, veint*[e]*, *caf*[e]*, *camot*[e]*) or /e/ elsewhere (*despu*[e]*s, *las* nub*[e]*s, *una* pi *[e]*gra, ar*[e]*na, *ci*[e]*go*);
7. final /a/ is occasionally centralized (*una* piegr*[a]*, *aren*[a]*, *la* ceniz*[a]*, *l*[a]* muchach*[a]*, *manzan*[a]* *pogrid*[a]*)
8. introduction of a voiceless vocoid initial in a Spanish word (*la* [i]iel).

**Morphophonemic Features:**

15. failure to assimilate a nasal to homorganic position (*so*[n] buenos);
16. misspeaking of a labial (*los* pájaros muelan instead of *vuelan*);
17. introduction of a nasal into a consonant cluster (*y* *un* ingl*[e]-sia *for* *y* una iglesia);
18. cluster /ky/ replacing /kr/ (*las* *[k]*aturas no andan instead of *las* criaturas . . .);
19. replacing of cluster /dr/ by /gr/ (*una* piegra, manzana pogrida, *un* cegro);
20. replacing of cluster /fu/ by /xu/ (*el* *[x]*uego);
21. *ia* or *io* vowel clusters replacing *illa* or *illo* (*la* semía, *la* rodía, *amarío*, *martío*), but: *se* 'stá riendo de mí instead of *se* está riendo de mí);
22. omission of preconsonant /s/ (*y* 'eamos comiendo);
(24) loss of [d] (y si 'spiértan temprano = y se despiétan temprano);
(26) loss of final vowel (carne de coch', por la noch').

Grammatical Features:

(35) plural endings are omitted (somos chico', cinco pescado', dos pájaro', nuestra' oreja', sus nariz', sus pie', sus cora-
zon') as are other final s'es (lejo[9']);
(36) plural number in possessive adjectives refers to plurality of
possessor rather than of thing possessed (sus nariz', sus 
pie', sus barriga', sus corazón', sus hígado');
(37) use of the article before the possessive adjective (está mala
la su mujer);
(39) feminine gender concordance fails (el orina, un inglesa, la 
ropa es blanco, 'stá muy frío la noche');
(40) special forms of the second person singular of verbs (vos 
sos ¿quién sos? ¿cómo te llamas?), although the Central
Mexican forms also occur (¿qué quieres? ¿porqué no vi-
es? no lo sabes);
(41) misplacing of grammatical agreement in imperatives (duér-
nate in place of Central Mexican duérmete or Chiapanec
dormite).

Lexical Features:

(60) special lexical items (una mata de palo, la cáscara del palo, 
carne de coch', una tijera, tsilicayote) (for árbol, corre-
za, puerco, tijeras, and chilacayote, respectively).

Accuracy of Translation:

(70) occasional poor translation equivalents (sbak te hshitik ≠ sus 
ojos de Ustedes, kakantik ≠ la rodilla, hshitik ≠ tu cara).

Nine items (5, 19, 20, 22, 23, 24, 26, 40, and 60) are character-
istic of local Indian Spanish, three items (18, 36, and 39) represent
special solutions to shift difficulties between the two languages, and
twelve items (1, 2, 4, 6, 7, 8, 9, 15, 16, 17, 35, and 37) are clearly
transfers of Indian linguistic habits into Spanish speech. Item 41
constitutes an unresolved problem (what to do wth a special im-
perative form which reflects a status difference?). Item 70 tells us
that Informant No. 3 occasionally strays from the translation stance
(the three Tzeltal examples all contain the first plural possessive
affixes, although none of the elicitation forms do).
Analysis of the content of Informant No. 3's Spanish-language responses to the T.A.T. pictures reveals clearly his own even greater remove from a Ladino frame-of-reference:

(1) he totally fails to see the violin, in Picture No. 1, does not note the half-closed eyes which indicate either sadness or weariness, and subsequently identifies the young male figure as happily relaxing, and proud of his cornfield; he suggests that the figure may be that of his compadre, disregarding age, situation, and objects in the picture, treating only those things the viewer has imaginatively attributed to the figure;

(2) his only suggested reason for hypothetical sadness of one of the two female figures, in Picture No. 2, is illness;

(3) the short-haired female figure in Picture No. 3BM is identified as male, and the attitude as due to a crippling infirmity;

(4) the female figure in Picture 3GS is said to be weeping from sadness at having lost some possession;

(5) he totally misses male resistance to female advances in Picture No. 4 and characterizes them both as happy;

(6) reaction to the female figure in Picture No. 5 is solely to her physical attitude—her environment, characteristically Ladino, is totally ignored;

(7) the female figure in Picture 6BM is identified as male, the house interior is identified as a park, and the thoughtful attitude of the young man is characterized as conceit;

(8) in Picture 6GF, the short-haired lip-sticked female figure is identified as a boy, and the pipe-smoking middle-aged figure behind her is described as a little old man smoking a cigar, and the living-room in which they are talking is said to be a summer-house in a garden;

(9) the doll in the hands of the pre-adolescent girl in Picture No. 7GF is said to be a baby, and the girl's mother (the baby's grandmother) is advising her to feed it, since it is crying;

(10) the scene with the men (one of whom is holding a knife) hovering over a prone figure, in Picture No. 8BM, with a thoughtful young man in the foreground, is interpreted as a post-violence scene in which the prone figure is the body of a man who has been murdered, and the formal clothes of the young man are said to be mourning-dress.

All of these reactions are typically non-Ladino, and the Ladino attributes of attitudes and environments are nowhere perceived.
The Pinola Tzeltal (Figure 8) of Informant No. 4 (Indian) contains certain features which mark this informant as extremely conservative:

(1) the very frequent use of nominal classifiers prefixed to numerals (h-htól 'un árbol', hōp'kōht cay 'cinco-pescados', ḥpēh tép 'un árbol', ṭospis tomút 'tres huevos', ḥpīh tón 'una piedra', ḥčīś ṭasarón 'un azadón');

(2) clear glottal /ʔb/ and glottalization /b/ in the reflexes of /b/ (kešam ka'bal 'mano izquierda', yak ta s-bohel 'ak 'está cortando zacate');

(3) the Spanish loans which appear are almost all completely assimilated (mantāh 'tela', mančanáh 'manzana', ṭasarón 'azadón', tešelešetik 'tijeras', kapēh 'cafe', tunāh 'nopal', nalašetik 'naranjas', tulesnāh 'durazno').

On the other hand, there are also several features of his Tzeltal which mark him as innovating (under Spanish influence):

(1) use of some unassimilated loans (kristyánōh 'gente', spulmonik 'sus pulmones');

(2) use of plurals of items in circumstances in which plurals would not, in Tzeltal, normally be used (ya s-tišawan
The Spanish (Figure 9) of Informant No. 4, however, shows hardly any influence whatsoever, either of Pinola Tzeltal or Pinola Indian Spanish:

(4) failure to assimilate /s/ to a following voiced consonant (tre[s] huevos);
(19) replacement of /dr/ by /gr/ (una piegra);
(28) insertion of /y/ in intervocalic hiatus (se está riendo de mí: normal riendo);
(5) omission of final /s/ (cuatrociénto').

On the contrary, many of the features characteristic of local (Indian) Spanish are not found in this informant's speech:

(5) his /r/ is a clear trill;
(20) his fuego is pronounced with an /f/;
(22) his illa and illo pronounce their /ll/;
(24) his [d] is not lost.

This informant presents very precise translation equivalents in what is otherwise very good Central Mexican Spanish. (kehtikik 'nuestros dientes', tukel layalbon 'él me dijo', há? ?ačišeh 'la muchacha', yakon ta ?ačel ta hkal 'estoy trabajando en mi milpa'.

Informant No. 4 contrasts with Informants No. 2 and No. 3 not only...
negatively, in the precision of his translation, where they are imprecise, but also positively in having as prevalent allophones of /w/[yu] and [gu], characteristic of neither of his two languages alone, but found in both.

Analysis of the content of Informant No. 4's responses to the T.A.T. pictures shows a considerable sophistication in things Ladino and an accentuation of Ladino ideals of conduct:

(1) the thoughtful boy, in Picture No. 1, is described as an orphan, having his music, as studious and hard-working, in preparation for becoming a great man;

(2) the man with a horse, in Picture No. 2, is said to be very industrious; the young female figure is described as being a teacher with a book;

(3) the short-haired female figure, in Picture No. 3BM, is described as a sad young man (or a drunken young man), who is an orphan;

(4) the woman covering her face in picture No. 3GF is said to be weeping because she cannot earn enough money to make a living;

(5) the scene of the reluctant male, in Picture No. 4, is described as a scene of marital bliss in which a loving husband and wife, faithful to each other, are in constant fear, lest one or the other die;

(6) in Picture No. 5, all the emphasis is on the material possessions in the house of the woman of affluence, who is good, and intelligent, too;

(7) in Picture No. 6BM, both female and male figures are correctly identified as to sex; the former is given the role of mother, the latter that of a studious, intelligent son who cares for her needs;

(8) the female figure in Picture No. 6GF is identified as male, and in him our informant sees a studious son and a loving father, and expatiates on the advantages of having a father to care for one;

(9) in Picture No. 7GF, the mother and daughter are identified as such, but the doll is described as the daughter's baby, and much is made of the possibility that the baby might die;

(10) the knife scene, in Picture No. 8BM, is described as involving doctor hired by a dutiful and loving son to operate on a loving father suffering from a serious illness;

(11) the smiling young lady of Picture No. 8GF is said to be sad because she is an orphan;

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(12) the resting workers, in Picture 9BM, are made the motive of a discourse on the salutary effects of rest for the tired body;

(13) the two women of Picture No. 9GS are said to love each other like sisters;

(14) in Picture No. 10, marital bliss is again extolled;

(15) in Picture No. 11, the informant sees an elephant, of whom people are wrongly afraid, and some vultures which are held in check by a man;

(16) in Picture No. 12M, God has sent a priest to bless a dying person, whom He will take, if that is His will;

(17) in Picture No. 12F, an old woman is seen side by side with her son (here a female figure is misidentified as a male), and the virtues of filial piety are extolled;

(18) in Picture No. 12BG, a pool for bathing in a summer scene is identified;

(19) in Picture No. 13MF, a man is said to be mourning his deceased wife;

(20) in Picture No. 13B, the child is said to be eating fruit without a care in the world, and the advantages of having parents who care for one is indicated;

(21) in Picture No. 13G, a child is climbing stairs;

(22) in Picture No. 14, a carpenter is working in darkness;

(23) in Picture No. 15, a man is described as standing among crosses and squares;

(24) the blank Picture No. 16 motivates a long discourse on the baptism of a child;

(25) the rope-climbing naked figure, in Picture No. 17BF, is motive for a discourse on the use of ropes to climb trees with low branches;

(26) the Picture No. 17GF (of the sun shining on the bridge and the boat-docks) gives rise to a discussion of the shape of the world and of night and day;

(27) the man held by hands, in Picture No. 18BM, is said to represent an ill person supported by friends;

(29) the cabin-in-the-snow scene, in Picture No. 19, is said to represent a tractor;

(30) the hatted figure beneath a street-lamp, in Picture No. 20, is said to represent a man who went out to buy medicine at a late hour.

This informant sees Ladino goals in almost all subjects. His preoccupation with orphanhood, and with death through illness, how-
ever, as well as his failure to recognize dolls and short-haired females mark him quite clearly as an Indian in his perception of the Ladino world.

**Figure 10**

SAN BARTOLO TZOTZIL

```
pt'ććk?
iu
t'ććk
 eo
dg
sx
mn
aá
wy
CV CVp
l
CVx CVČ
r
```

The Tzotzil (Figure 10) of Informant No. 5 likewise presents features which mark him as carefully conservative:

1. use of nominal classifiers prefixed to numerals (*hwo ?ánč 'una mujer', *hkot ūp 'un perro');
2. use of *kul 'holy, sacred' with the names of numerous objects belonging to the category of revered natural phenomena (trees, seed, sun, moon, star, rain, stone, clouds, fire, night, sky, day, fog, year, cornfield, drum, God);
3. use of *cin 'small' as a similarly reverential diminutive for other objects (feather, bird, frog, butterfly).

His translation equivalents for the Spanish elicitation formulae are exact (*asátik ho'osúk 'sus ojos de Ustedes', *šákel ketík 'nuestros dientes', *hkótnikál ūp 'un perro negro', *kul čáwokíl tók 'la neblina (santa nube del rayo)').
The Spanish of Informant No. 5 is, in general, near the Central Mexican norm, but departs from it in some respects:

(5) his /i/ is fricative (reboso, corral);
(19) replacement of cluster /dr/ by /gr/ (en una piegra);
(42) omission of a in the expression ir a (hacer algo) (lo va calentar);
(43) use of general object form lo not in agreement as to number or gender with those of the objects to which it refers (él lo está cuidando las cositas que tiene, lo va calentar una taza de agua);
(60) lexical peculiarities (está poco triste, pos, ansí, apuntando en el dedo, saber de dónde for algo, pues, así, con, quién sabe);
(61) gender shift (el costumbre, but also la costumbre).

Noteworthy is the careful use of tampoco which not even all native speakers of Spanish use in its required context.

Indian Informant No. 5's perceptions of the Indian world, found in the photographs of the P.C.P., differ considerably from those of Ladino Informant No. 1:

(1) in P.C.P. No. 1, Informant No. 5 sees the girl as being from cold country (by contrast with San Bartolo);
(2) in P.C.P. No. 2, he sees the couple as Chamulas from Amatenango (which is Tzeltal, not Tzotzil);
(3) in P.C.P. No. 3, he sees the old man as sad and everyone as solemn as if in church (it is a wedding);

(4) in P.C.P. No. 4, he sees the old man as from Aguacatenango (he is in fact from Pinola), and as guarding his possessions;

(5) in P.C.P. No. 5, he sees the ladies as supping, as in a house, as Ladinos from Pinola (they are Pinola Indians in a grave-yard on All Souls’ Day);

(6) in P.C.P. No. 6, he sees a Ladino lady as about to heat a cup of water (she is an Indian from Pinola);

(7) in P.C.P. No. 7, he sees an Indian boy with a sling-shot as Ladino (because he has good trousers);

(8) in P.C.P. No. 8, he sees a Ladino house in San Cristóbal (it is in fact to be found in Pinola);

(9) in P.C.P. No. 9, he sees a man blowing the fire with his head tied up like a chef’s (it is, in fact, a curing ceremony);

(10) in P.C.P. No. 10, he sees old men (Chamulitas) playing the fife and drum (they are in fact Pinola Tzeltal, not Chamula which is Tzotzil);

(11) in P.C.P. No. 11, he sees a lady from Ladino Soyatitán as a Chamula redressed as a Ladino.

Our San Bartolo Informant No. 5 sees a clear line between San Bartolefios and others, some from cold country. The line between redressed Indians and Ladinos, however, is not so clear to him, nor is the line between one cold-country Indian and another. For the rest, his description of the content of the P.O.P. photographs is relatively colorless. He, like the Ladino from Ocosingo, fails to identify curing ceremonies as such. He himself, as revealed by his reactions to the P.C.P., is a relatively dispassionate person, who reacts without great involvement to external stimuli.

In the Spanish, as we have seen in these test responses, of Informants 2, 3, 4, and 5, all Indians, only one feature (19) is shared by all four, and only two features (5 and 35) are shared by three (by 2, 3, and 5, and by 2, 3, and 4, respectively), but eleven are shared by Informants 2 and 3, and two by Informants 3 and 4 (1, 6, 8, 15, 20, 22, 24, 26, 36, 39, 40, and 4 and 28, respectively). Of the 11 shared by Informants 2 and 3, features 1, 6, 8, 15, 22, 26, 36, 39, 40, and 4 and 28, respectively. Of the 11 shared by Informants 2 and 3, features 1, 6, 8, 15, 22, 26, 36, 39, 40, and 4 and 28, respectively. Of the 11 shared by Informants 2 and 3, features 1, 6, 8, 15, 22, 26, 36, 39, 40, and 4 and 28, respectively. Of the 11 shared by Informants 2 and 3, features 1, 6, 8, 15, 22, 26, 36, 39, 40, and 4 and 28, respectively. Of the 11 shared by Informants 2 and 3, features 1, 6, 8, 15, 22, 26, 36, 39, 40, and 4 and 28, respectively. Of the 11 shared by Informants 2 and 3, features 1, 6, 8, 15, 22, 26, 36, 39, 40, and 4 and 28, respectively. Of the 11 shared by Informants 2 and 3, features 1, 6, 8, 15, 22, 26, 36, 39, 40, and 4 and 28, respectively. Of the 11 shared by Informants 2 and 3, features 1, 6, 8, 15, 22, 26, 36, 39, 40, and 4 and 28, respectively. Of the 11 shared by Informants 2 and 3, features 1, 6, 8, 15, 22, 26, 36, 39, 40, and 4 and 28, respectively. Of the 11 shared by Informants 2 and 3, features 1, 6, 8, 15, 22, 26, 36, 39, 40, and 4 and 28, respectively. Of the 11 shared by Informants 2 and 3, features 1, 6, 8, 15, 22, 26, 36, 39, 40, and 4 and 28, respectively. Of the 11 shared by Informants 2 and 3, features 1, 6, 8, 15, 22, 26, 36, 39, 40, and 4 and 28, respectively. Of the 11 shared by Informants 2 and 3, features 1, 6, 8, 15, 22, 26, 36, 39, 40, and 4 and 28, respectively. Of the 11 shared by Informants 2 and 3, features 1, 6, 8, 15, 22, 26, 36, 39, 40, and 4 and 28, respectively. Of the 11 shared by Informants 2 and 3, features 1, 6, 8, 15, 22, 26, 36, 39, 40, and 4 and 28, respectively. Of the 11 shared by Informants 2 and 3, features 1, 6, 8, 15, 22, 26, 36, 39, 40, and 4 and 28, respectively. Of the 11 shared by Informants 2 and 3, features 1, 6, 8, 15, 22, 26, 36, 39, 40, and 4 and 28, respectively. Of the 11 shared by Informants 2 and 3, features 1, 6, 8, 15, 22, 26, 36, 39, 40, and 4 and 28, respectively. Of the 11 shared by Informants 2 and 3, features 1, 6, 8, 15, 22, 26, 36, 39, 40, and 4 and 28, respectively. Of the 11 shared by Informants 2 and 3, features 1, 6, 8, 15, 22, 26, 36, 39, 40, and 4 and 28, respectively. Of the 11 shared by Informants 2 and 3, features 1, 6, 8, 15, 22, 26, 36, 39, 40, and 4 and 28, respectively. Of the 11 shared by Informants 2 and 3, features 1, 6, 8, 15, 22, 26, 36, 39, 40, and 4 and 28, respectively.
those speaking what is essentially the same dialect, make exactly the same selection of Indianizing (or, for that matter, Hispanicizing) features.

Informant No. 1, who speaks good Central Mexican Spanish with only a slight local rural tinge, speaks fluent but inaccurate local Tzeltal, and identifies himself, by his reactions to the P.C.P., as a benevolent but undoubted Ladino. His job is that of local Commissioner of Indian Affairs.

Informant No. 2, who speaks Pinola Indian Spanish with wide departures from the Central Mexican norm, nonetheless speaks a Pinola Tzeltal which is innovating and, in some respects, Hispanicizing. His transfer from one to the other is, however, difficult, and his translations lack precision. His reactions to the T.A.T. show clear evidence of his considerable distance from the Ladino world, and his attempts at fluent Spanish never progress beyond the Indian Spanish level. He is caught between his desire for Ladinization and his inability to attain it. He is a local curer.

Informant No. 3, who speaks a conservative Pinola Indian Tzeltal, on rare occasions innovates or Hispanicizes. He speaks an Indian Spanish which departs even farther from the Central Mexican norm than that of Informant No. 2, and contains further special features not found in the speech of No. 2. His reactions to the T.A.T. are far removed from the Ladino frame-of-reference, and he repeatedly misses in the pictures Ladino attitudes and environments. He is a local farmer.

Informant No. 4, who speaks an extremely conservative Pinola Indian Tzeltal, has, nonetheless, certain minor features which mark him as innovating (under Spanish influence). His Spanish, on the other hand, is closer to the Central Mexican norm than that of either of his two fellow Pinoltecos. Indeed, it betrays only very occasional traces of departures from this norm, not even in the direction of local Ladino Spanish. He transfers quickly from one language to the other, and his translation equivalents are extremely precise. He has, both in his Tzeltal and in his Spanish speech, a single feature, the fricative /w/, which sets him apart from his fellows. His responses to the T.A.T. show great sophistication in things Ladino and in Ladino ideals of conduct, although his private themes of orphanhood and the fear of death through illness frequently recur, and his failure to recognize items such as dolls and short-haired women betray his Indian origin. He is an orphaned tubercular who is employed as
a town-clerk and interpreter and functions as a kind of communications officer between Indians and Ladinos.

Informant No. 5, finally, is in his Tzotzil speech conservative, in his local San Bartolo Spanish fluent, and in his translation equivalents exact. His perceptions of his fellow Indians in the P.C.P. differ considerably, as one might expect, from those of Ladino Informant No. 1. He is, nonetheless, imprecise in his identification of Indians elsewhere, and, in some cases confuses them, since dress seems to be his primary index, with Ladinos. His responses to the P.C.P. do not strongly identify him with the Indians from elsewhere when he sees them, and he reacts somewhat dispassionately to other stimuli in their environment. He is employed as informant to the anthropologist who is working in San Bartolo.

Informant No. 1 is the only Ladino among our bilinguals and plays his role well.

Informants No. 2 and No. 3 both innovate and hispanicize, the former frequently, the latter rarely, in their Tzeltal. Both speak fluent Indian Spanish, but the speech of both of them departs widely from the Central Mexican norms. Although No. 2 still strives to approach that norm, No. 3 is blissfully unaware of its existence. Bilingualism with No. 2 is a means for change, but he finds himself completely at home in neither sociocultural community. For No. 3, bilingualism seems to have no important function at all—it would be hard to be more monocultural and yet bilingual.

Informants No. 4 and 5, on the other hand, both speak an Indian language which is even more conservative than that of Informant No. 3. At the same time, their Spanish departs only slightly from the Central Mexican norm. No. 4, in his reactions to the T.A.T., shows himself to be a good interpreter of Ladino culture, although not fully identified with it. No. 5, however, although like No. 4 in most other respects, is dispassionate toward it, and is quite happy to keep his distance.

Only Informants 1 and 4, therefore, succeed as mediators between the two cultures, the first by virtue of his attitude more than through his performance, the latter by skillful manipulation of the communication media of two worlds.

Editor's Note: There was no discussion after this paper due to the lateness of the hour.
PANEL III

NATIONAL LANGUAGES
AND DIGLOSSIA
GREEK DIGLOSSIA

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A. The purpose of the present study is not to recount the history of the "language question" in Greece, a fascinating story which could not be adequately told in any reasonable compass, full of the most remarkable human passions and incredibly illogical behavior; it is to specify as accurately as possible the linguistic marks of katharevousa and dhimotiki, the various degrees and shades of each and the typical situations in which each is used.

B. In this section we shall present the main distinguishing features of the two written standards, classified in two ways: (a) as spelling, inflection, vocabulary (subdivided into derivation and stems), or syntax; and (b) as obligatory, criterial, regular, occasional, and forbidden. An indication 1 (K1, D1) will indicate that the form in question is regarded as extreme, i.e., archaic in the case of K, dialect (O.K. for poetry and dialog) or conversational in the case of D; 2 (K2, D2) indicates a criterial form—i.e., any text with a K2 form in it is surely K, with a D2 form D; 3 (K3, D3) marks forms which are normally K or D but sometimes occur in the other language (D or K); 4 forms belong freely to both languages.

1. Spelling

a) The two most striking and frequent elements distinguishing D and K are points which involve both spelling and inflection. The first is final -n. Several forms which in K (and Ancient Greek) obligatorily end in -n, plus one or two which optionally do so with high frequency, never do so in D. The chief forms are: A. sing. in -on, -an, -in. N.-A. sing. neuter in -ion, -on, first person plural active in -men, third person singular past in -en (or -e). An average page of 300 words or so of Greek will have 50 or 60 examples of this presence or absence of final -n, or about two in every line.

The other, slightly less pervasive difference, lies in the preposition meaning "to, at, in," which in ordinary katharevousa is obligatorily a separate word is but in dhimotiki appears in three forms: se before consonants in general, s' before vowels, and combined with
the definite article as *sto, sto*(n), *sti(n), stou, stis, stous*, and *sta*. This occurs about 10 or 15 times on an average 300-word page of text. In recent years this has begun to turn up occasionally in the mildest form of K, but otherwise is a mark of dhimotiki.

b) Of all the other orthographic cues, certain differences in accentuation are probably the most pervasive, occurring (on the average) about seven times on a page. These fall into several classes. (1) There are occasional instances where K regularly uses a circumflex accent, whereas official D (as laid down in Triandaphyllichis' grammar) uses an acute. (2) The D accent is on a different syllable from the K accent: (a) because the D accent persists through all or most of a paradigm, as in the feminine forms (and genitives, and masculine accusative plurals) of most adjectives including comparative degree forms and in many nouns; (b) because the K accent is on an iota which becomes non-syllabic in D, inducing a shift to the following vowel (D pedhyá, etc., K pedhía, etc., D kardhyá and many other feminines in-ýá —K kardhía, etc.); (c) because of differences like D amerikaníkos, K (and D) amerikanikós where D has developed a suffix which differs only in accent from another shared by K and D. But in addition to actual minimal pairs, there are many more cases per page where D has a purely D word whose accentuation is contrary to K rules (e.g. andilalus, gháydharios, etc.) but which would be replaced in K by a different word, or conversely cases in K of accentuation contrary to D rules where D would use a different word or form.

c) About five or six times per page there will occur a consonant cluster which will have one form in D and a different one in K. There are two main classes: the cluster of two voiceless obstruents (stops or fricatives) and the cluster of nasal plus obstruent. In the first case (two obstruents) K allows three types: stop plus stop or s (e.g., pt, ps), fricative plus fricative (e.g., fth) and fricative plus stop or s (e.g., ft, fs). The ideal "pure" spoken language would allow only two, fricative plus stop (e.g. ft) and stop plus s (e.g. ps); D has, in fact, borrowed numerous words from K and from foreign languages which have reintroduced the other types, but there still remain many words in which K has (e.g.) pt or fth, where D has ft, kt or xth, where D has xt, fx where D has fk, fs, where D has ps, etc.

In the second case (nasal plus obstruent), K allows the obstruent to be a voiced stop, or a voiced or voiceless fricative, whereas "pure" vernacular Greek allowed only the voiced stop (for both stop
and fricative of K), and dropped the nasal before a voiceless fricative. When nasal plus voiced fricative was reintroduced at a later date, spoken Greek dropped the nasal here too. D has reintroduced the K clusters in some words, and both K and D also have occasional sequences of nasal plus voiceless stop in loans from other European languages. However, many contrasting pairs remain.

d) Many items in D (and even more in S) differ from their K equivalents by the absence of an initial unaccented vowel or, in other cases, its replacement by another. This can occasionally cause trouble in looking up words in a dictionary, because not all the shortenings are recognized in every dictionary—though the most common and important ones are, of course. The unstressed syllabic augment, e-, is seldom used in D but regularly in K.

e) The preposition ya and the forms yatı “why,” yatı “because,” and yana “in order to” occur at least three times per page of D. The K equivalents for ya are varied: sometimes dhiá with acc.; sometimes a dative, sometimes metá with gen., sometimes pros with acc.; and there are others. Yatı “why” is rendered as dhia tí, but yatı “because” is dhió óti and yana is simply ína most of the time, though dhiá na occurs in “simple” K.

Virtually all the spelling differences (basically phonological differences in most cases) listed above belong to categories D1 and D2, i.e., they are used only in D, so their presence proves D while their absence does not prove K. A few of them, however, (e.g., the accent of comparatives) are almost criterial in both directions, like final n and yá.

Inflection

A. If we allow (for the moment) identity of forms with and without final -n, then a large proportion of noun-adjective endings and a fair number of verb forms are identical in the two dialects. (1) In the old first declension (stems in -a and -i) feminines are mostly the same in the singular and G. pl., but differ in the plural N.-A. (K N. -e, A. -as, D N.-A. -es). Feminines which are third declension (consonant stems) in K will have the same forms in D in the A. Sing., and N. G. Pl.: A. Sing. -a, N. Pl. -es, G. Pl. -on, while the other forms differ (K N. S. -s, G. -os, A. Pl. -as, D N. S. -a, G. -as, A. Pl. -es). The old i-stems present a different picture; here the whole plural is the same (N.-A. -is, G. -eon) and the acc. sing., as usual, differs only by an -n, while the genitive singular is commonly the same, espe-
cially in longer and more elegant words (-eos); but otherwise the
singular is different: in D N.-A. -i, G. -is, in K N. -is, G. -eos, A. -in.
(2) Masculines of this declension are like feminines, except that D
and K have now the same nominative (in -is or -as), but different
genitives (D -i, -a, K -u), although many proper names in K use the
D genitive. Two variations occur in D: some agent nouns in -is or
is have added-syllable plurals in -idhes, -idhes, or ádhes, and some in
-tís, recently borrowed from K, use the whole K plural. Those mas-
culines which are third declension in K are declined like the K fem-
inines of that type, with the same three cases like D, while the D N.
Sing. is in -as, G. Sing. in -a. There is an important type of agent-
noun which in K runs S -efs, -éos, -éa Pl. -ís, éon, -ís. Normally D
here shares the A. sing. and the whole plural, reshaping the N. Sing.
to -éas and the G. to -éa, but a few of these words appear also in a
fully demoticized form -yás (etc.) with the extra-syllable plural in
-yádhes, etc. (like the feminines). (3) Old second-declension mas-
culines are nearly identical in K and D (allowing for the final -n of
the A. Sing.). A few words (chiefly given names) take the (D)
vocative singular in -o instead of -e, but most of these names have a
different shape in K. Feminines of this declension are exactly like
masculines in K, and this declension may be preserved also in D.
But some old S. words (chiefly names with no plural) shifted to the
regular type in N.-A. -o, G. -os. Neuters, too, are generally the same
in D as in K except for the -n. But there is a D type in Sing. -í, yú,
Pl. -yá, -yón which corresponds to K S. -ión, -iu, Pl. -ía, -ión. How-
ever, K frequently avoids these nouns (even when they are good clas-
sical Greek), because they are so common in D and spoken Greek.
But moderate or "simple" K is sometimes willing to use them (even
with the D declension occasionally). (4) The old neuter s-stems are
also the same in D as in K: Sing. -os, -us, Pl. -i, -ón, except that K
uses an optional G plural in -éon for a few of these nouns. A large
group of neuter t-stems is identical in K and D, those which end in
Sing. -ma, -matos, P. -mata, -máton. D also has a productive de-
verbal noun suffix -simo which is declined in the same way, Sing.
-simo, -símatos, Pl. -símata, -símáton. This formation is not admitted
in K where it is replaced by -sis, -ma, -mós or the ancient infinitive
_used as a noun. (5) Among adjectives, D has one new masculine
type (S. -ís, -yú, Pl. yl. -yón, -yús; where the irregularity lies in the
nominative singular (a noun would have -yós); the feminine is like
any noun in -yá, the neuter like neuters in -í. The corresponding K
type runs M. Sing. -ís, -éos, -in, Pl. -ís, -éon, -ís like the common
feminine noun type except for the accent and the spelling; D has a

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type with ordinary o-stem masculine and neuter and normal feminine plural, but with a feminine singular in -yá, -yás (instead of -i, -ís). Many of these can also be declined either like ordinary adjectives in the feminine with -i, -ís or like the -ís, -yú adjectives in masculine and neuter. In K they are all regular of one or the other type. Finally, D has a type with M. Sing. -is, -i, Pl. -idhes, -idhon, F. Sing. -a, -as, Pl. -es, -ikon, N. Sing. -iko, -iku, Pl. -ika, -ikon, with a few possible variants in F. and N. These words are always replaced by synonyms in K (and many of them are classed as D1, extreme Dhimotiki).

B. The inflection of verbs is a different matter; here a large proportion of the endings used in K and D are different and criterial. (1) In the active primary endings (except for certain “contracted” present types) everything is the same except for the final -n and sometimes the thematic vowel of the first plural. But in the third plural, D has a longer variant -une which is quite common in conversation, and K has also an extreme type in -usi or -usin, (-osi/n for subjunctives). (2) In the middle primary endings of the same class, four endings agree: -o-me, -e-se, -e-te and 3 Pl. -o-nde; D has 1Pl. o-maste, 2Pl. -e-sthe and K 1Pl.-o-metha, -e-sthe. The difference in the second plural is one of the systematic phonological differences (reducing fricative plus fricative to fricative plus stop in D) between K and D. (3) In the past tense D has only one active set of endings (-a, -e-s, -e, -a-me, -a-te, -a-n or a-ne) which differ from the primary ones only in the quality of the (first) vowel (a instead of e, o, u; e for i), and they occur in the imperfective past active and the perfective past (aorist) active and passive. In K these three tenses differ, so that only 2 Sing. and 3 Sing. come out identical in the imperfect (-e-s, -e), while in the aorist everything except the 2 Sing. is the same (D -e-s, K -a-s), with the usual allowances for final -n (K) in 1Pl. and the optional final -e in 3Pl. In the aorist passive D has a characteristic -k- between these endings and the -i-, -ti- or -thi- which marks the paradigm, while K drops the first vowel (a or e) of the endings and adds the remainder directly after -(th)i-, introducing new suffixes -n for 1 Sing. and -san for 3 Pl. So of 18 forms, 11 are different. (4) The secondary middle set of endings (used only for the imperfective past) differ absolutely: K -ó-min, -e-so, -e-to, -ó-metha, -e-sthe, -ó-ndo, D -ó-mun, -ó-sun, -ó-tan, -ó-maste, -ó-saste, -o-ndan. There are several spoken variants in the D set, but all are even less like K than these. (5) In the imperative the agreement is nearly perfect, K having adopted three (out of 8) forms from D in place of ancient types. The only remaining difference
(aside from -sth- vs. -st-) is in the plural aorist active, where D adds -te directly to the final consonant of the stem (usually s), or (in more elegant forms) uses -e-te, while K has -a-te. (6) While K has a complete set of two active and three passive participles (extreme archaistic Kl may have even more) all fully declined, D matches the K imperfective active participle with an indeclinable gerund or conjugate in -ondas (which coincides in form with the masculine accusative plural of the K participle) and otherwise has only the equivalent of the K perfect passive participle in -ménos. Here there is variation in K: many of these p.p.p's are identical with D, but others are reduplicated in K (e.g. dhedheménos) though never in D (dheménos) except for sporadic borrowing of common expressions from K. Vamboulis and Zoukis give lists of participles which must be, may be, and cannot be reduplicated (Pars. 275-280), but the general impression is that the lines are shifting so that more and more unreduplicated participles are coming into K. (7) In the so-called contract conjunctions, of which D essentially has only one (and part of a second), where K2 has three (though the mildest sort of K omits one), there are various other differences. The normal present active agrees with the alpha-contract verbs of K throughout, except for first-person plural, D -úme or sometimes -áme, K-ómen. But D has also an alternative 3 Sing. -áy (K and D -á), and a 3 Pl. -án, -áne (K and D -ún, D-úne), while Kl has an alternative 3 Pl. -ósí(n). And many verbs conjugated in this way in D are conjugated as epsilon-contract verbs in K.

This latter pattern also exists in D, and (in the present active) agrees completely with the K pattern (barring the final -n of 1Pl.). Many verbs in D and S fluctuate between the two patterns, some speakers and writers preferring (e.g. patí and others patáy). (8) The present middle or passive forms of D and K can easily be separated into a thematic affix followed by personal endings. The personal endings of contract verbs are the same as those discussed above in (2); the thematic affixes, however, are different. Nearly all D verbs use -ye- before singular endings and 2Pl. -ste, -yú- before 1PL and 3 Pl. (optionally and in some dialects also before 1 Sing.); K alpha-contract verbs have -ó- before 1Sing., 1 Pl., and 3Pl., but -á- before the rest, while epsilon-contract verbs have -ú- in 1Sing., 1Pl., 3Pl., and -i- for the rest. A small class of verbs recently borrowed by D from K agrees with K in this (e.g. mimúme "I imitate"); an even smaller class (e.g. thímúme "I remember") combines the two K types, having -ú- for 1Sing., 1Pl., 3Pl. but -á- for the rest (also, optionally, -á- for the 1S). (9) The formation of the active past (imperfect) of
contract verbs differs widely in the two languages. In K alpha-contract verbs in the active have in the singular '-on, '-as, '-a and in the plural -ó-men, -á-te, '-on, while epsilon verbs go '-un, '-is, '-i, -ú-men, -i-te, '-un. In D all these verbs, regardless of the present tense formation, form their imperfect with a stem-affix -ú- or, in some dialects and some literary styles -agh- (with the accent always on the antepenult), followed by the regular endings mentioned under (3) above. The formation with -ú- appears occasionally in simple K now. (10) In the imperfective past middle (or passive) of such verbs, K and D differ both in thematic affix and in the endings (which are as in (4) above). For alpha verbs K uses the affix -ó- in 1Sing., 1Pl. and 3Pl., -á- elsewhere (just as in the present); for epsilon verbs, again as in the present, -ú- in 1Sing., 1Pl., 3 Pl., -í- elsewhere. In D, verbs which form their present with -ye- / -yu- add -yo- throughout for the past; those whose presents have -ú- and -i- use -ú- throughout the past, while those few with -ú- / -á- in the present have -ó- throughout the past.

In general we may say that inflection provides as strong criterial evidence as final -n; average pages show between 30 and 45 items per page (whether in K or D) which guarantee the dialect, and the amount of optional use of D forms in K, or K forms in D is usually slight, even in “simple” K or “journalistic” D.

Vocabulary

This is the realm where “simple” K and “moderate” D may, on occasion, approach each other very closely but, on the other hand, extreme forms of the two languages may differ almost one-hundred percent. In general, as we have already remarked, D feels free to borrow any K item that it needs, while K is self-conscious about borrowing a D word without first somehow cleaning it up a bit.

The most striking vocabulary differences between K and D are in the realm of basic or every-day vocabulary: prepositions, frequent verbs, body parts, kinship terms, common animals, etc. Here K adopts the D word only very rarely, when (a) the AG word would now be a monosyllable beginning with a vowel or (b) it is homonymous with some other word which might often occur in the same context, or (c) the D word did exist in AG where it had a slightly narrower or broader sense, or (d) the D word is of such very high frequency that it would be altering the essence of the language to change it (e.g., dhén “not,” nà “that, to,” tòn “him,” etc.) not that earlier K writers didn't try. Out of over 200 basic vocabulary items, only some half-
a-dozen or so have been taken from D or S by K, whereas about 25
have come into D from K.

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Because of the possibility of multiple choice in many cases, it is perhaps wiser to state a range rather than a single figure. For the 100 words above there is between 63 and 77 percent agreement. If these were two related languages which had been isolated from each other, the lexicostatistician would say that they had diverged somewhere between 430 and 1090 A.D. Their non-isolation would lead him to push the date even further back, say 100 B.C. to 650 A.D. It is, of course, the fact that K is not a living language which is responsible for this peculiar result; a lexicostatistical comparison of K with Ancient Greek would lead to a divergence date around 1850 (A.D.), or (at the earliest) 1700.

Substantial differences also exist in many other more specific areas of every-day vocabulary: of food, meals, cooking (here K often simply gives up and avoids the subject); (2) every-day crafts and trades (sailing, building, smith-work, garage-work, etc.); (3) names of ordinary plants, animals, birds, fish, etc.; (4) words for common aspects of human emotion and behavior; (5) the home and every-
thing about it; (6) Christian names and nicknames; (7) common adverbs, prepositions, and similar grammatical words (several are on the list, but there are many more); (8) words having to do with time of day, seasons, weather, etc.; (9) most recent foreign loan-words in any field. On the other hand, realms in which there is very little difference between the two are very much the same as those where French resembles English; (1) terms belonging to a science or learned profession (including religion, to a large extent); (2) words of government or politics; (3) words in journalistic writing on general subjects; (4) most geographical names; (5) certain rather vague but dignified words of common occurrence in journalese. Unlike the French-English parallel, however, are the major classes of words not originally borrowed by D from K: (1) the essential grammatical words (many, but not all of them), like δὲν “not,” ὅχι “no, not,” ἂν “will” εἰμαι “me,” many prepositions, some pronouns, some common adverbs, etc.; (2) certain items which have remained sufficiently unchanged in the spoken language tradition so that they can still be spelled exactly as in Ancient Greek; this includes some items which also fall in the first category, such as many forms of the article, some conjunctions, pronouns, etc., but also a number of verbs, nouns, and adjectives.

Syntax

In large measure the syntax of the two languages is identical so that (especially in simple journalistic material) a text in K and the corresponding text in D might differ only in phonology, spelling, and morphology. But there are a few striking differences in most styles which are confined almost exclusively to one language or the other.

K tends to use longer, more complex sentences, and D to use more parataxis. On an average page of K, the conjunctive particle κατὰ (κάτω) occurs about 7 times; on a page of D, about 16 times, or more than twice as often. This is in part because it has in D a number of special functions (sometimes subordinating) not found in K.

A second frequent difference noticeable in K is the free use of participles in virtually every way in which they were employed in AG, including even the genitive absolute and the future participle of purpose. A count of several pages of K text selected at random shows an average of eight participles to the page, used in many different ways, of which four will be present participles, two aorist, and
two perfect (including in all cases both active and passive forms). Less than three of the eight will be used as adjectives or nouns. In a page of D, on the average, there will be four participles, all perfect passive, and nearly all used as adjectives; gerunds in -ondas are less than one per page.

A third rather striking syntactic feature of K is the frequency of nominal genitives in many functions not possible in D. A sample count yields the following figures: K has an average of about 15 genitive nouns per page, D about 6. Several factors are involved here. D has, in general, long avoided the use of plural genitives and neuter genitives as much as possible. K, on the other hand, has no self-consciousness about genitives, and has, in addition, borrowed a number of genitive constructions from AG. One of these, the genitive absolute, was briefly mentioned above; its (rare) D equivalent is a nominative absolute (of the noun; the gerund in -ondas is, of course, uninflected). Of the fifteen normal D prepositions, none is regularly construed with nouns in the genitive; of 30 regular K prepositions, 18 take the genitive, including a group of "improper" prepositions whose D equivalents take sê, apô (or less often ne) when followed by nouns. D has only a handful of verbs which govern a genitive (mostly cases where Kl would have a dative), but in K there are dozens of such verbs. D rarely uses verbal nouns with subjective and objective genitives (though it is less rare in journalesic D, of course), but K is extremely fond of these constructions. The construction of phrases like "glass of water" involves agreement in D, but a partitive genitive in K; and various other less common phrases differ in this same way.

Intermediate Languages

It is often alleged or implied that, if one chose a thousand modern Greek documents at random and sorted them according to language and style, these documents would form a single perfectly continuous series ranging without a break from extreme K to extreme D, and that there would be in the middle at least a few dozen items which could not easily or fairly be classified as one or the other. This is, of course, quite false. Any Modern Greek document can be unambiguously assigned to K or D on the basis of a half-dozen lines or less (in fact, a half-dozen words is normally enough), with very few exceptions.

The two readers prepared by Professor Pappageotes of Columbia University under contract with the Office of Education provide a very good sample of the total range of variation of each language.
There are (chiefly in the K reader) a number of selections whose language is in some measure mixed. Of this “mixed” language there are two main types: (1) a form of D which has been mildly purified by writing dhìa for yà and ìs for s(è), adding a respectable number of final n’s and augments, and occasionally one or two K verb forms (the favorites seem to be -metha for -maste, imperfective middle participles in -ómenos, íto and ísan for itan), and spelling all or most clusters in the K manner; (2) a form of K which has been “popularized” by using a few D words (neuter nouns in -i are most favored), including one or two D prepositions and particles (ya for dhìa and particularly mésa), a few D inflections (especially N.-A. Pl. of feminines in -es, third plural forms in -ne often where literary D would not use them—, imperfective pasts of contract verbs in -úsa), and omitting an occasional -n or augment.

In some cases it is very difficult to say which sort of mixture is being used, but such cases are rare. If we separate the possible differences into four classes, we can, by counting, arrive at ratios or indices of Demoticity or Katharevusianism. The four are: (1) particles (mainly prepositions and conjunctions) and syntactic constructions; (2) inflectional affixes (including augments and reduplications); (3) vocabulary items (chiefly nouns, verbs, and adjectives, and only those where K and D each have an established word in regular use; (4) spellings. Of the four, inflection is without doubt the most important. In what follows we will give a sample of typical counts (per page, in general) of these items (in the form K/D), first in indubitable K documents, then in pure D, then slightly modified K and D, and finally problematical mixed-dialect cases, all selected from the preliminary drafts of Pappageotes’ two readers.

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<tr>
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<td>69/0</td>
<td>16/0</td>
<td>2/0</td>
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<tr>
<td>Papaconstandinu 91</td>
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<td>30/2</td>
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<tr>
<td>Average for pure K</td>
<td>22.2/2</td>
<td>54.6/.9</td>
<td>9.9/0.7</td>
<td>8.2/0.3</td>
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D Reader:

G. Kazantzaki, 4  3/23  1/85  0/8  1/13
Papantoniu, 6  0/24  0/56  3/9  0/21
Megas, 11  3/32  6/69  1/28  0/30
Poriotis, 17  1/18  13/49  4/13  3/15
Palaiologhos, 20  4/20  5/62  12/12  6/12
Papadopulu, 26  0/15  5/54  1/33  7/30
Matsas, 32  3/17  1/68  1/11  3/10
Basias, 38  2/31  3/87  6/9  2/31
Michalopulos, 44  3/24  7/60  10/9  3/5
Kontoghlu, 57  0/48  1/131  1/44  1/33
Average for pure D  1.9/25.2  4.2/72.1  4.8/17.6  2.6/20

From a glance at these figures, it is obvious that pure D is more strongly characterized (in all four ways) than pure K; i.e., K uses more neutral forms and items than D does. It is also clear that D accepts a few more inflections and words from K than vice versa.

Now a sample of mixed forms: first predominately K, then mildly K, then predominately D, and finally one or two where the figures are almost equal.

K Reader:

(newspaper filler), 5  11/7  41/5  9/4  1/1
Moraitidhis, 20  34/6  65/5  6/15  17/1
Dhamveryis, 23  21/5  34/3  5/4  12/2
Khadzopulos, 27  22/4  78/9  15/10  25/2
Mitsakis, 201  45/5  75/9  18/17  4/2

In this group the uses of D items are principally of two kinds: in most of the selections a number of D vocabulary items are used for various special effects, and the other D forms result mainly from the principle of agreement or harmony; in the other selection there is a small amount of dialog, and dialog is generally (in K texts) composed either in good S or in a mildly purified D.

With more D mixture, but still K. (K Reader)

Cheropulos, 19  30/8  88/19  13/13  26/8
Petmezas-Lavras, 25  10/7  48/10  11/3  8/4
(Letter from Nea), 35  19/4  43/10  10/0  1/4
Papaconstandinu, 47-8  32/2  66/12  8/10  6/0
Here the types of D material are the same; the proportion is just a little higher. The first two selections use D principally in dialog, but all show some instances of phrasal harmony.

The next group already includes a few which might be considered modified D, or at least “mixed.”

K Reader:

Kosmas, 12-13  18/17  65/29  6/26  48/16
Skokos, 14    20/10  45/23  5/19  17/5
Xenopulos, 17  6/12  36/22  3/13  12/13
Nirvanas, 30-32 12/13  27/22  9/26  8/2
Trikupis, 71  16/5  51/26  12/4  11/2
Kolokotronis, 180 4/10  11/11  0/8  17/2

Note that almost all of these have a high D vocabulary ratio. Three of these (the first and the last two) are quite old, and all are earlier than Triandaphyllidhis. Hence it becomes important to consider not only how many, but what forms are used and how consistently. Kosmas the Aetolian is represented by four selections. The first (On Charity) is just about as pure D as one could expect in the 18th century: there are seven final n’s, two is, and a dhid, but otherwise (except for a K phrase “in word and in deed”) everything is D. The other three have a little more the appearance of K.

The funeral oration on Byron by Trikoupis is K only to the extent of final n’s, is, dhid, and certain spellings; the contract imperfect is always in -úsa, a-stem plurals are always in -es, aorist passives in -ika, active participles in -ondas. The brief speech attributed to Kolokotronis likewise has only dhia, is, final -n (not consistent), and a few spellings to justify the K label. Much the same applies to Laskaratios, who omits many final -n’s, uses always the -ika aorist passive, the -ondas gerund, and the -es plural (except for one tas). Skokos, on the other hand, uses two -thi aorists, several -os adverbs, and two participles in -on (as well as many final n’s), and even one dative phrase, but the basic pattern is clearly D. Xenopulos uses perhaps fewer peculiarly K forms than Skokos, but inserts the D items only singly, here and there, except in dialog, so that the impression is of a basic K style with a little spicing of D. This is a pretty fair sample of what is meant by “simple” K.

—124—
It is interesting that there are very few specimens with ratios (for endings) between 1/1 and 1/2. The best are these two:

Laskaratos, 15  
19/18  17/28  1/5  16/2

Ghavrielidhis, 150  
10/15  22/28  2/30  3/3

Both are unquestionably D, though not yet strictly according to Tri-andaphyllidhis. The K element consists chiefly of final n's and augments (though each selection has one feminine accusative article tas, induced by harmony with a following K noun ending in -is,) while the D elements include -ika forms, -ondas, many forms without final n, stin, stus, etc.

Laskaratos, 11  
6/7  4/29  1/2  8/1

Laskaratos, 16  
5/20  10/48  4/21  4/24

Oekonomakis, 33  
7/31  19/49  2/9  10/8

Psathas, 36-7  
2/31  8/41  2/10  18/2

Kondihylakis, 181  
12/35  31/72  2/18  5/7

Most of these are pre-1900 and represent pretty good D for those days. The slight K coloring of the Oekonomakis selection is attributable to the journalistic nature of the subject and treatment. If we turn for a moment to the D reader, we can find selections with ratios very much like those in the last set.

Sykokis, 2-3  
2/22  13/47  0/27  11/24

Angelomatis, 51  
3/20  15/38  1/13  5/7

Vrachas, 59  
3/27  7/53  13/7  2/14

Emmanuel, 60  
5/25  11/68  6/5  5/3

Melas, 63  
12/15  15/64  5/2  3/1

Churmuzios, 67  
4/16  8/57  2/4  6/6

Pallis, 98  
5/17  8/29  6/3  7/2

Symeonidhis, 128-9  
3/20  8/38  2/3  2/3

All but one or two of these represent recent journalistic style, where the commonest K elements are augments, middle participles in -ómenos, an occasional third-declension nominative (esp. -sis, ótis), or genitive (-os, -eos). Pallis has some accusative plurals in -as; Churmuzios and Angelomatis several final n's.

Typical ratios from the recent reader by Sapoundzis, Sapoundzis and Hodge:

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<td>1/28</td>
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—125—
Until the last three, these are all fairly pure D (in spite of the implication that this Reader is for a third language, neither D nor K), and the next-to-last one in pure K. The other two are substantially D, with phrases and sentences of K embedded (in one case fairly naturally, by references to a book).

There remain two selections in the K reader where the mixture is very difficult to classify because of wild inconsistencies.

Moraitinis, 21-2 12/11 28/29 11/18 17/3
Vlachu, 40-41 16/23 27/23 5/3 3/3

The Moraitinis selection is over 20 years old, uses forms with and without final -n, nominatives in both -sis and si, plurals in -es and -e (but no A's in -as) numerous iota subscripts, both se and is, both ya and dhia. A sentence will start D and end K. Or one sentence will be good K, and the next good D. This must be called truly "mixed," considered as a whole, but individual sentences or phrases are either clearly D or clearly K.

The Vlachu selection, on the other hand, is recent journalase with an obvious K base. The third declension types (especially the abstracts in -sis and -ōtis) are consistently declined as in K (-sis, -seos, -sin, -ōtis, -ōtitos, -ōtita, etc.) and augments are consistently added. But there are other inconsistencies. First and second declension accusative singulars occur both with and without final n; first declension plurals are given in both styles (D -es, -es, K -e, -as); the adverb is formed either in -os or in -a. There are gerunds in -ondas, contract imperfects in -úsa and third plurals in -ne. Both ya and dhia are used, both is and se, both boró and imboró, both aerodhromio and aerodhromion, for example. However, since most of the D items appear singly or in short (harmonizing) phrases embedded in longer stretches of K or neutral forms, the general impression is not as schizophrenia as the Moraitinis selection.
The vast majority of Greek writers, then, adopt a language which is either clearly D or clearly K; a few K writers (principally journalists) mix in occasional D words and inflections, and a few D writers adopt some K forms. Virtually no modern writers leave any doubt as to which language they are using, and there is no such thing as a consistent intermediate language. The net result of these mixtures is clearly that, as time goes on, K will replace more and more of its peculiar features with corresponding D ones, until, barring external influences, K as a distinct language will disappear. D, on the other hand, shows no strong disposition to borrow anything but vocabulary (nouns, verbs, adjectives).

The Uses of K and D

Pappageotes' K reader has a section called "Literary Texts" about 40 pages in length. It is instructive to note the nature of the 14 selections given here. The first (Kondhylakis) is actually in a mildly purified D, as we noted above, and another one (Mitsakis) is in a K with strong D admixture. Both were written before 1900; most of these selections were written before 1930, (indeed that is the case with over half the selections in the whole book). Two or three of the short pieces used as fillers are not properly literary at all, but encyclopedia articles or the like. Several selections have the character of memoirs; none are in verse.

In the D reader, on the other hand, half the selections are classified as literary; 123 pages of prose (chiefly fiction) and 22 of verse. In both readers the section called "Elementary Texts" (69 pp. in the K reader, 58 pp. in the D) consists mostly of literary pieces; again there is no verse in the K section, but there are seven poems in the D. Furthermore, as we have seen, four or five of the selections in this section of the K reader are either in D or have a heavy D mixture. So we may say:

(1) All verse, prose fiction (including plays) and (personal) essays are in D.

In the sections called "Articles" in the two readers there are several bits of literary criticism or art criticism or essays about writers: four (pp. 154-161) in the K volume, of which three are pre-1930, and the fourth by a member of the Academy, and nine (60, 67-84) in the D volume. This represents the facts pretty well.
(2) All or nearly all criticism—on art, literature, the theatre, etc.—is nowadays written in D. Articles on art in the newspapers are sometimes in K.

A number of the first few selections (pp. 2-9) in the D reader are from Greek Readers used in the elementary school; there are no such items in the K reader. On pp. 236-246 of the K reader there are selections from a number of science textbooks. This does not quite mirror the facts, but does so partially.

(3) In school, the prose in readers for the first three grades (and most of the poetry in all readers) is in D; nearly all other textbooks, from fourth grade through graduate school, are written in K, though there are now a few in D. This is also the case with most reference books, particularly encyclopedias, and learned works on most (but not all) subjects.

In the D reader there are letters on pp. 12-15, in the K reader on pp. 6, 35. But the K letters on p. 6 are formal social notes, and the one on 35 is a formal protest in a letter to the editor (and has a considerable D admixture, at that), whereas the letters in the D reader include some personal letters and two "letters to the editor," both couched in an informal style.

(4) Informal letters to friends or to periodicals are commonly written in D; social or business letters and some "letters to the editor" are written in K.

The D reader contains one selection which might be called religious (p. 40), and that one is a newspaper article; in the K reader (aside from Kosmas the Aetolian) there are three religious selections (67, 102, 106), of which two are from newspapers.

(5) The official language of the Church is K, and most sermons, theological treatises, serious articles on religion, etc., are in K. Occasionally a non-conformist preacher may use D, and the popular press will include some religious pieces in D (particularly for children).

In the K reader a whole section (pp. 70-87) is labeled Official Texts. In addition, pieces by (or about) leading politicians or by King Paul appear on pp. 99, 107, 113, 119, 130, 142 and 210. Pp. 2-10 include a variety of announcements, classified ads, programs, and the like. In the D reader there are only two somewhat political pieces (pp. 63 and 144), and both are quite unofficial and journalistic.
(6) The official language of government, politics, and law is K, and all official documents (licenses, diplomas, certificates, receipts, etc.), official and semi-official announcements, and classified ads, etc., public statements by political figures, laws, legal proceedings, court decisions, decrees, legal announcements, debates in parliament, proclamations, etc., and many political speeches are in K.

Among the "Articles" in the D reader, mostly taken from newspapers and magazines, are five on recent or current history, three on earlier (Byzantine to modern) history, six on the language question and four on science for the layman. In the K reader, besides the political and religious articles already mentioned, there are seven on historical topics, six on the language question, and three (not recent) on literature.

(7) In a few fields articles are written (and books as well, for that matter) with almost equal likelihood in both K and D: history, art history, biography, contemporary problems, science. In some cases the choice of language depends upon the stand taken: obviously a proponent of Katharevousa is not going to argue his case in Dhimotiki; and in politics a conservative Monarchist is not likely to write in D. In history, the period of the Revolution is more likely to be treated in D than are the wars of ancient Athens or Sparta.

Very few selections from the news columns of the daily press are included in these readers (in fact, other collections have been published exclusively of such material). But the D reader does have (291) one account of a basketball game.

(8) In most newspapers all principal news stories and editorials are in K; sports news, social columns (and many special columns) are in D.

To all this it must be added that spoken (or read) K exists in certain formal situations, particularly politics, law, church, and the classroom. And the proponents of K are particularly vigorous in pointing out that the Communists use and advocate D, so much so that, some years ago, one's stand on the language question was of basic political importance. Though the situation today is not quite as bad as it was, it is still the case that few right-wing believers are public users of D, and few socialists or liberals are violent advocates of K. Of course the most loyal supporters of K are the poor people who don't know it and can't use it.
DISCUSSION

PAUL GARVIN: (Thompson Ramo Wooldridge) I found one detail in the presentation which seems to be of very great interest from the standpoint of the analysis of the language, and that is the statement about the fact that when you take over a Demotic noun into Katharevusa, you then also apply with it, the Demotic article. It seems to me, that the use of Demotic forms in the K text might serve as a guide for the analysis of syntactic units that are very closely fused. In other words, those that have to be taken over en bloc could be considered more closely fused than those which can only be partially taken over and where the remainder of the unit can be in the other dialect. Did you observe whether, for instance, Demotic nouns also had to have Demotic adjectives with them, at least from an inflectional standpoint? This would give you a very nice tool for defining certain close orders of syntactic units.

MR. FERGUSON: I would like to ask for a clarification of four terms that were used. They were: simple K, moderate D, journalistic D, and spoken Greek. I think it would help us a great deal to understand the orientation, if you could explain briefly those four terms.

MR. HOUSEHOLDER: This will not take very long because we cannot define them precisely. Simple K is what a man says he is using. That is, a writer says, "I am writing this in simple K." And whatever he writes and calls by that name is simple K. It usually means that he avoids some of the most extreme K forms and mixes in a little bit of D, particularly D prepositions, and maybe one or two verb forms that usually belong to D.

Moderate D means much the same thing as journalistic D. This is the language which is used by writers of articles in D, that appear in journals or newspapers, and it, again, avoids extreme uses peculiar to D or to spoken Greek, or to dialects, and incorporates a large amount of K vocabulary. It is the vocabulary that is most important in moderate D, which, however, is combined with D morphology. It usually does not adopt K vocabulary with K morphology.

Spoken Greek is just anything that people speak. In general, it resembles D, except that it is a lot more free, has more variation and many inflectional endings; it has alternate forms that occur very frequently in spoken Greek, but hardly ever in written D; it freely uses many K phrases, and is very mixed. That is, educated spoken Greek is a very mixed language. You can switch back and forth from a D
context to a K phrase, or even have a whole K sentence in there, all depending on the subject you are talking about. This is due to the fact that on certain subjects the only natural language to use in talking about them is K. Practically all books are written in K, and if you want to talk about them, you naturally use the language that you have seen in the books.

ERIC HAMP: (University of Chicago) The matter of vocabulary was raised just now and this matter occurred to me also in another context during the presentation. I have the strong impression that when Greeks themselves make judgments as to what they are using, they seem to do so overtly on the vocabulary they think they are selecting and not on the grammatical features. In fact, discussion of vocabulary is a very large and important part of Greek folk culture as a whole, and discussion of grammar plays almost no part, relatively speaking. In connection with the degree to which Greeks will label what they are using, I am reminded of a thesis just completed by a student by the name of Demetrius Moutsos¹ dealing with the totality of all entries under the letter phi in all normally accessible Modern Greek dictionaries. He has studied the nouns and the substantives separate from the verbs to see whether there is any difference of behavior. It turns out, it seems, that he had a broad selection of informants from various backgrounds, all the way from an illiterate late-nineteenth century American immigrant to various kinds of learned people from Greece transplanted in the U. S. (The field work was done in this country.) It turns out that in their vocabulary usage, there is no bunching in range over these informants which would reflect any dichotomy at all. In fact, their vocabulary content, if we can use that word, seems to be a direct correlation of their educational level. And in this fashion, if we consider what they have to draw on, and considering the fact that the Greeks make judgments on the basis of vocabulary, Moutsos' findings may inform us on at least two things: first, the native's reaction as to what language he is using at a particular time, second, that while (as Mr. Householder points out) texts can be clearly K or clearly D, I suspect that that is likely to be much more strongly a reflection of the grammar than of the vocabulary, based on the vocabulary access that individuals seem to have on the basis of Moutsos' findings.

¹ The unpublished thesis by Demetrios Moutsos is on the stratification of a sample portion (words in phi-) of the Greek lexicon [University of Chicago, 1962.]
MR. HOUSEHOLDER: All this is quite true. The only part of the vocabulary on the basis of which you can make any judgments are the extremely common words. There would be perhaps two or three hundred very common D words which would not normally be used in a K context, and about the same number of K words which would not likely occur in a D context. But all the rest of the vocabulary, mostly of K origin, is freely used by writers and speakers without any hesitation. And you are quite right in pointing out that this notion that the Greeks themselves have that there is a continuous range, is entirely based upon vocabulary. In vocabulary there is a continuous range from the most extreme K to the most extreme peasant dialect vocabulary.

CHARLES BIDWELL: (University of Pittsburgh) I would like to ask, what is the status of place names in this situation? I have the impression that contrary to the entire rest of the range of vocabulary, in place names the K forms seem to be pushing out D forms. In the older handbooks you can get very long lists of place names where there are different K and D forms, the D form often being of Turkish or Slavic origin, but in present day Greece I have the impression that the K forms of place names used in all official contracts, such as railroad station nomenclature and the like, are to a certain extent replacing the D forms.

MR. HOUSEHOLDER: That is true, and I mentioned that at one point. The reason of course is official government policy. The government, since the formation of an independent Greece, has been trying to restore ancient place names to all of Greece and has substantially succeeded to the point where many of the former spoken names are no longer known by the peasants of the neighborhood. If you go there and ask where such and such a town is they say that they never heard of it and even their grandfathers never heard of it, but you find it mentioned in the accounts of travellers who went there fifty years ago. So you are quite right, those former spoken place names are rapidly disappearing.
WHAT STANDARD FOR DIGLOSSIA?
THE CASE OF GERMAN SWITZERLAND

WILLIAM G. MOULTON
Princeton University

If such a thing is possible within the brief space of three years, I think it is fair to say that Charles A. Ferguson’s 1959 article on “Diglossia” has already become a classic. It has achieved this status not because it presented us with any new data, but rather because it identified and described a particular type of linguistic situation which can arise quite independently in widely separated geographical areas, and which—if we may be allowed to speculate—has probably arisen many times in the past. Stated briefly, and in Ferguson’s words, “Diglossia is a relatively stable language situation in which, in addition to the primary dialects of the language (which may include a standard or regional standards), there is a very divergent, highly codified (often grammatically more complex) superposed variety, the vehicle of a large and respected body of written literature, either of an earlier period or in another speech community, which is learned largely by formal education and is used for most written and formal spoken purposes but is not used by any sector of the community for ordinary conversation” (p. 336).

Among the various examples of diglossia which Ferguson describes—Arabic, Swiss German, Haitian Creole, Greek—Swiss German stands out as being, apparently, much the most sophisticated and self-conscious. Ferguson reports that, through a kind of self-deception, many speakers of Arabic and of Haitian Creole go so far as to deny the very existence of the dialect speech which they use almost exclusively in everyday life. Such an attitude would be unthinkable in Switzerland. Every adult speaker is fully conscious of the distinction between standard and dialect, even though some do not control the standard very well. Further, the more educated and

2 My knowledge of Swiss diglossia comes in part from the literature on the subject, in part from a year’s residence in Zurich (1958-1959) during which time I worked with the materials of the Sprachatlas der deutschen Schweiz. I would like to express my gratitude to the editor of the Atlas, Professor Rudolf Hotzenkocherle, for his kindness in letting me use these materials; and to the American Council of Learned Societies for a grant which made my year of Swiss dialect research possible.

—133—
sophisticated a speaker is, the more he tries to make the distinction between standard and dialect as sharp and clear as possible. Like so much else in Swiss life, the use of language is well regulated, and each mode of speech is neatly assigned to its proper place within the general scheme of things.

This complete awareness of the distinction between dialect and standard is reflected in a number of phenomena which seem to be unique to Swiss diglossia. Scholarly interest by the Swiss in the analysis and description of their many local dialects extends back over a century and a half, beginning with the work of Franz Joseph Stalder. In 1862, motivated partly by a mistaken fear that dialect speech was on its way toward extinction, work was begun on a far more ambitious national dialect dictionary, the Schweizerisches Idiotikon. Publication began in 1881, and is still continuing; it is carried on by a full-time staff of scholars in Zurich. A landmark in the history of dialectology—whether in Switzerland or elsewhere—was the publication in 1876 of J. Winteler’s Die Kerenzer Mundart des Kantons Glarus (Leipzig & Heidelberg, 1876), a work which to a considerable extent anticipated modern phonemics and even the theory of the over-all pattern. The 20th century has seen the publication of large numbers of dialect descriptions, notably the 20 volumes of the Beiträge zur schweizerdeutschen Grammatik (Frauenfeld, 1910-1941), edited by the late Albert Bachmann; and the 11 volumes—to date—of the Beiträge zur schweizerdeutschen Mundartforschung (Frauenfeld, 1941 ff.), edited by Rudolf Hotzenköcherle, Bachmann’s successor at the University of Zurich. As I was writing the first version of this paper, I received a prospectus announcing that the first volume of a linguistic atlas of German Switzerland, edited by Hotzenköcherle, would soon be off the press.

Works of this type, written for a scholarly audience, prove only that the distinction between standard and dialect in Swiss diglossia is clearly recognized by Swiss scholars. But there are other signs that this awareness extends throughout the whole population. As early as 1921 there appeared a textbook written specifically to teach the local dialect: Karl Stucki, Schweizerdeutsch: Abriss einer Grammatik mit Laut-und Formenlehre (Zürich, 1921). (There is, of course, no such dialect as “Schweizerdeutsch;” what Stucki’s book teaches is Zurich German.) This was followed in 1948 by Albert Weber,

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3 Probe eines schweizerdeutschen Idiotikons, Aarau, 1806. Revised and enlarged as Versuch eines schweizerischen Idiotikon, 2 vols., Aarau, 1812. Cf. also Die Landessprachen der Schweiz, oder schweizerische Dialektologie, Aarau, 1819.
Zürichdeutsche Grammatik (Zurich, 1948), a work which bears the significant subtitle: *Ein Wegweiser zur guten Mundart* ("A Guide to Good Dialect"). I do not know whether this book found the wide popular audience which its author hoped it would. But I do find it highly significant that the publisher (Schweizer Spiegel Verlag) was sufficiently encouraged by its sales to follow it with several more books of the same sort. A guide to good Lucerne dialect was published in 1960 (Ludwig Fischer, Luzerndeutsche Grammatik); this was followed in 1961 by a "Zurich German Dictionary for School and Home" (Albert Weber and Jacques M. Bächtold, Zürichdeutsches Wörterbuch für Schule und Haus); and a combined grammar and dictionary of the dialect of the canton of Zug has been announced for the near future.

I mention these various works because I gather they would be inconceivable in the other diglossias which Ferguson describes. But there is more to come. During the 1940's there was a successful "Swiss German School" in Zurich, where Auslandsschweizer (native Swiss who have spent most of their lives abroad) and foreigners could learn how to speak the local dialect. This was desirable from a social point of view, since only dialect is spoken at normal social gatherings, whether of humble folk or of the cocktail set. But—a very significant point—it was also necessary for more practical reasons. Any candidate for citizenship in the canton of Zurich—and, thereby, for federal citizenship—is required, as an earnest of his intentions, to demonstrate at least some knowledge of local dialect. Again, I gather that such a thing would be inconceivable in other diglossias.

All of the things I have described are clear evidence that the diglossia of German speaking Switzerland is extremely stable. Ferguson is surely right when, in his prognosis for the next two centuries, he gives to Swiss German the label "relative stability" (p. 340). Such prognoses are, of course, hazardous. Just 58 years before Ferguson, in 1901, another linguist made precisely the opposition prediction for Swiss diglossia. Taking due account of the high percentage of foreigners in the large cities at that time—28% in Schaffhausen, 29% in Zurich, no less than 35% in Basel—and recognizing the greater prestige which standard German enjoyed among

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4 Cf. Paul Zinsli, "Hochsprache und Mundarten in der deutschen Schweiz," *Der Deutschunterricht*, VIII (1956), ii, 61-72, esp. p. 65. This article is full of valuable information on Swiss diglossia.
all segments of the population, Ernst Tappolet predicted, more in sorrow than in anger, that the diglossia of German Switzerland would not outlast another half century. "For," he wrote, "where a dialect is in conflict with a written language—and I believe I have shown this to be the case in German Switzerland—the standard language will win out if political conditions remain unchanged" (p. 39).

What happened in the half century following 1901 to prove Tappolet's prediction so wrong and to make Ferguson's seem so right? The answer can be given in three short phrases: World War I; Adolf Hitler; World War II. During each of these three major catastrophes of 20th century Europe, the Swiss firmly held themselves aloof—first refusing to be embroiled in the senseless conflict of World War I, next repulsed and horrified by the inhumanities of Nazism, and then stubbornly maintaining their neutrality in World War II even though all the world knew where their sympathies lay. As an indication that this fierce neutrality still continues today, let us note the fact that Switzerland joined the economic group called the "Outer Seven" only with the greatest reluctance, and that it still refuses to join the United Nations. A people with this passion for independence will not soon give up its local dialects.

Diglossia depends, of course, both on the maintenance of local dialects and on the continued acceptance of a separate but related standard language. Is Swiss independence so strong that it will abandon diglossia by rejecting the standard language of Germany in favor of a new standard of its own? Again, predictions are hazardous; but at the moment such a development seems unthinkable. The only possible candidates for a standard Swiss German are the dialects of Basel, Bern, and Zurich; but the whole Swiss Federation would have to collapse before a Baseler would be willing to learn Bärndütsch, a Berner learn Züritüütsch, or a Zuricher learn Baaseldütsch. This would be the most inconceivable development of all. Secondly, the Swiss are too practical and reasonable: they accept standard German as a kind of necessary evil because it would be unreasonable for so small a country to add yet another standard language to a world that is already overburdened with them. Finally, the Swiss are proud of their own share in the great cultural heritage which is recorded in standard German. In this connection it is worth noting that the only two really first-rate dramatists writing

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6Über den Stand der Mundarten in der deutschen und französischen Schweiz, Zurich, 1901; = Mitteilungen der Gesellschaft für deutsche Sprache in Zürich, Heft 6.

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in the German language today are both Swiss: Friedrich Dürrenmatt and Max Frisch.

Though the Swiss are too practical and too proud to abandon standard German as their second language, they are also too independent to feel that they must accept it in exactly the form that it has in Germany. For over 60 years now, the phonology of standard German has been carefully codified in a work generally known as the *Siebs*, after its first editor, Theodor Siebs. 6 To oversimplify, the type of spoken language prescribed by the *Siebs* might be said to consist of south German phonemes pronounced with north German allophones. And it is precisely this north German element in standard German which the Swiss cannot stomach. It strikes them as too sharp and too racy, it carries overtones of Prussian militarism, and it even bears a suggestion of concentration camps looming in the background. 7 This type of German must be avoided at all costs. They are willing to accept—indeed, they demand—a neutral sort of standard German on the stage; but in all other situations they wish to use a German which is standard, to be sure, but which nonetheless clearly sounds Swiss rather than German. 8

In order to codify such a Swiss version of standard German, a special "Swiss Siebs Committee" was established in 1954. Three years later the results of its deliberations were published in a little booklet which I shall refer to as the *Boesch*, after its editor, Bruno Boesch. 9 If I were to give this booklet a frivolous title, I would call it: "How to Speak German Without Sounding Like a German"—or, more accurately, "... and Still Sound Like a Swiss." In the re-

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7 Cf. the following in the *Boesch* (see below): "Die Hochsprache ist unserem Gemüt, besonders in ihrer norddeutsch-umgangssprachlichen Färbung, wie wir sie im Kino und sogar gelegentlich im Theater vorgesetzt bekommen, zu 'preußisch', zu schneidig, zu schnittig. Gewiss, das sind Gefühlsmomente, die schwer zu umschreiben sind; aber sie sind vorhanden, nicht nur bei Schweizern, auch bei Bayern, Schwaben, Österreichern. Dazu lassen politische Gründe aus der Vergangenheit in vielen Kreisen noch heute keine rechte Freude an der gepflegten Hochsprache aufkommen" (13). Cf. also the reference on p. 7 to the "überspitzte norddeutsche Aussprache."

8 Cf. the following in the *Boesch*: "Beim Schauspieler ist es uns peinlich, den Schweizer herauszuhören; in allen andern Sprechsituationen ist es ebenso peinlich, ihn nicht zu vernehmen, ganz besonders peinlich ist es aber, wenn er sich nach gewundenen Anstrengungen schliesslich doch verrät" (14).

mainder of my remarks I should like to compare the phonemic sys-
tems of Siebs-German and Boesch-German, to show the interesting
ways in which they differ.

Siebs-German has the following vowel system, presented here in
terms of distinctive features:

<table>
<thead>
<tr>
<th>Lax</th>
<th>Tense</th>
<th>Diphthongal</th>
<th>Unstressed</th>
<th>Long</th>
<th>Nasalized</th>
</tr>
</thead>
<tbody>
<tr>
<td>i</td>
<td>y</td>
<td>u</td>
<td>i</td>
<td>y</td>
<td>u</td>
</tr>
<tr>
<td>e</td>
<td>œ</td>
<td>œ</td>
<td>e</td>
<td>o</td>
<td>o</td>
</tr>
<tr>
<td>A</td>
<td>a</td>
<td>ae</td>
<td>ao</td>
<td></td>
<td>å</td>
</tr>
</tbody>
</table>

Redundant features and other details:

(1) The Siebs describes lax vowels as short, non-lengthenable,
open (except /A/), and as joined closely to a following consonant
(fester Anschluss); they are also centralized, i.e., /i/ is lower and
more central than /i/, /u/ is lower and more central than /u/, /A/
is higher and hence more central than /a/, etc. Distributionally they
are checked: they occur only before consonants.

(2) The Siebs describes tense vowels as short when unstressed,
long when stressed, lengthenable, close (except /a/), and as joined
loosely to a following consonant (loser Anschluss); they are also de-
centralized, i.e., /i/ is higher and farther front than /i/, /u/ is higher
and farther back than /u/, etc. Distributionally they are free: they oc-
cur before consonants, before vowels, and in word-final position. The
Siebs describes /a/ as being only very slightly lower than /A/, and
treats length as if it were the relevant opposition between the two,
writing /a/ vs. /a:/ The opposition between these two phonemes
is suspended in unstressed position.

(3) The Siebs states clearly that the only two vowels in which
length is the sole distinguishing feature are /e/ and /e:/; it does not
indicate whether this opposition is to be maintained in unstressed
position. It should be added that /e:/ is a very unstable phoneme
for many speakers of standard German: it is prescribed wherever
the spelling shows the letter ä representing a long vowel, but many
speakers—especially in the north—frequently substitute /e/ for it.

(4) The nasalized vowels occur only in words borrowed from
French. Boesch-German has the following vowel system:
<table>
<thead>
<tr>
<th>Short</th>
<th>Long</th>
<th>Diphthongal</th>
<th>Unstressed</th>
<th>Nasalized</th>
</tr>
</thead>
<tbody>
<tr>
<td>[i]</td>
<td>[u]</td>
<td>[iː]</td>
<td>(iə)</td>
<td>[uə]</td>
</tr>
<tr>
<td>[i]</td>
<td>[y]</td>
<td>[iː]</td>
<td>(iə)</td>
<td>[uə]</td>
</tr>
</tbody>
</table>

\[
\begin{align*}
\text{Discussion:} \\
\text{(1) Two deviations from Siebs-German are minor and unimportant. First, where the Siebs prescribes} \ /æ/, \text{with lip rounding maintained throughout, the Boesch prescribes} \ æ/, \text{with rounded syllabic vowel but unrounded glide. (This is also the commonest pronunciation in Germany, despite the Siebs.) Secondly, Boesch-German uses the ingliding dipthongs} \ /iə\ yə\ uə/ \text{only in Swiss names spelled with} \ iε, \ iε, \ uε \ (uο): \text{Brien\ss}, \ Flüelen, \ Hueb (Ruoff). \text{These are therefore “foreign phonemes,” used only in dialect loans, much like the nasalized vowels which are used only in French loans.} \\
\text{(2) Far more interesting are the following two differences between Siebs and Boesch: (a) where the Siebs prescribes lax (and, redundantly, open and centered)} \ [i \ y \ u], \text{the Boesch permits either open} \ [i \ y \ u] \text{(presumably not centered, following Swiss articulatory habits; the Boesch does not mention this feature) or close} \ [i \ y \ u], \text{depending on what the speaker uses in dialect speech; (b) the Boesch prescribes a contrast, similar to that in the dialects, between close} \ /e/ \text{and open} \ /e/: \text{Esche vs. Wäsche, Held vs. hält, wetten vs. hätten, Netz vs. Schütze. These two deviations from the Siebs produce three structural changes in the system of the monophthongs. First, the vowel inventory is increased by one, through the addition of} \ /e/. \text{Secondly, the system of distinctive features is changed in that the opposition “short ≠ long,” which is redundant in Siebs-German (except for} \ /e/ \text{vs.} \ /e:/), \text{now becomes relevant, replacing the opposition “lax ≠ tense.” Thirdly, the long} \ /e:/ \text{of Siebs-German is no longer structurally isolated (and hence unstable), but is now well integrated (and hence stable). (The Boesch mentions the common north German use of} \ /e/ \text{for} \ /e:/ \text{—} [\text{̥}pɛ:t] \text{spät rather than} \ [\text{̥}pɛ:t] \text{—as one of those north German pronunciations which are particularly offensive to Swiss ears.)} \\
\text{(3) Distribution. In Siebs-German, the lax vowels are checked, and hence occur only before consonants. In Boesch-German, the corresponding short vowel are checked in native words, but short open} \ /e/ \text{occurs in word-final position in such French loans as} \ Bud-}
\end{align*}
\]
get, Couplet, Filet. (Siebs-German uses tense /e/ in words of this type.)

(4) Incidence. Though the short and long vowels of Boesch-German generally correspond, respectively, to the lax and tense vowels of Siebs-German, there is some difference in incidence. For example, the stressed vowels of the following words are tense in Siebs-German but short in Boesch-German: Jagd, Krebs, Obst, Nische, düster, pusten, Art, Pferd, Geburt, Fabrik, Notiz, Wuchs. Contrariwise, the stressed vowels of the following words are lax in Siebs-German but long in Boesch-German: brachte, Rache, rächen, Amboss (2nd syllable).

(5) Glottal stop. Previous to 1933, the Siebs prescribed the north German practice of using for all vowels in syllable initial position an allophone consisting of glottal stop plus vowel: [t̚Alt] alt, [t̚o:na] ohne, [f̚ə:en] Verein [t̚über t̚Al] überall, etc. In 1933 it made the glottal stop optional, but still prescribed a clear onset of the vowel (i.e., open juncture): /f̚r-ə'en/, /'yibar-f̚-'Al/. The Boesch prescribes ONLY the latter treatment, without glottal stop. (Though the Boesch does not specifically say so, the glottal stop is another north German feature which the Swiss find objectionable.)

Siebs-German has the following consonant system, presented here in terms of distinctive features:

<table>
<thead>
<tr>
<th>Stops</th>
<th>voiceless</th>
<th>p</th>
<th>t</th>
<th>k</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>voiced</td>
<td>b</td>
<td>d</td>
<td>g</td>
</tr>
<tr>
<td>Fricatives, voiceless</td>
<td>f</td>
<td>s</td>
<td>ž</td>
<td>[ɕ~x]</td>
</tr>
<tr>
<td>Nasals</td>
<td>h</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Liquids</td>
<td>v</td>
<td>z</td>
<td>ž</td>
<td>j</td>
</tr>
<tr>
<td></td>
<td>m</td>
<td>n</td>
<td>ɻ</td>
<td></td>
</tr>
<tr>
<td></td>
<td>voiced</td>
<td>l</td>
<td>[r~R]</td>
<td></td>
</tr>
</tbody>
</table>

Redundant features and other details:

(1) Voiceless stops and fricatives are fortis; voiceless stops are also aspirated except when they are in close juncture with a following fricative.

(2) Voiced stops and fricatives are lenis. Distributionally they do not occur in word-final position.

(3) [ɕ] and [x] are in complementary distribution: [ɕ] occurs initially, after front vowels, after liquids and nasals, and in the diminutive suffix -chen; [x] occurs after non-front vowels. However,
\[\zeta\] has quasi-phonemic status: first because it occurs in -chen, regardless of what precedes it (i.e., even after non-front vowels); secondly because it is the voiceless counterpart of voiced /\j/.)

(4) The Siebs specifically describes /\j/ as a voiced palatal fricative, corresponding to voiceless \[\zeta\], and states that it should not be pronounced as a semivowel. (This is contrary to widespread usage, especially in the south.)

(5) Though the Siebs sanctions the use of both apical [r] and uvular [\R], it expresses a preference for the former. Previous to 1933 it prescribed only [r]. It should be added that, though the Siebs condemns such a practice, popular pronunciation uses the allophone [\R] (non-syllabic, lower and farther back than [\a]) for postvocalic /r/. This is consistently used after tense vowels: ['thi:R] Tier (vs. prevocalic [\R\~\R] in ['thi:ra~'thi:ra] Tiere). After short vowels it is used in free variation with [\R\~\R]: ['vir\~\ir] wirr (vs. prevocalic [\R\~\R] in ['vir\~\ir\~\ir\~\ir] wirre). Postvocalic /ar/ regularly gives the phone [\a] (syllabic), in which the syllabicity may be ascribed to the /\a/, the quality to the /r/: ['besa] = /'besar/ besser (vs. prevocalic [\ar\~\ar] in ['besara~'besa\~\ra]'besere).

(6) The phoneme /\z/ occurs only in loanwords.

**Boesch-German** has the following consonant system:

<table>
<thead>
<tr>
<th>Stops,</th>
<th>fortis</th>
<th>p</th>
<th>t</th>
<th>k</th>
</tr>
</thead>
<tbody>
<tr>
<td>lenis</td>
<td>b</td>
<td>d</td>
<td>g</td>
<td></td>
</tr>
<tr>
<td>Fricatives, fortis</td>
<td>f</td>
<td>s</td>
<td>schläf</td>
<td></td>
</tr>
<tr>
<td>lenis</td>
<td>v</td>
<td>z</td>
<td>schläf</td>
<td></td>
</tr>
<tr>
<td>Nasals</td>
<td>m</td>
<td>n</td>
<td>j</td>
<td></td>
</tr>
<tr>
<td>Liquids</td>
<td>l [r R]</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Semivowels</td>
<td>w j</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

**Discussion:**

(1) Stops. In Siebs-German, the relevant opposition which distinguishes /p t k/ from /b d g/ is "voiceless \[\neq\] voiced," redundant oppositions are "fortis \[\neq\] lenis" and, in part, "aspirated \[\neq\] unaspirated." In Boesch-German, on the other hand, all stops are voiceless. The relevant opposition which distinguishes /p t k/ from /b d g/ is therefore "fortis \[\neq\] lenis." (On the redundant opposition "long \[\neq\] short," see below.) Following the Siebs, the Boesch states that /p t k/ should be aspirated (presumably everywhere except in close juncture with a following fricative; details are not mentioned). This is in sharp
structural contrast with the dialects, where \([p^b \th^h k^b]\) are clusters of two phonemes, and not allophones of single phonemes. For example, the phonetic sequence \([\th^h u:t]\) gives three phonemes in Siebs-German /tut/, Boesch-German /tu:t/ (er) tut; but it gives four phonemes in dialect /\th u:t/ d'\Huut 'the skin' (standard die Haut).

(2) Fricatives. The only fricatives which the Boesch describes clearly are /s/ and /z/, and here Boesch-German differs from Siebs-German in just the same way as in the case of the stops. In Siebs-German, the relevant opposition which distinguishes /s/ from /z/ is "voiceless \(\neq\) voiced;" the opposition "fortis \(\neq\) lenis" is redundant. In Boesch-German, on the other hand, both /s/ and /z/ are voiceless; the relevant opposition which distinguishes them is therefore "fortis \(\neq\) lenis." (On the redundant opposition "long \(\neq\) short," see below.) The same opposition also distinguishes fortis /\$/ (as in /'tao\$en/ tauschen) and (voiceless) lenis /\$/ (as in /ba'ga:\$a/ Bagage). Though the Boesch might not approve (it does not discuss the matter clearly), the same opposition is commonly used also to distinguish fortis /f/ (as in /'zla:f\$en/ schlafen) and (voiceless) lenis /f/ (as in /'ha:v\$en/ Hafen or /'skla:v\$en/ Sklaven). Some dialects likewise have an opposition between fortis /\x/ and (voiceless) lenis /\y/; possibly their speakers also carry this habit over into standard German.

(3) \([r-\]\). The Boesch expresses no preference for either apical /r/ or uvular /\r/, but suggests simply that each speaker continue to use whichever allophone he already uses in his dialect speech. It is worth noting that the Swiss dialects do not show the allophone [\x] which is so common for postvocalic /r/ in Germany and Austria. Hence Swiss speakers have no trouble in following the Siebs prescription of [r] or [\r] even in postvocalic position.

(4) Long consonants. In the Swiss dialects, length is a redundant feature of fortis obstruents. Roughly speaking, fortis obstruents are long after short vowels: /'of\$a/ = /'ofs/ offen, and half-long after long vowels: /'\$la:f\$a/ = /'\$la:fe/ schlafen, and after consonants: /'h\$elf\$a/ = /'h\$elf\$a/ helfen. Length is also a redundant feature of fortis nasals and liquids in those dialects which extend the opposition "fortis \(\neq\) lenis" to this part of the consonant system: /'\$w\$m\$a/ = /'\$w\$m\$a/ schwimmen, etc. The Siebs (p. 59) strongly condemns all such long allophones; the Boesch (p. 27) just as strongly supports them wherever the standard spelling writes double letters. Hence Siebs [\cfe\$a] = /'cfe\$en/ offen vs. Boesch [\cfe\$a] =
(5) Semivowels. The Siebs states that /j/ should be articulated as a voiced palatal fricative, and thus classes it as an obstruent; the Boesch, on the other hand, states that it should be articulated as non-syllabic i and thus classes it as a semivowel. Concerning the phoneme represented by the letter w, the Boesch is less clear. The Siebs describes it as a voiced labiodental fricative, and hence makes of it an obstruent, transcribed /v/; the Boesch, on the other hand, states merely that w "ist stimmhaft und kräftig zu artikulieren" (p. 29). This description of w as "voiced" indicates that it is not an obstruent, since all Boesch obstruents are voiceless. Accordingly, I have classed w as a second semivowel, transcribed /w/. Presumably the Boesch accepts its dialect articulation as a non-syllabic vowel with slight bilabial and/or labiodental friction.

(6) Siebs /f/—/v/, Boesch /f/—/v/—/w/. In both word-initial and intervocalic position, Sibe-

S,German shows only the two-way opposition /f/—/v/: voiceless /f/ in /'faen/fein, /'hafa/Höfe; voiced /v/ in /'vaen/ Wein, /'lov/ Löwe. In word-initial position, Boesch-German also shows only a two-way opposition; but since fortis /f/ does not occur word-initially, it is not /f/—/v/ but rather /v/—/w/: (voiceless) lenis /v/ in /'vaen/ fein, (voiced) semivowel /w/ in /'wain/ Wein. In intervocalic position, however, Boesch-German shows the three-way opposition /f/—/v/—/w/: fortis (long) /f/ in /'of an/ offen, (voiceless) lenis (short) /v/ in /'o:van/ Ofen and /'hova/ Höfe, (voiced) semivowel /w/ in /'lov/ Löwe. Siebs /v/ regularly corresponds to Boesch /w/ (for some differences in incidence, however, see below); but Siebs /f/ corresponds both to /f/ and to /v/ in Boesch. As a result, Boesch-German has one more consonant phoneme than Sibe-

S,German.

(7) Distribution. In Sibe-

S,German, the opposition "voiceless ≠ voiced" is suspended in word-final position, where only voiceless obstruents occur. Thus, to illustrate with the examples cited in the Boesch, the words grob, Sod, genua end not in voiced /-b -d -g/ but in the same voiceless (and aspirated) /-p -t -k/ as Stopp, Lot, Spuk. In Boesch-German, on the other hand, the corresponding "fortis ≠ lenis" opposition is maintained in word-final position: the first three of these words end in (voiceless) lenis /-b -d -g/, the second three in fortis (and aspirated) /-p -t -k/. Though the Boesch does not mention it,
we may assume that fricatives also maintain the opposition "fortis ≠ lenis" in word-final positions: fortis /f/ in /'za:f/ Schaf vs. lenis /v/ in /'bra:v/ brav; fortis /s/ in /'haes/ heiss vs. lenis /z/ in /'aiz/ Eis.

(8) Incidence. We have seen above that Boesch-German generally renders the Siebs voiced fricative /v/ with its voiced semivowel /w/: Wein, Löwe, etc. In a good many words of foreign origin, however, Boesch-German renders the Siebs voiced /v/ with its voiceless lenis /v/: Klavier, November, Provinz, etc. (Cf. Boesch, p. 29.) A far more striking difference in incidence concerns the suffix spelled -ig. Though Siebs /g/ is otherwise unvoiced to /k/, in this one suffix it is usually to be unvoiced to /ç/: voiced medial /g/ in /eviɡə/ ewige, but unvoiced final /ç/ in /'eviç/ ewig. This is yet another north German practice which is offensive and unacceptable to the Swiss. The Boesch therefore prescribes voiceless lenis /g/ both medially and finally: /eːwig/ ewig just like /'eːwiga/ ewige.
DISCUSSION

PAUL GARVIN: (Thompson Ramo Wooldridge) I thought that the final portion of Dr. Moulton's paper was reminiscent of the discussion yesterday after the paper by Prof. Haugen, inasmuch as it does relate to a realistic linguistic description of a norm, and apparently in the case of Switzerland this realism was carried pretty far. That is, the Swiss are apparently describing the pronunciation of standard German as they really pronounce it, rather than having a highly unrealistic description in terms of sounds that might be repugnant to the Swiss.

MR. MOULTON: It is quite true that this is to a considerable extent the way they actually do it, although of course you can hear people who think they are speaking in standard German who are almost unintelligible if you do not know Swiss German. This shows, in a sense, the limits of just how much you can deviate, and also how far away from the standard Siebs German you ought to stay.

CHARLES BIDWELL: (University of Pittsburgh) I would like to ask if the situation, in the case of Swiss diglossia, is such that in every region you have the one standard language, namely, the Swiss variety of standard German, and then you have a local variety of what Dr. Ferguson called, I think, "the lower language." Or, do you have perhaps the sort of situation that exists in the Slovenian area, where there is one codified standard language which nobody ordinarily speaks, and also various regional colloquial dialects which are still distinct from local peasant dialects?

MR. MOULTON: Dialect standards hardly exist. You do find the following: people who work, let us say, down in Wallis in a ski resort, will, besides their local dialect have some sort of vaguely Bern Dütsch form which they use with the people who come down from the north; but one could not describe this as a sort of local standard. There are vague attempts at it, but nothing more than that. Perhaps one might ask, why down in Wallis, for example, the local people attempt to do this? The answer is quite simple; if they really talked Wallis German, the Swiss would hardly understand them.

WALLACE CHAFE: (Smithsonian Institution) I have the impression that one of the most distinctive differences between standard German as spoken in Germany and as spoken in Switzerland is a difference in intonation. I wonder if you would agree that this is so, and whether Bösch makes any mention of this, or whether anybody has even studied this in any detail?
MR. MOULTON: Since I was speaking here of standard languages, I thought that what I ought to do was quite specifically to limit myself to the standard language as described in the accepted two volumes, the Siebs, and the Bösch. I don’t dare say that neither one says anything about intonation, but there is nothing about intonation that you or I could do anything with. Nor is there really anything on stress which also plays a role. So, I have included only what is in the two books.

RICHARD HARRELL: (Georgetown University) To what extent is there a written literature in Swiss German?

MR. MOULTON: There is some dialect literature, but relatively little; it is not widespread. The ordinary person is quite incapable of writing anything that could be called dialect; the only written form he possesses is standard, and regardless of how he pronounces it, it is written in the standard form. I suppose I would call the existing dialect literature slight because I have no standards to judge it by. It has existed for quite a while, but it is just not very widespread, and people do not do much with it.

CHARLES FERGUSON: (Center for Applied Linguistics) I wonder if I could comment on that, because of the words “relatively little” compared to the Arab world, you might think the Swiss dialect literature is quite large. There are in Swiss German historical novels for example, books of poetry and so forth, and I don’t know of a single full length historical novel in spoken Arabic, although there are some very short ones.

COLEMAN O’HUALLACHAIN: (Georgetown University) Could you tell us to what extent a standard is used in the schools for teaching German? What would be asked in a school play, for instance, that would not be mimicking a Prussian drama? (A serious play, not a dialect play.)

MR. MOULTON: I fear I simply have to say I don’t know enough about it. I do know that in the beginning grades in school they use dialects, but the first task in school is to learn the standard language which very soon becomes the sole medium of instruction. Of course, as soon as the class period is over the teacher and the children will talk to each other in dialect, but during the class, all class work is carried on in the local version, whatever it may be, of the standard language. About plays, if it were a serious play I am sure it would be in standard German, but I am only guessing here because I don’t know. The only play I could conceive of that would
be acted in dialect would be something that the pupils have written themselves, but there may be some dialect plays they could perform.

FR. O'HUALLACHAIN: But my question is mainly regarding the standard. What is accepted as the thing to be aimed at by an educational institution? Is it Bösch standard or Siebs standard?

MR. MOULTON: Never Siebs German, only Bösch German! One would not dream of trying to make the pupil use glottal stops and say 'König' or make him change [bitte] to [bite]. One would never dream of doing this! All the things described in the Bösch are not only merely acceptable—you do not want to use anything else!

AGATHA BURCKHARDT: (Embassy of Switzerland) I am a member of one of those self-conscious tribes in German Switzerland, and I want to thank you very thoroughly and very heartily for the excellent presentation you have given. You have made me laugh all the time, because I think you have with a loving ear listened to the Swiss diglossia. However, I would like to try to give a few answers to questions which have been asked. About written literature in Swiss German, I would say the father of written literature in Swiss German is Johann Peter Hebel, [1760-1826] who was of Alemannic origin from southern Germany and born in Basel. He has written the famous Alemannische Gedichte in his idiom which is closely related to the lower Alemanisch dialect of Basel city. And he also wrote, in what you call standard, the Schatzkästlein des Rheinischen Hausfreundes [1811]. In the same author's writings, we find standard prose and lyrical poetry in dialect. I would only mention that there is a fairly large production of lyrical poetry in all Swiss German dialects. Another famous Swiss author, Jacob Burckhardt [1818-1897], the historian of the cultural history of the Renaissance, has written a very small, very intimate volume of love poetry, in Baseldietsch. It is probably not spoken Baseldietsch but an in-between dialect which leans towards the Alemannische of Hebel. The language has been changed through the years, of course. In Basel today, we have a large production of political satire in local dialect, which is written for the carnival.

About the question of school plays. Each higher, secondary school produces plays, and always in High German not in dialect.

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This is because we aim at teaching a good standard, even if it is not the Siebs. Incidentally, you did not stress enough the high repulsiveness of the voiced /z/ which is so Siebs. We cannot pronounce it, or if we do, or try to, we are criticized as being un-Swiss, as you would be criticized if you pronounced English in the British manner. Plays are studied in the best standard possible. And very often for the stage, Siebs pronunciation is aimed at, even if it is not realized.

JAMES REDDEN: (Foreign Service Institute) The most interesting part to me is the Swiss 'a' phoneme. About 5 years ago I made a phonemization of a certain brand of Middle High German used in Switzerland, and I was able to phonemicize the 'a' out. I'd like to know if this is a recent development or if this is really phonemically distinct, or just an insistence on using the right allophone in the right place? What exactly is the condition there?

MR. MOULTON: It would be impossible to cover in one brief statement what all the dialects do. But let me sketch it roughly. Take a triad of words which in standard German all have the same vowel like "Vetter, "Wetter, Wespe". Some dialects in the north-east still have the old Middle High German three-way contrast, that is, there these three words sound like: [fettar, wettar, waespi]. There is another group, in the north in Schaffhausen, which has [fettar, wettar, wespi], a two-way contrast, the last two being identical. Then, there is a small area in the Canton of Glarus where the first two are identical and the third is different. It is [fettar, wettar, waespi]. But in the largest part of the area, including all of the west, it is either: [fettar, wettar, waespi], or [feettar, wettar, waespi]. So all of the dialects have two vowels here. The Swiss would not accept that. What they have done then, and I really do not know how individual speakers carry this out, is to take the widespread two-way differentiation, particularly the one that goes all over the west, move up that lower [æ] to an [e] and speak standard German this way.
In an article published in 1959, Charles A. Ferguson directed the attention of linguists to a special kind of bilingualism in which a speech community may use two varieties of the same language under different conditions or for purposes. To designate such a situation, Ferguson suggested use of the term *diglossia*. If this terminological refinement is accepted, then the term *bilingualism* itself can be reserved to indicate situations where the co-occurring languages are sufficiently unalike in all ways as to be readily distinguishable from each other at any level, and where they may well serve as the linguistic aspects of two relatively autonomous and perhaps competitive cultures. Situations involving bilingualism, in this specialized sense, can be expected to be fairly unstable and to eventually result in the dominance of one of the languages over the other.

In contrast, a diglossia situation is one in which the juxtaposed linguistic systems are sufficiently alike in some ways to encourage their structural fusion at certain points. This, in turn, allows for enough mutual identification of the two systems on the part of their users that they may function as situational variants of each other. Such a functional complementation of two linguistic systems is characterized by more stability than is usual in other kinds of bilingualism, so that diglossia situations may endure for considerable stretches of time without any serious encroachment of one of the languages upon the domain of the other.

At about the same time that the Ferguson article on diglossia appeared, Martin Joos made a preliminary classification of English style variation in terms of five levels of usage, i.e. intimate, casual, consultative, formal, and frozen, ranging from maximum informality to maximum formality. As isolated by Joos, style variation occurs largely within that part of linguistic behavior which is usually de-

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scribed in structural analysis as “free” in that it not easily specified in terms of purely linguistic constraints (i.e. phonological, morphological, or syntactic rules). Yet, style variation is not really unspecifiable, once factors outside the language itself are taken into consideration. Thus style variation is like semantic differentiation in being functionally linked to extra-linguistic behavior.

It is somewhat surprising that in the aftermath of the Ferguson and Joos articles there has been almost no attempt made to investigate the relationship between diglossia and style variation, this especially in view of the fact that even a casual comparison of the two phenomena reveals striking functional similarities. Perhaps these similarities have been obscured in part by the fact that the structural relationships between the linguistic subsystems in a diglossia situation bear an overall resemblance to the structural relationships between geographical or social dialects in other situations. It is understandable how this could lead to an assumption that the rationale of diglossia should be sought within the realm of dialectology, rather than elsewhere. Yet, there are some important differences between diglossia and dialect variation. In situations involving different geographical or social dialects, each linguistic subsystem, or dialect, is in most cases used by its speakers to the exclusion of other dialects of the same type. That is to say, speakers of different geographical or social dialects do not normally have a command of each other’s linguistic systems. Dialect differences, far from being part of the productive linguistic repertoire of the members of the wider speech community, are historically imposed upon individuals by their geographical provenance or group membership.

In diglossia, on the other hand, the linguistic subsystems are like the style levels of a single language in that they are used together as interchangeable linguistic devices within the discourse of individuals throughout the wider speech community. Furthermore, as will be seen, the subsystems are linguistically differentiative in that the use of one or the other conveys speaker-intended meaning.

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3 A possible relationship between the two was suggested, though not elaborated along precisely these lines, for one kind of Arabic diglossia in the article by Haim Blanc, “Stylistic variations in spoken Arabic: a sample of interdialectal educated conversation” in: Charles A. Ferguson, ed., Contributions to Arabic Linguistics, Cambridge, Massachusetts, Harvard University Press, 1960.

4 It is possible for originally different geographical or social dialects to come to be used coterminously in a diglossia relationship. But when this happens, the speech forms cease to be geographical or social dialects as such, and become instead the potentially common property of all members of the speech community.
rather than information about the speaker's background, as is the case with dialects.

It seems obvious, then, that the relationship of diglossia to style deserves further study. It will be the purpose of this paper to contribute to that study by examining such a relationship in the functional distribution of Creole and French in the diglossia situation of Haiti.

The Republic of Haiti is located on the western third of the island it shares with the Dominican Republic. Haiti's predominantly rural population is rather large, considering the size of the country (10,714 square miles), being placed by the last official census at some 3,097,000 persons and presently estimated to have reached three and one-half million. As the former French colony of Saint-Domingue, Haiti was the center of an Afro-French cultural complex which coincided with French domination in the Caribbean and extended from what is now the State of Louisiana down to French Guiana in north-eastern South America. Like most of the other areas which were once part of this complex, Haiti has inherited two languages. One is a French-based Creole which, though normally unwritten, has continued to be an everyday language for virtually one-hundred percent of the population. The other, French, is the official and written language of the Republic. French, as spoken in Haiti, exhibits certain local characteristics when compared with, say, educated Parisian usage. As might be expected, many of these local characteristics are due to the influence of Creole. In its written form, however, there is little difference between the French of Haiti and that of the Continent. The number of French speakers in Haiti contrasts sharply with the number of Creole speakers, and although there are no figures available which show how many Haitians can speak French, the fact that it is the country's only written language allows one to take the approximate literacy rate of 12.5% for those over ten years of age as a rough indication of some formal exposure to French (although the number of persons who are really fluent in the language must be well below this). Of course, nearly all French speakers in Haiti are native speakers of Creole as well, and hence are bilingual.

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All-important in the description of any diglossia situation is a statement of the structural relationships which the linguistic subsystems have to each other. Perhaps the best place to begin such a statement of the structural relationships of Creole and French is with the phonological inventories which serve as their fundamental building blocks.

When one compares the phonologies of the two most mutually remote varieties of Creole and French in Haiti, namely the rural Creole (= RC) of a monolingual Haitian peasant with the Haitian French (= HF) of an educated urbanite, one finds surprisingly little difference in the basic phoneme inventories of the two languages. The greatest divergence is in the vowel systems, illustrated as follows:

<table>
<thead>
<tr>
<th>Rural Creole</th>
<th>ORAL</th>
<th>Haitian French</th>
</tr>
</thead>
<tbody>
<tr>
<td>i</td>
<td>u</td>
<td>i y u</td>
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<tr>
<td>e</td>
<td>o</td>
<td>e e o</td>
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<tr>
<td>e</td>
<td>a o</td>
<td>e a o</td>
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<tr>
<td>ê</td>
<td>ô</td>
<td>ê ẽ ô</td>
</tr>
<tr>
<td>å</td>
<td></td>
<td>å</td>
</tr>
</tbody>
</table>

Thus HF has the rounded front vowels /y e ǔ/ contrasting with the unrounded front vowels /i e ê/, while RC has only the latter set, e.g. HF /dyr/ dur ‘hard’ vs. /dir/ dire ‘to say’, both of which are /di/ in RC.

In the consonants, HF has a rounded front glide /u/ contrasting with the rounded back glide /w/, where RC has only the latter, e.g. HF /qit/ huit ‘eight’ vs. /wi/ oui ‘yes’, corresponding to RC /wit/ and /wi/ respectively. Otherwise, the main difference between RC and HF consonants lies, not in the inventories themselves, but rather in the combinatorial possibilities of their elements. Thus /r/ occurs post-vocalically in HF, while in RC it does not, e.g. HF /port/ porte ‘door’, RC /pot/. Also, HF can have final consonant clusters of the type /Cl/, /Cr/, and /st/, whereas RC has simply /C/ and /s/, e.g. HF /tabl/ table ‘table’, /livr/ livre ‘book’, /turist/ tourist ‘tourist’, beside RO /tab/, /liv/, and /turis/. Finally, it is worth noting that the allophonic conventions holding for like phonemes in
RO and HF are practically identical.\(^6\)

In comparing the morphological characteristics of RC and HF, one must distinguish between grammatical morphemes, which are more properly dealt with as part of the grammatical systems, and other lexical items. Haitian Creole is referred to as a French-based Creole because its lexicon is obviously and overwhelmingly French-derived. There are, however, certain lexical differences to be found between RC and HF. Generally speaking, these are of three main types:

(1) Differences in the phonemic shape of lexical cognates, due to divergencies in the two phonological systems (exemplified above).

(2) Differences in the phonemic shape of lexical cognates, due to other (principally historical) causes, e.g. RC /šej/ ‘chair’, /žâmi/ ‘friend’, /blie/ ‘to forget’; HF /šez/ chaise, /ami/ ami(e), /UBLie/ oublier.

(3) Differences where the direct semantic equivalents are non-cognate, e.g. RC /me/ ‘here (is)’, /kaj/ ‘house’, /gumē/ ‘to fight’, /kuljea/~/kunjea/ ‘now’; HF /wvasi/ voici, /mezō/ maison, /kôbatr/ combattre, /mētōnā/ maintenant.

Differences of type (1) are by far the most frequent, accounting for the vast majority of lexical divergencies in RC and HF. Type (2) differences are much less common, and type (3) differences are fairly rare. Even rarer are cases where lexical items which are structurally cognate in RC and HF have different meanings, cf. RC /neg/ ‘fellow’, /bagaj/ ‘thing’ beside HF /negr/ nègre ‘Negro’, /bagaz/ bagage ‘baggage’.

In grammar, the differences between RC and HF are more marked than in phonology or lexico-semantics. Without going into the extensive detail necessary for an accurate comparison of the two systems, it can be stated that among the more important grammatical differences are those of different arrangements of categories for

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\(^6\) This sub-phonemic behavior which is common to Haitian Creole and Haitian French is quite different from the sub-phonemic behavior of French anywhere on the Continent. In fact, it is largely through its allophones that Haitian French gets its characteristic sound. This sub-phonemic difference from Continental French is reinforced by a few actual phonemic differences as well. Thus HF normally has, like Creole, /ñm/, /ñn/, and /ññ/ word-finally where Continental French has /ñb/, /ñd/, and /ñg/, e.g. HF /žām/ jambe ‘leg’, /mɔn/ monde ‘world’, /lʌŋ/ langue ‘tongue’; Continental French /žāb/, /mɔd/, and /lɔg/.

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nouns, adjectives, pronouns, prepositions, and verbs in the two languages, as well as quite different syntactical structures. 7

The foregoing comparisons have shown that Haitian Creole, in its rural form, is structurally different from Haitian French at all linguistic levels—phonology, lexicon, and grammar. However, there also exists in Haiti another variety of Creole, which is spoken principally in the urban centers, largely (but not exclusively) by Haitians who know some French. Though generally ignored or discounted by linguistic descriptions so far, this urban Creole (= UC) is of practical importance because of the fact that it has much higher social prestige than RC, even in the opinion of RC speakers themselves. In addition, UC is of linguistic importance because it, and not RC, is the variety of Creole which is the normal co-participant with French in Haitian diglossia.

UC differs from RC principally in being structurally closer to HF in certain ways, so that many of the differences in form which distinguish RC from HF do not hold for UC. For example, the phonemes /y ɔ ɔ ɪ u/, absent in RC, occur in UC with much the same lexical distributions as in HF. The same is fairly true for post vocalic /r/ and, to some extent, for the final clusters /C/ /Cr/, and /st/ as well. Thus lexical differences from HF of type (1) above, so frequent in RC, are largely eliminated in UC. In addition, type (2) differences may also be eliminated in UC by the free use of HF forms beside the RC ones, e.g. /šez/ beside /šej/, /ami/ beside /zami/, etc. 8 Even the relatively rare lexical differences of type (3) are deemphasized by the general convention that, even where they are non-cognates, French equivalents may be substituted for Creole forms (but not vice-versa!).

Although phonological and lexical divergences from HF are thus generally minimized in UC, the same is not true with respect to the two grammars. Even in UC, accommodations to French grammatical patterns are slight. For the most part, they involve the use of certain prepositional constructions, e.g. UC /m vwaʒe dɔ portoprɛs a žakmɛl/ 'I travelled from Port-au-Prince to Jacmel', where RC would be /m vojaze potopres žakmel/, the use of the subordinator /kɔ/, e.g. UC /li di m kɔ l(i) ta fe sa pur mwɛ/ 'he told me that he would

7 For more details about the basic grammatical structure of Haitian Creole, the reader should consult Robert A. Hall, Jr., *Haitian Creole; Grammar, Texts, Vocabulary*, Menasha, Wisconsin, 1953 (= Memoir 74, American Anthropological Association and Memoir 43, American Folklore Society).

8 Sometimes the form in UC does not have the shape anticipated. Thus, RC /vле/ 'to want' has a UC equivalent /vía/, apparently as much on analogy with HF /və/ /veux as with the expected HF /vule/ /voulez.
do that for me’, for RC /li di m li ta fe sa pu mwé/, and gender accord for nouns and adjectives (signalled by morphological changes), e.g. UC /neg sa-a se aisjé/ ‘that fellow is a Haitian’ vs. /fám sa-a se aisjen/ ‘that woman is a Haitian’, where RC would have /aisjé/ in both cases. In most other ways, however, Creole grammar remains relatively little influenced by French.

The result of these similarities and differences is that UC and HF stand, in the linguistic makeup of the bilingual Haitian, in an interesting relationship to each other. The two linguistic systems are more or less fused at those points where they correlate with a commonly shared part of the real world—once at the phonological end, where language behavior is correlated with the organs of speech—again at the lexico-semantic end, where language behavior is correlated with its external referents. This relationship can be illustrated schematically as follows: 9

The two grammars remain distinct enough so that it is possible for the speaker to select at will one or the other, and possible for the hearer to tell at any given moment which grammar—and hence which language—is being used. 10

With the structural parameters of Creole and French outlined, it is now possible to move on to the central concern of this paper—

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9 I am indebted to Martin Joos for this diagram, which he suggested in the course of a private conversation about the diglossia situation, in Haiti.

10 In fact, one may occasionally hear such UC utterances as /mwa pa kône lui./ ‘I don’t know him, where all the constituent morphemes are perfectly French (the phrase could be written moi pas connais lui) but which is immediately recognized as Creole, simply because the construction is meaningful only in terms of the syntax of that language, cf. RC and more usual UC /m(wé) pa kône ni/ but French je ne le connais pas.
the functional distribution of the two languages within the Haitian bilingual community.

Certainly the most obvious kind of restriction on language use is that placed upon the speaker by immediate considerations of a purely linguistic or mechanical nature, such as the necessity to use Creole to a monolingual peasant, or French when addressing a foreigner or dictating correspondence. However, such cases account for only a small fraction of the decisions which, in the course of his daily activities, the bilingual Haitian must make as to which language to use. In most cases, his choice will depend largely upon non-linguistic factors, many of which relate to the immediate social context. Observation has shown that two main kinds of behavioral variables play an important role as the determiners of language usage in any social situation. These are:

- **Public** (impersonal or representative) behavior vs. **Private** (personal and non-representative) behavior
- **Formalized** (formally prescribed) behavior vs. **Unformalized** (not formally prescribed) behavior

The usual language distribution in terms of these variables is illustrated by the following chart, with the most frequently used language listed first in those cases where both can be used:

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<tr>
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<tbody>
<tr>
<td><strong>Public</strong></td>
<td></td>
</tr>
<tr>
<td>French</td>
<td>Creole (~French)</td>
</tr>
<tr>
<td>Private</td>
<td></td>
</tr>
<tr>
<td>French (~Creole)</td>
<td>Creole (~French)</td>
</tr>
</tbody>
</table>

It can be seen from the chart that French is the only appropriate language for public-formalized behavior, e.g. that involved in official ceremonies, legal and administrative procedures, formal education, and the like, while, at the other extreme, Creole is the usual language for private-unformalized behavior such as interaction between

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11 Another version of this diagram appears in William A. Stewart, *op. cit.*
friends or relatives, at informal gatherings or within the family. In between, French is the more common language for private-formalized behavior, e.g., introductory conversations between strangers, and Creole is somewhat more common for such public-unformalized behavior as advertising, general bartering, etc.

Except for public-formalized situations, the social context itself is only a partial determiner of language usage in Haitian diglossia. In most situations, one of the languages is more commonly used, but the other can also be heard. Part of this variation is undoubtedly due to personality differences among individual speakers; conservative minded Haitians will lean toward the greater use of French, while more relaxed and progressive Haitians will prefer to use Creole whenever possible. Yet this is clearly not the full answer, since often the same individual can be observed to alternate between languages in cases where the social context appears to remain constant. It would seem that language usage in Haitian diglossia is to some extent determined by conditions which are relatively independent both of speaker personality and the social context. But then the inevitable question is—what conditions?

When I first went to Haiti, I quickly noticed that bilingual Haitians would often shift from French to Creole (or vice versa) and back again right in the middle of a conversation, and sometimes even in the middle of a sentence. Although I had already caught on to the differences in linguistic behavior which were clearly conditioned by the social context, I continued for some time to regard this other kind of fluctuation in language use simply as erratic behavior, produced by the two linguistic systems in contact. Then one day, on a subsequent stay in the country, I was engaged in conversation with a Haitian friend with whom I had been on Creole speaking terms for some time. I happened to enquire about the health of his mother, who had only a short time previously been hospitalized because of an illness. I was surprised by the fact that my friend switched to French for his reply. Then it struck me that the changeover corresponded rather neatly to a shift which an English speaker would

12 For the use of Creole in the family, exceptions to the rule may be found in those cases where parents use only French with their children, so that the latter will get accustomed to using the language. But the parents usually continue to speak Creole to each other, as well as to friends and servants. The children, on their part learn Creole from servants and playmates anyway, and usually speak it among themselves when not in earshot of their parents. In effect, what happens within such families is that French becomes the special medium for parent-child communication, while Creole continues to be used for all other family purposes.
be likely to make under similar circumstances from a colloquial to a more formal style of speech, say, from Joos' casual to consultative. It began to appear that my friend had used the change from one language to the other as a stylistic device to indicate a change in the mood of his discourse.

Subsequent observations have tended overwhelmingly to support this hypothesis. In particular, most of the language shifts which I have observed in conversations involving bilingual Haitians have corresponded to cases where a style shift would also be appropriate in English. Take, for example, part of a telephone conversation in which one Haitian businessman was overheard speaking (primarily in French) to another: "Je comprends très bien pourquoi vous demandez l'argent d'avance, /wap proteze tet w/". Compare a somewhat free English equivalent: "I certainly understand why you're asking for the money in advance—you've got to (= /gat+tə/ or /gata/) look out for yourself, I guess." Note the consultative style of the sequence before the break, marked by the use of certainly, among other things, followed by a shift into casual, marked by the pronunciation of got to and the softener, I guess. Both the language shift in the Haitian conversation and the style shift in its English equivalent are devices used by the speaker to communicate information to the addressee—in this case, a message of voluntary identification and understanding.

If correct, this hypothesis about the nature of diglossia in Haiti has several implications. I will mention just two here—one for assumptions which are often made about Haitian French, and another for the theory of diglossia itself.

In the first case, foreigners who have visited or resided in Haiti have often remarked that Haitian French seems to them to be stuffy and bookish, and lacking the relaxed, fluid quality which it often has when used by Frenchmen. The foreigners tend to assume from this that French is not well spoken in Haiti. Haitians, on their part, have noticed this characteristic of their French, too. However, they explain it differently—saying that French usage in Haiti is simply more "pure" than in France. What is probably the case, however, is that Haitian French tends to lack equivalents for Joos' casual and intimate styles, since where a Frenchman would use his French counterparts for these styles, Haitians normally switch to Creole. This characteristic of HF is matched by a converse one in UC. I have noticed that the Creole of bilingual Haitians seems to lack the spe-
cial kinds of styles which monolingual Creole speakers use in certain formalized activities, such as the recitation of folk tales, religious ceremonies, etc. This is probably because in his own formalized situations the bilingual Haitian will not normally use Creole at all—he will use French. 13

As regards the theory of diglossia, a situation has been described in this paper in which the two related linguistic systems function, for bilinguals, as stylistic variants of each other. Eventually, if the term *diglossia* is not to degenerate into just a somewhat fancy synonym for bilingualism, it will be necessary for linguists to formalize a little more definitely the ways in which diglossia differs from other kinds of bilingualism. The diglossia situation in Haiti suggests one possible difference. If subsequent examinations of diglossia situations elsewhere reveal style function as a similar factor, it may well serve as a central criterion for the general definition of diglossia as a unique sociolinguistic phenomenon.

13 In RG, a style with many of the characteristics of Joos' frozen style has been developed for newspapers, educational broadsides, pedagogical materials, religious tracts, etc., written in Creole. This style of Creole is not really identical with any spoken form of the language.
DISCUSSION

MARK HANNA WATKINS: (Howard University) Bilingualism involves the speaking of two different languages. Diglossia, on the other hand, refers to different varieties of the same language. If this is so, then how do you classify Haitian Creole? I seem to remember that Robert A. Hall, Jr., once classified Haitian Creole as one of the Romance languages, and not as a dialect of French.

MR. STEWART: I can best answer your question by taking up the linguistic classification of Haitian Creole first and then returning to the matter of bilingualism and diglossia. Hall's classification of Haitian Creole as a member of the Romance family was originally explained in his article "The Genetic Relationships of Haitian Creole" where he based his assignment on what he claimed were phonological, morphological, syntactical, and lexical correspondences. Now, although such an assignment could be justified by a comparison of phonological and morpho-lexical correspondences, Hall must have been highly selective in the kinds of syntactical features he considered relevant for his comparisons, since the fact is, that the overall shape of Haitian Creole syntax is dissimilar to anything Romance—except possibly other Creoles which are lexically Romance-derived. Assuming that we want to avoid the arbitrary course of holding that only phonological and lexical correspondences are relevant for ascertaining genetic relationships between languages, it seems necessary for us to consider the evidence offered by syntactical structures as well. Furthermore, there may be cases in which a language may have a dual genetic relationship, wherein a certain part of the language's structure may derive from one source, while another part derives from a different source entirely. Haitian Creole appears to be a language of this type, with its grammatical structure having more in common with other Caribbean Creoles than with Romance languages, and probably deriving ultimately from a West African source. Evidence for this has been discussed in various places by Douglas Taylor, but particularly in his article "Language Shift or Changing Relationship?". Thus, in its grammar, Haitian Creole is clearly a distinct language from French. On the other hand, Haitian Creole is obviously closely related to French in its phonology and vocabulary—all the more so if one takes urban Creole and Haitian French for

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the comparison. It is this particular kind of relationship between the two languages which characterizes the diglossia situation in Haiti, and perhaps in other areas as well. And this brings me back to the first part of your question.

The difference you mention between bilingualism and diglossia appears to be the original one made by Ferguson in his "Diglossia" article. Yet here I would press for a modification of that definition of diglossia, since the Haitian example shows that a diglossia relationship can hold just as well between different languages as between different varieties of a single language, provided that the different languages are similar enough in certain ways to be able to interact somewhat like different varieties of a single language do.

**JACOB ORNSTEIN:** (Department of Agriculture) What was the follow-up on the study that Robert A. Hall, Jr. made of Haitian Creole, and on his recommendations that the use of Creole be extended for official purposes?

**MR. STEWART:** Hall's study of Haitian Creole was, I believe, started independently by him, but eventually became part of the UNESCO Pilot Project in Haiti. It was in connection with this project that Hall spent part of the year 1949 studying the language situation in Haiti itself. The scholarly outcome of this study was a linguistic sketch of the language: his book, *Haitian Creole.* The practical outcome included certain recommendations for the implementation of Creole as a formally recognized medium of communication and instruction, and particularly for its use in primary education.

Creole enjoyed a certain amount of use for these purposes during the late 1940's and early 1950's, but its popularity in official circles was short lived and the language has long since reverted to its old status as a primarily spoken and informal medium of communication. The failure of Creole to become permanently established in Haiti as a formally recognized and written language was probably due to many causes. One of the most obvious of these was that the supporters of the extended use of Creole were simply unable to overcome the denigrating mythology which surrounds Creole in Haiti, matched as it is by the impractical national role envisioned for French by most Haitian educators and administrators. Perhaps the lack of sufficient financial resources and qualified technicians also

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played a part. In addition, I can't help feeling that, in their own way, the supporters of Creole themselves contributed inadvertently to that failure.

First, there is the matter of the variety of Haitian Creole chosen for standardization. This was a stylistic modification of almost straight rural Creole, with its vocabulary enriched with phonemically (and orthographically) altered French loans. The choice of rural Creole was undoubtedly sound to the extent that the primary purpose intended for standardized Creole was as a tool of rural education. However, the universally low prestige of rural Creole compared with urban Creole was apparently overlooked, as was the fact that the Creole movement, if it were to succeed in the countryside, would have to receive support in the urban administrative centers first.

Secondly, there was the matter of orthography. The most widely used orthography for Creole was that devised by McConnell and Laubach, which tended to please linguistics by being near-phonemic, as orthographies go. However, the McConnell-Laubach orthography made some rather marked departures from French orthographic conventions in many places where it seems to me that departures were unnecessary, especially in view of urban Creole usage. Creole is, of course, a distinct language from French, and, theoretically at least, has every right to its own system of spelling. However, there is the practical question of whether, in an area where Creole and French are so closely associated (and indeed are even fused together in some ways), users of the two languages can be expected to keep proper track of the places where the French and McConnell-Laubach orthographic systems are similar and where they are different. In any case, Haitians certainly have not done so with ease.

Finally, we must consider the way in which the standardization of Creole was effected. Little was done to formalize the language beyond the development of an orthography. Yet, a spelling system, no matter how good, is simply not enough to give a language the social prestige necessary to encourage its formal use, especially in a culture where people feel that a language is only a language when it has, not only an official spelling, but an official "Academy" grammar and vocabulary as well.

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INTRODUCTION

CHARLES A. FERGUSON

Center for Applied Linguistics

In the article on Diglossia I attempted to describe a certain kind of language situation. This description or, let us say, the article as a whole, was a result of my interest in describing the language situation, or more accurately, the socio-linguistic situation of the Arabic speaking world. I felt that interesting though the situation was in the Arabic speech community, it was not in all respects unique. That is, in many ways this language situation was similar to the situation in other speech communities. What I tried to do was to characterize this situation in the hopes that here we would have discovered one possible element in a general typology of socio-linguistic situations. I mentioned about seven examples of diglossia and used four of them as the defining languages or the defining speech communities. These four were: Arabic, Modern Greek, Swiss German, and Haitian Creole. I tried to find the common features in the linguistic situations in these four speech communities, and after a fairly careful examination of this, I reached a definition which we will assume today as the topic under discussion. It reads like this: “Diglossia is a relatively stable language situation, in which in addition to the primary dialect of the language, which may include a standard or regional standards, there is a very divergent, highly codified, often grammatically more complex, superposed variety, the vehicle of a large and respected body of written literature, heir of an earlier period or in another speech community, which is learned largely by formal education and is used for most written and formal spoken purposes, but is not used by any sector of the community for ordinary conversation.” That is a fairly long and involved kind of definition. What it really did was to list the characteristics which the four defining speech communities seemed to have in common. On our panel today we will listen to specialists in each of the four languages that were used as defining languages in that original article.

2 Ferguson. op. cit., p. 336.
In this discussion, I should like to outline the special problems involved in the teaching of languages with diglossia, as distinct from the general problems involved in all language teaching. The discussion will be limited to the kind of teaching which has as its aim enabling native Americans to understand, speak, read, and write a modern language in a manner approximating that of the educated native speaker of the language. The approach to language teaching which will be assumed for the discussion is roughly the so-called audio-lingual approach, which is currently accepted by many specialists as one of the best approaches to language teaching in terms of current educational aims and resources in this country. It may be worth while to summarize first some of the general assumptions of this approach. It is assumed that, other things being equal, the learning of a modern foreign language is more effectively accomplished if:

1. the learner concentrates first on understanding and speaking and later on reading and writing;

2. the learner has as a model a native or near-native speaker of the language he is studying;

3. the basic phonology and grammatical patterns of the language are learned to a great extent through extensive, carefully-planned drills intended to develop automatic responses similar to those of the native speaker;

4. the instruction is planned and supervised by someone with sound orientation in linguistics and is carried out with the use of materials prepared on the basis of sound linguistic analysis;

5. the learning is on an intensive basis, that is, involves at least ten contact hours a week with a model to be imitated;

6. audio-visual materials are employed to the extent feasible as an integral part of the course work, whether in the classroom or outside.
The acceptance of these assumptions eliminates or radically simplifies the discussion of many problems of procedure and content in a course of instruction in a modern language with diglossia. Some of the problems which remain, however, are serious and deserve careful examination.

1. Learning two languages in one

One point must be made at the outset. The problem of teaching a language with two major forms cannot be solved by teaching only one of the forms. I realize that there are teachers of these languages who feel that the only satisfactory solution is just this, and limit the aims of their courses to the mastery of just the H variety or just the L variety. It is no doubt possible that this solution is adequate for certain individuals who are studying the languages for certain limited purposes, but it is clear that this solution will not meet the needs of someone who wants to learn to understand, speak, read, and write these languages in a manner approximating that of the educated native speaker. The teacher and student alike must face the fact that there is more to be learned than one language; perhaps it is not as much as two full languages, but it is certainly more than is generally attempted in a single language course. All apart from considerations of the content and procedure of courses, it seems clear that more time will be required to achieve results comparable to those obtained in other language courses. An American college student or government official who undertakes a program of study of Arabic or modern Greek must be prepared to learn double sets of forms and vocabulary items for most of the language, as well as a whole set of skills involved in selection of the appropriate variety for a given context.

Three problems are immediately apparent for the one who is planning the language curriculum:

What is the relative emphasis to be accorded the two major varieties of the language?
In what order should the two varieties be studied?
How can skill in one variety be maintained when the learning is concentrated on the other variety?

Since about 1940, modern Arabic and modern Greek have been taught intensively along the lines of the audio-lingual approach at a considerable number of universities, government agencies, and pri-

1 H—High variety. L—Low variety.
vate organizations in the United States. Haitian Creole has been taught very rarely in this way and to my knowledge there has been no intensive teaching of Swiss German in the United States. During these two decades of teaching, many shades of relative emphasis have been tried for the two varieties. It is probably correct to say that the trend in intensive Arabic teaching has been from courses where the emphasis was over 75% on the L variety, towards courses with exactly the opposite emphasis. In the teaching of Greek, the trend is less apparent, but it seems, if anything, to be mildly towards increased emphasis on the L variety.

In view of the heavy emphasis at early stages on understanding and speaking, it was only natural that most intensive courses in the 40's and 50's began with colloquial Arabic. Recently, some intensive or semi-intensive courses have attempted to begin with the H variety, making such oral use of H as the ability and inclination of the instructor would permit. One factor in the choice of order is the highly debated question of whether the transition and carry-over of knowledge is easier H to L or L to H.

Whether one begins with H or L, there is a serious problem in maintaining the skill acquired during the first part of the course while concentrating on a different set of skills in the second part. Students who have learned to converse fluently in some variety of spoken Arabic and have gone on to study the classical, often in a year's time lose their ability to converse. On the other hand, as people who have started with Classical Arabic acquire proficiency in the spoken language, they develop a tendency towards errors in the written language. No Arabic program with which I am familiar has solved this problem satisfactorily and only a very few of them have attempted any systematic solution.

It would be premature at this point to suggest promising ways of coping with these three problems, but at least one observation can be made. The nature of the problems is different in instances of diglossia such as Swiss German and Haitian Creole, where H is a standard language used as a medium of ordinary conversation in another speech community, and the instances of diglossia such as Arabic or Greek where this is not the case.

2. **Dialect problems**

Strictly speaking, the question of which dialect or dialects should be chosen for instruction in L is not a problem directly con-
nected with diglossia. If there is a single standard variety of L (as in Haitian Creole and Greek), this is obviously the one to be chosen, but if there is no standard L the situation is parallel to a language without diglossia which has no standard form. In this case the one who is planning the course must decide on the variety of spoken language to be taught.

Often in the past the decision has been taken on the basis of which dialect has the best instructional materials or has native speakers most readily available for models or best prepares a student for work in a particular region or country. While these are all valid considerations, they probably should not be decisive in any general curriculum planning for language instruction. Additional criteria which may be suggested for dialect choice are: relative number of speakers; degree of intelligibility throughout the entire speech community; ease of transition to H. As an example of a possible solution to this problem for Arabic, I would like to repeat a suggestion originally made in 1951.

The solution suggested here for most American students of Arabic is to concentrate on learning well the ordinary conversational Arabic of educated people of any of the important urban centers of the Arabic-speaking world. The four such dialects which are to be recommended because of the number of speakers, probable future importance, and availability of teaching materials and native speakers, are:

Cairo ("Egyptian Arabic"), Baghdad ("Iraqi"), Damascus-Beirut-Jerusalem ("Syrian"), Rabat-Salé-Fes-Meknes ("North Moroccan"). Specific recommendations would be:

a. For a particular company, project, or government agency: Where possible teach the most appropriate local dialect.

b. For universities, institutes, etc. offering a program of Middle East studies: choose Egyptian, Iraqi or Syrian Arabic and teach it regularly or, if facilities permit, alternate years among the three.

c. For universities, institutes, etc. offering a program of North African studies: teach North Moroccan.

One question not mentioned in the suggestion just quoted is the need for facilitating the student's adaptation to a local dialect when he goes to a part of the Arab world where the people do not
speak the kind of Arabic he has studied. This problem exists, of course, for native speakers of Arabic, but for them it is much less serious because of the far greater language resources at their command. It would seem that any responsible course of Arabic instruction at the college or university level should offer sufficient information on the nature and range of dialect variation, in particular lexical differences, to enable the student to make an adequate adjustment to a new dialect area within a matter of weeks, assuming that he has a solid basis in one particular variety of L.

3. Intermediate forms of language

Up to this point, we have not mentioned the existence of mixed forms of language intermediate between H and L which are used in certain kinds of situations. These actually constitute the most interesting problem from the point of view of those concerned with the theory of language learning and with general principles of methodology.

Most language teaching is based on the assumption of a single set of relatively stable forms and lexical items in the language being learned. Every language probably has alternative forms or constructions as well as synonyms when the choice depends on style, context, speed of utterance, and the like. But these alternates generally constitute a very small part of the whole language. The mixed varieties in a case of diglossia, on the other hand, involve exactly this kind of choice on a large scale. The native speakers mix elements from H and L in a highly variable way. In a semi-formal discussion a speaker may use the H word for "man" in one sentence and the L word in the next; he may use an H stem with an L grammatical ending (e.g. *ra?eto "I saw him" = *ra?aytuhu crossed with *sufto). How does one teach a student to produce such mixed utterances? At a meeting on the teaching of modern standard Arabic (H) held at Harvard in the summer of 1958, it was agreed that the first step in answering this question would be the gathering of reliable data on intermediate varieties. One such study has appeared, but this is not yet enough on which to base the new materials and techniques required. The descriptive work is a job for the linguists, the subsequent work really would include the cooperation of linguists, language teachers, and psychologists.

4. Suggested experimental programs?

It is very difficult to use experimental methods to determine the most effective procedures for teaching one of the "neglected"

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languages in the U. S. Not only does the experimenter have the usual problem of numerous completely interrelated variables which make the construction of satisfactory experimental designs almost impossible in the language field, he also has such small populations of subjects to deal with that "pure" controlled experimentation is out of the question. What can be tried, however, is a careful clinical approach limited to several major plans. What I am suggesting is that specialists should try to work out programs of study which seem very likely to produce desirable results, although with major differences in content and procedure. Then as these programs are put into effect in a number of institutions, those concerned with the planning and with the actual teaching should meet from time to time to talk over points which emerge in the courses. By the end of a four or five year period of such limited experimentation, it ought to be possible to agree on a fair number of matters which are currently judged by guesswork and bias.

Plan No. 1

One such plan would be to begin with H, using the kinds of procedures and materials which are appropriate to the audio-lingual approach. In the case of Haitian Creole and Swiss German this is relatively easy since material produced for regular instruction in French and German could be used with only minor supplementation. For Arabic and Greek there are no materials available for such a course. It would take at least two years of collaborative work to produce a core of instruction in H which would emphasize learning the oral use of the language insofar as this can be done given the limitations on the spoken use of H. This first stage of the plan would constitute the first year's work at a college course of 8-10 hours per week.

The second stage of the plan would be directed primarily to L, but with a continued heavy dose of H. Something like one-fourth of the student's time and effort should continue to be focused on H, with the material concentrating now on the reading and writing and integrated as much as possible with the L material being presented for oral skills. This might constitute the second year of a college program.

The third and last stage of elementary instruction in this plan would consist of a course about two-thirds of which is devoted to reading in H with accompanying oral and written drills and about

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one-third to L, this time being taken up largely with planned conversation and discussion designed to review the oral material of the second year and to give conversational skill on certain topics met primarily in the reading.

At the end of such a course one would hope that the student would be able to make extensive use of the language for documentary research, travel to the country, listening to the radio, and so on. He would be ready to begin the serious study of literature in the language.

Plan No. 2

Another plan would be to begin with L and shift to H. Since this plan has been tried in a number of places, some instructional materials exist which could be used. Programs of this kind have in the past generally suffered from one or the other of two defects—either the study of L was dropped too soon, before the student had really acquired the basic structure, or no systematic attempt was made to maintain competence in L after the shift. Some courses suffered from both these defects.

Here again it must be assumed that three years of college courses will constitute the minimum elementary program of instruction before the student is expected to make great use of the language or is encouraged to proceed to a study of literature or to advanced linguistic studies.

Plan No. 3

Of all the programs in Arabic instruction reported on in the U. S., only the Army Language School program has tried to present H and L simultaneously, or nearly so, with textbooks and recorded materials designed accordingly. This plan is well worth attention and its planners and teachers should participate in the discussion of the followers of the other plans. In this connection it must be pointed out that discussions and reports on courses are not sufficient—the instructors and planners must visit each other's programs often enough to get an adequate understanding of the problems and the results.

An Experiment

One final proposal: a group of specialists should design one or more experiments to throw light on the psychological and linguistic
problems involved in teaching mixed varieties of languages when the variation is embarrassingly random and current methods of teaching are clearly inappropriate.
DISCUSSION

PAUL GARVIN: (Thompson Ramo Wooldridge) It seems to be very useful to differentiate rather clearly in all these situations between function and structure. On the one hand, there seems to be a scale of stylistic functions, and we might say perhaps the end points are more or less universal; that is, in most cultures you would have as one end point 'intimate' and as the other end point 'frozen' or 'superformal' with as many subdivisions as can be observed, perhaps with a finite limit. This is one critical dimension. The other critical dimension is that of structure, that is, the particular different languages or different dialects that are used jointly by the speech community in question. And then you get into the problem of the relation between the two, and it seems to me, that apparently the same functional differentiation can be carried by structures which are linguistically more different as in the case of French Creole, or not very different from each other, as in the case of American English. Perhaps this would be one way of easing the burden of description.

MR. FERGUSON: I think that it would be fair to comment that the original definition of diglossia was based almost completely on factors outside pure linguistics. That is, they were social factors, or factors of function rather than structure. And I would stand by this approach to the problem. As soon as we try to define socio-linguistic situations in terms of linguistic structure, we find, just as Mr. Garvin said, that the same kind of structure can be used for different purposes in different speech communities, and vice versa. This gives me a chance to comment on some of the observations that were made before. First, whether diglossia is not really about the same as the standard language with dialect situation, which is found in many parts of the world. In my article, I specifically tried to show the difference between these two situations. It was this difference that really led me to discuss it at all. In communities with diglossia, as described here, the H language, (the 'standard' or 'higher' language) is not the normal conversational language of the speech community. In a standard language with dialect situation, such as Persian or Italian or Bengali, there may be speakers of dialect who then learn to speak standard language in addition, and do so on certain occasions, and sometimes the functions may be quite parallel to those described here for diglossia. But in each of those speech communities I just mentioned, that is, in the Persian speaking world, the Italian speaking world, the Bengali speaking world, there are sub-

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stantial numbers of people who carry on ordinary every day conversations in the standard language. On the other hand, no one speaks classical Arabic in ordinary conversation; and Swiss German speakers don't speak standard German in ordinary conversation. This is a major difference. As to the question whether diglossia could be parallel to bilingualism, or whether we should shift over in that direction, I explicitly mentioned in a footnote in my article, that this situation does occur with bilingualism. That is, where the two varieties that are being described are generally recognized as two different languages, better yet, two unrelated languages, there is no question where to draw the boundary line. The functional description is the same. Here the language structural difference enters, and I have the feeling that when someone some day works out a general typology of socio-linguistic situations, the linguistic structure will play a relatively unimportant role, and that those kinds of bilingualism as distinguished from other kinds would fit together with these diglossia situations. But I felt it was best to start from one fairly small group of situations which, as you can see, have much in common. And I would still insist that the four defining languages, or rather their speech communities, do share all the factors which were mentioned in the rather lengthy definition of diglossia with which we started this panel, even though each case has certain unique characteristics.

PAUL PIMSLEUR: (Ohio State University) It seems to me that the introduction here of Joos' notion of level of discourse is an extremely important one that serves as a very useful instrument for clarifying many things that linguists find difficult to talk about. I would like to propose, as an operational way of distinguishing between diglossia and bilingualism, that bilingualism exists in those situations in which the two languages in question both have relatively complete levels of discourse, whereas diglossia exists in the situation in which the burden is shared by certain levels in the one, and different levels in the other.

MR. FERGUSON: This has been suggested before, but it runs into several difficulties. One is the use of two different varieties outside this speech community, in some other speech community where they may have the full range of language available, although they do not in the speech community under discussion; another is that this would include in diglossia large numbers of language situations which are quite different in other respects, like for example, speakers of Syriac in Baghdad who do not have a complete language,
that is, they have one that is restricted to certain limited situations, because in a sense it is a dying language which is used for ever more limited purposes in the society. However, the occasions in which it is used, and the limitations, are startlingly different from the ones that we have described for languages discussed today, so that this would then have diglossia include a much larger number of sociolinguistic situations. It might well be, however, that this is still a valid classification to suggest.

**MR. PIMSLEUR:** If I may comment on the preceding statement just very briefly, I think that the term bilingualism, since it has the word 'language' in it, should only be applied to those cases in which the language is a language, that is, in which a sufficient number of levels of discourse exists so that it is still functioning as a language and evolving as a language. And I would suggest that what is needed is the lower end of the scale. If the lower end of the scale does not exist, then in fact it is not functioning as a language and therefore should not be used under the term bilingualism.

**MR. FERGUSON:** This raises a difficulty. What do we call 'Syriac' then? If it is not a language, or a dialect, or anything else, we have to invent another term to describe a language with limited use, or which is on its way out, or something similar.

**ERIC HAMP:** (University of Chicago) On the matter of bilingualism, it seems to me too, that we have to recognize that the natural world provides every conceivable gradation and sometimes we are going to have to clarify what we really mean by differences on this scale. In my own experience, for example, in Albanian enclaves in the south of Italy, we have people in the diglossia situation. We have separate things, which have, I suspect, the same transformational syntaxes, but with different phonologies, as between Italian and Albanian, and to a great extent different morphophonemics and nuclear morphologies. In the Balkans, in my experience, it is very wide-spread to find what appear to be the same transformational syntaxes spoken with identical phonologies, and in some cases almost a total carry-over of morphophonemics. Thus it becomes a little difficult to say where you have different morphologies or where you have merely just different shapes occupying the same syntactical slots, and everything else the same. Finally, I have found that same situation between Vannetais Breton and the local French that they speak there. In many cases, what has been conventionally called bilingual situations from the structural point of view, quite apart

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from function, may very well turn out to be somewhat varying situations of part-systems.

CHARLES BIDWELL: (University of Pittsburgh) To return to the pedagogical, I think perhaps one factor which might help in making a choice between your first or your second plan, namely whether to start with L or the H, would be: is one form of the diglossic language derivable from the other in great part by the application of rules for sound change, what we might call phonological transformations? It would be true, for example, of Slovenian that the colloquial spoken language could be derivable in large part, though not totally, from the written standard by the application of rules of sound change. But the reverse would not be true, because many phonemes are lost through these sound changes.

EINAR HAUGEN: (University of Wisconsin) I think this discussion has clarified a number of things and I was particularly glad to have it clearly brought out that the term diglossia was not strictly a linguistic, but a socio-linguistic term. This is very important, because otherwise we get involved with the whole problem of what is language, what is dialect, and how we distinguish different varieties within that infinite scale that Prof. Garvin mentioned. Perhaps one could define diglossia as those linguistic situations which create difficulties for teaching. It does not seem to me that the situation in Switzerland is entirely parallel, though it has points in common with the others. We have been accustomed to call such peoples bilingual, but we should distinguish between bilingual and bidialectal. I thought at first that perhaps the term 'diglossia' could be equated with bidialectal, but I see now that it cannot, because in some situations it would be impossible to refer to these idioms as dialects. In the case of French and Creole, some say that they are two dialects and some that they are two languages. The Greek situation may be susceptible to an interpretation by which one could say that there is one Greek language written in two different ways. When I say "Greek language" I mean it in the same sense as I might say there is a German language, that is, apart from the writing, one which is used among educated people who associate with one another and have no very marked local characteristics in their speech. As I understand it, Katharevusa words can be used in cultivated speech, and so can Demotic words. So perhaps we really have a one to two relation rather than the diglossic relation suggested.

As for the teaching problem, this is one that I have had to face too,—what kind of Norwegian to teach. The situation in Norwegian
is clearly diglossic, though I would worry about applying the terms ‘high’ and ‘low’. In the case of two accepted standard languages, which is ‘high’ and which is ‘low’? The people who use either one would resent the use of these terms. There is a difference also between teaching for production and teaching for reception. I think we should expose the students to all possible varieties, but not ask him to produce anything but one norm. This is where the intermediate form in Arabic might some day become the real norm. This would be the result of a development in the Arabic community and something that no one can impose on the Arabs. They have to work it out for themselves, as they no doubt will some day, with more communication. This point is one I was trying to make in my discussion of linguistic norm, that the norm of the foreign language is a useful thing to teach Americans who go abroad in order that they may sound like educated people. The situation when the Spoken Series was created was, to my mind, a totally anomalous one in this respect, because the manuals stated specifically that any native speaker could be used as a guide for this series, and that it did not matter who or what he was. No attempt was made to suggest that some native models would permit you to be accepted in the circles you would like to be accepted in, while others that you might learn from would not.
APPENDICES
Appendix I

PROGRAM OF THE THIRTEENTH ANNUAL
ROUND TABLE MEETING

REGISTRATION—Lobby of the Edmund A. Walsh Memorial Building, 8:30 a.m., April 6, 1962.

FIRST SESSION—Friday, April 6, 1962, 9:30 a.m.,
The Hall of Nations.

WELCOME REMARKS:

Reverend Frank L. Fadner, S.J.
Regent, Institute of Languages and Linguistics
Robert Lado
Dean, Institute of Languages and Linguistics
Elisabeth D. Woodworth
Chairman, Thirteenth Annual Round Table Meeting
Robert J. Di Pietro
Co-Chairman, Thirteenth Annual Round Table Meeting

PANEL: The Transformation Theory: Advantages and Disadvantages

Moderator: Eric P. Hamp (The University of Chicago)

1. Raimonde Dallaire (Massachusetts Institute of Technology)
2. William W. Gage (Center for Applied Linguistics)
3. Paul L. Garvin (Thompson Ramo Wooldridge, Inc., RW Division)
4. Paul M. Postal (Massachusetts Institute of Technology)

LUNCHEON MEETING—Friday, April 6, 1962, 1:15 p.m.
Faculty Lounge, New South Dormitory

Speaker: The Honorable John Brademas, Congressman from the Third District, State of Indiana.

*Foreign Languages in the National Interest
SECOND SESSION—Friday, April 6, 3:00 p.m.
The Hall of Nations

PANEL: Bilingualism

Chairman: Norman A. McQuown (The University of Chicago)

1. A. Richard Diebold, Jr. (Harvard University)
   *Code-Switching in Greek-English Bilingual Speech*

2. Einar Haugen (The University of Wisconsin)
   *Schizoglossia and the Linguistic Norm*

3. Reverend Coleman L. O'Huallachain, O.F.M. (Georgetown University)
   *Bilingualism in Education in Ireland*

4. Norman A. McQuown
   *Indian and Ladino Bilingualism: Sociocultural Contrasts in Chiapas, Mexico*

RECEPTION—Friday, April 6, 6:15 p.m.
Faculty Lounge, New South Dormitory
Host: Georgetown University

THIRD SESSION—Saturday, April 7, 1963, 10:00 a.m.
The Hall of Nations

PANEL: National Languages and Diglossia

Chairman: Charles A. Ferguson (Center for Applied Linguistics)

1. Fred W. Householder, Jr. (Indiana University)
   *Greek Diglossia*

2. William G. Moulton (Princeton University)
   *What Standard for Diglossia? The Case of German Switzerland*

3. William A. Stewart (Center for Applied Linguistics)
   *Functional Distribution of Creole French in Haiti*

4. Charles A. Ferguson
   *Problems of Teaching Languages with Diglossia*
CLOSING LUNCHEON—Saturday, April 7, 1962, 1:30 p.m.
Dupont Plaza Hotel

Speaker: John Simons (Assistant Chief, Division of University Relations, Peace Corps)

*Young America Speaks

*Editor's Note: Both MR. BRADEMAS' and MR. SIMONS' speeches have been omitted from this monograph because they were not received in written form before this volume went to press.
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MEMBERSHIP OF THE THIRTEENTH ANNUAL ROUND TABLE MEETING*

Abbey, Robert L.  
Acevez, Rev. Louis, S.J.  
Alatis, James E.  
Allee, John Gage  
Allen, Rolfe L.  
Alrayah, Mohiddin K.  
Ani, Moukhtar  
Aquino, Fred C.  
Atsal, Ulkü  
Badia-Margarit, A.M.  
Barritt, Westbrook  
Baydalakoff, Victor  
Bayerle, Gustav  
Berezhovsky, Helen C.  
Bidwell, Charles E.  
Binda, Jeffrey  
Binda, Margaret  
Bosco, Frederick J.  
Bostain, James C.  
Bostain, Mary  
Brazier, Rev. Henry  
Brockman, E. W.  
Brower, Helen B.  
Burton, Robert A.  
Bushey, Ruth L.  
Butcher, Clifton H.  
Caino, Domingo  
Canu, Jean  
Carter, Marion Elizabeth  
Castiglione, Salvatore  
Castro, Manuel C.  
Cetin, Sükrü  
Chafe, Wallace  
Chavchavadze, Helen  
Chew, John J., Jr.  
Clarity, Beverly E.  
Cole, Desmond T.  
Convers, Gilbert

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*Not all those who attended the Thirteenth Annual Round Table meeting signed the register. Consequently, the above list is not complete.
Cook, Harriet B.
Cordova, Gabriel, Jr.
Cortright, Richard W.
Cowell, M. W.
Cox, Maybelle D.
Dallaire, Raimonde
Dampoint, Gilbert B.
Desberg, Dan
Di Gregorio, Vincenzo
Di Pietro, Robert J.
Dinneen, Rev. F. P., S.J.
Dindorf, Fr. Meinard, O.S.B.
Doye, H. G.
Duic, Antonya
Echeverei, Gabriel
Eddy, Frederick D.
Epée, Étienne V.
Eppink, Alice J.
Erwin, Bernarda
Erwin, Wallace M.
Espí nosa, Alfonso
Fadner, Rev. Frank L.
Fargo, Nancy Lou
Ferguson, Charles A.
Fleming, Jeanne
Flinton, Sister Margaret
Fox, Robert P.
Gaarder, A. Bruce
Gabbert, Nancy
Gage, William W.
Gandoin, Evelyn
Garvey, Catherine
Garvin, Paul
Goldin, Mark G.
Goodwyn, Frank
Greene, Harry
Giraldo, Brother Marcarip
Guss, Allene
Hall, Edward T.
Hardy, Suzanne
Harrell, Richard S.
Hashimoto, Mitsuo
Haugen, Einar
Hayes, Alfred S.
Hellwig, Sister M. Cuthbert, SCMM
Wewitt, Earl R.
Higgins, William L.
Hildurn, Donald C.
Hiten, Edith A.
Hodge, Carleton T.

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Hoffman, Reinhold
Hohlfeld, A Maurice
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Horn, Stefan
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Lang, Anton
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Lehr, Marianne
Lobo, Rev. Felix, S.J.
Louvet, Genevieve
Luongo, Alfred A.
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Manley, T. M.
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Masica, Colin
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McReed, Larry
Meenakshi, T. P.
Mikus, Renée M.
Mills, Marjorie

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