Empirical Foundations
for a
Theory of Language Change

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Uriel Weinreich died on March 30, 1967. Those who knew him, friends and colleagues in many fields of research, find it difficult to contain their grief. He was not yet forty-one years old. In the last weeks of his life he devoted his major effort to the final revision of this paper, and worked actively on it until two days before his death.

This paper emerged when, after several years of research and discussion on problems dealing with language change, the three authors felt it opportune to attempt a joint formulation of certain ideas on which their thinking had been converging. It was Weinreich who prepared the original draft incorporating appropriate materials submitted to him by the second- and third-named authors. He was, at the time, an NSF Senior Postdoctoral Fellow at the Center for Advanced Studies in the Behavioral Sciences; the first draft, for presentation to the Symposium in April, 1966, was produced across a geographic distance, and under a schedule which ruled out the possibility of full discussion. Thereafter, some conclusions remained to be hammered into more mutually agreeable form. This process of revision began after Weinreich's return to New York in the fall of 1966, and proceeded actively despite his illness.

Weinreich's personal editing of the final draft comes to an end with Section 2.4. The final formulation of the remainder, from 2.41 on, is the work of the second-named author. The third section of the paper was sketched out only lightly in the draft presented at the Texas conference. Though many of Weinreich's formulations and evaluations appear here, and the overall framework is a product of our joint thinking in the early months of 1967, there are undoubtedly many details

which would have taken a different form if he had shared in the final editing.

Whatever revisions have been introduced, the basic orientation of the paper remains unchanged. It thus largely reflects Weinreich's conception. The historical perspective, especially the sections on Paul and Saussure, are exclusively his. The Introduction is also Weinreich's work: it emerged after our frequent meetings during the last few weeks of his life. In this final version, after many revisions, Weinreich fused the several themes of the paper into a single statement. His coauthors are honored that he deliberately chose this means of preparing a final statement of his views on the structure of language and the nature of linguistic change.

0. INTRODUCTION

The present paper[1] is based on the observation that structural theories of language, so fruitful in synchronic investigation, have saddled historical linguistics with a cluster of paradoxes which have not been fully overcome. Ferdinand de Saussure, in laying the foundations of synchronic study, was aware of the corresponding intractability of language change, and was apparently resigned to it. But with the majority of linguists after Saussure, the choice between studying either the structure or the history of languages did not sit well. It would not be unfair to say that the bulk of theoretical writing in historical linguistics of the past few decades has been an effort to span the Saussurean dilemma, to elaborate a discipline which would be structural and historical at the same time.

We would like here to depict the origins of the structure-history antinomy in Neogrammarian theory; we will dwell particularly on Hermann Paul, who apparently was the first to isolate the language of the individual as the most legitimate object of linguistic study. We

will trace the hardening of the paradox in the Saussurean period, when homogeneity of language—assumed to be found in the idiolect—was drawn upon as a prerequisite for analysis. We will show the fresh opportunities for explaining language change that came with the efflorescence of linguistic description after World War II, and comment also on the limitations that developed in viewing language states as determinants of their own further development. We will review a number of attempts that were made to see the language of a community as a differentiated system and to reconcile the observed facts of linguistic heterogeneity with the theoretical desiderata of finding order and structure. We will, finally, suggest that a model of language which accommodates the facts of variable usage and its social and stylistic determinants not only leads to more adequate descriptions of linguistic competence, but also naturally yields a theory of language change that bypasses the fruitless paradoxes with which historical linguistics has been struggling for over half a century.

In referring to *theory* in the title of the paper, we have been conscious of the new connotation which this term has acquired in the discourse of linguists of the past decade. When Chomsky in 1957 proposed to view the grammar of a language as (1) a *theory* of its sentences, and linguistics as (2) a *theory* of correct grammars, he gave a new seriousness to linguistic investigation and reached out for a fresh alliance between linguistics and the exact sciences. To be sure, Chomsky's second use of *theory* has turned out to be more utopian than it seemed originally. But the first application of the term has already brought about such significant advances that it is worth considering the bearing which this strong sense of theory may have on language change.

A "theory" of language change in the rigorous sense can be visualized in a relatively strong form and in a weak form. In its strong form, the theory would predict, from a description of a language state at some moment in time, the course of development which that language would undergo within a specified interval. Few practicing historians of language would be rash enough to claim that such a theory is possible.[2] In a more modest version, a theory of language change would

[2] Coseriu (1958), in his monograph on structuralist theories of language change and their philosophical foundations, distinguishes between the "ra-

merely assert that every language constantly undergoes alteration, and it would formulate constraints on the transition from one state of a language to an immediately succeeding state. It might predict further that no language will assume a form in violation of such formal principles as are postulated to be universal in human languages. Without predicting positively what will happen (except that the language will somehow change), such a theory would at least assert that some changes will *not* take place.

Our own view is that neither the strong nor the modest version of such theories of language change, as they proceed from current generative grammar, will have much relevance to the study of language history. We will argue that the generative model for the description of language as a homogeneous object (see §2.1) is itself needlessly unrealistic and represents a backward step from structural theories capable of accommodating the facts of orderly heterogeneity. It seems to us quite pointless to construct a theory of change which accepts as its input unnecessarily idealized and counterfactual descriptions of language states. Long before predictive theories of language change can be attempted, it will be necessary to learn to see language—whether from a diachronic *or* a synchronic vantage—as an object possessing orderly heterogeneity.

The facts of heterogeneity have not so far jibed well with the structural approach to language. We will see the seeds of this conflict in Saussure (§1.21) and its deepening in the works of descriptivists struggling with the phenomena of change. For the more linguists became impressed with the existence of structure of language, and the more they bolstered this observation with deductive arguments about the functional advantages of structure, the more mysterious became the transition of a language from state to state. After all, if a language has to be structured in order to function efficiently, how do people continue to talk while the language changes, that is, while it passes through periods of lessened systematicity? Alternatively, if overriding

tional" problem of why languages change of necessity, the "general" problem of conditions under which particular changes usually appear in languages, and the "historical" problem of accounting for concrete changes that have taken place. He finds linguistics widely plagued by the mistake of confusing the three levels of the question (p. 37).

pressures do force a language to change, and if communication is less efficient in the interim (as would deductively follow from the theory), why have such inefficiencies not been observed in practice?[a]

This, it seems to us, is the fundamental question with which a theory of language change must cope. The solution, we will argue, lies in the direction of breaking down the identification of structuredness with homogeneity. The key to a rational conception of language change —indeed, of language itself—is the possibility of describing orderly differentiation in a language serving a community. We will argue that nativelike command of heterogeneous structures is not a matter of multidialectalism or "mere" performance, but is part of unilingual linguistic competence. One of the corollaries of our approach is that in a language serving a complex (i.e., real) community, it is *absence* of structured heterogeneity that would be dysfunctional.

The problem of constraints on immediately succeeding language states, to which we alluded above, is in our view subsumed under the broader theoretical question. Of course, we too want to inquire into the set of possible changes and possible conditions for changes which can take place in a structure of a given type. Nor do we want to dismiss the *transition* problem: it remains entirely relevant to ask about intervening stages which can be observed, or which must be posited, between any two forms of a language defined for a language community at different times. But if the theory is to be illuminating with respect to recorded histories of languages, we must ask two further questions: How are the observed changes *embedded* in the matrix of linguistic and extralinguistic concomitants of the forms in question? (That is, what other changes are associated with the given changes in a manner that cannot be attributed to chance?) And how can the observed changes be *evaluated*—in terms of their effects upon linguistic structure, upon communicative efficiency (as related, e.g., to functional load), and on the wide range of nonrepresentational factors involved in speaking?

We will refer to these four questions and their associated problems

[a] We are aware, of course, of Gilliéronian examples of "pathology" in language, which have been adopted by Martinet as explanations of push-chains in phonology (1955:54 ff. and *passim*). However, we will try to show below that only a small fraction of well-documented language changes seems at present explicable by such causality.

as those of *constraints, transition, embedding,* and *evaluation.*[4] Evidently the problems are partially ordered: a solution to the constraints question provides a set of changes within which the other questions can be put. In the light of answers to these, we can approach a fifth question, perhaps the most basic: What factors can account for the actuation of changes? Why do changes in a structural feature take place in a particular language at a given time, but not in other languages with the same feature, or in the same language at other times? This *actuation problem* can be regarded as the very heart of the matter. It is thus apparent that we want a theory of language change to deal with nothing less than the manner in which the linguistic structure of a complex community is transformed in the course of time so that, in some sense, both the language and the community remain the same, but the language acquires a different form.[5]

We will not be presenting a fully worked-out theory of linguistic change in this paper; it is doubtful whether any linguist has enough relevant facts at his disposal to attempt anything so ambitious, and we are not sure that with the facts available to us, the three coauthors would agree on the detailed outlines of such a theory. But, as our title suggests, we do feel in a position to make concrete proposals concerning the *empirical foundations* for a theory of change. By this we mean (1) the empirical findings which have significance for the theory, which the theory must account for, and which indicate directions for fruitful research; (2) certain conclusions drawn from these findings as to the minimal complexity of linguistic structure and domains for

[4] The transition, embedding, and evaluation problems were discussed by Labov (1965) under the heading of *mechanism* of a change. However, it seems difficult to give a precise meaning to the term *mechanism of a change,* and here we do not distinguish between explanation of a change and the analysis of the mechanism itself.

[5] The community has also changed, of course: it will be important for the purposes of this paper to note that the structure of the community, as well as the individuals who fill various functional slots in the structure, will normally show changes. In many cases, then, it will be difficult to establish that the community and the language are the same as they were, since both are changing; the changes may be so rapid that it is not easy to assert that the new members are the simultaneous inheritors of the same language and the same community. The major empirical studies referred to in this paper deal with changes so rapid that it is impossible to trace the gradual transformation of one form into the other.

defining such structure; and (3) methods for relating the concepts and statements of a theory to empirical evidence—that is, evidence based on rules for intersubjective agreement among investigators. We feel it important to dwell explicitly on empirical foundations, in view of the conscious or unconscious disregard of empirical principles which pervades some of the most influential work in linguistics today. We will, in what follows, try to document and account for this state of affairs.

We think of a theory of language change as part of a larger theoretical inquiry into linguistic evolution as a whole. A theory of linguistic evolution would have to show how forms of communication characteristic of other biological genera evolved (with whatever mutations) into a proto-language distinctively human, and then into languages with the structures and complexity of the speech forms we observe today. It would have to indicate how present-day languages evolved from the earliest attested (or inferred) forms for which we have evidence; and finally it would determine if the present course of linguistic evolution is following the same direction, and is governed by the same factors, as those which have operated in the past.[6]

It is the third general area of investigation which is the focus of the present paper: the description and explanation of linguistic change over the past four or five millennia. But even this limited area would be too large for a theory of change today. We might consider different temporal ranges separately: long-term changes with similar effects over millennia; completed changes which cover a century or two at the most; ongoing processes that can be observed in the course of one or two generations; or even purely synchronic sections in which we identify inferentially the directions of change of certain variable elements. In this discussion we will be concerned primarily with the second and the third of these ranges, although some comments will be made on the first problem and some data drawn from studies of the last.

[6] Investigations of the long-range effects of language planning, of mass literacy and mass media, have therefore a special relevance to the over-all study of linguistic evolution, though these factors, whose effect is recent at best, may be set aside for certain limited studies of language change. On language from an evolutionary point of view, see Greenberg 1954: chap. V; Greenberg 1959; Hymes 1961.

1. THE ISOLATION OF THE IDIOLECT

1.1. THE THEORIES OF HERMANN PAUL

Long before the nineteenth century it was widely realized that languages change,[1] but it is that century which is distinguished as the most vigorous period of historical linguistics. The theoreticians of the period were at pains to show that consistency of linguistic behavior, and in particular the regularity of sound changes, could be derived from more general, preferably psychological, principles. The culmination of this search was achieved by Hermann Paul (1880),[2] who developed the view that the language of the individual speaker-hearer encompassed the structured nature of language, the consistency of speech performance, and the regularity of change. In isolating the language of the individual from the language custom of the group, Paul developed a dichotomy which was adopted by generations of succeeding linguists and which lies, as we will try to show, at the bottom of the twentieth-century paradoxes concerning language change.

Idiolect and Language Custom. The task of the historian of language, according to Paul, is to state the sequence of particular language states (*Sprachzustände*; p. 29). The primary datum in this procedure is an object which he calls *psychischer Organismus*. This organ-

[1] For obvious reasons, awareness and discussion of language change developed first in the Romance world. The interest of Dante in this question is well known, that of his compatriot Tolomei less so (Claudio Tolomei, *Il Cesano*, ca. 1530). J. Chr. Kraus (1787) was already sophisticated enough to stress the opportunities offered to culture history by the greater conservatism of grammar over vocabulary. Many other examples could be cited. Therefore, Hockett (1965:185), like the authorities on which he bases himself, oversimplifies the matter in attributing the "genetic hypothesis" to Jones, Gyármathi, Rask, Grimm, and Bopp. The plain enumeration of these names is an oversimplification in another sense, too: the writers named differed greatly in their ability to draw inferences from the fact of change. For Grimm, temporal seriation of attested stages of Germanic languages was fundamental, but then he set himself no reconstructive tasks; Rask, on the other hand—although perhaps the boldest and most clear-headed thinker of the group—was slow in coming to terms with the facts of change; in his 1818 masterpiece he was still asking what *attested* languages Old Norse may have originated from.

[2] Our page references are to the more or less "standard" fifth edition (1920), which does not differ from the original on the points at issue here.

ism is conceived by Paul as a psychologically internalized grammar which generates the utterances of speakers.[9] "The true object of the linguist is the totality of manifestations of speech activity in all individuals in their mutual interaction" (p. 25). [This and succeeding translations are ours.]

The description of a language, in order for it to form a truly usable foundation for a historical view, must do more than fully enumerate the elements of which a language consists; "it must depict the relation of the elements to each other, their relative strengths, the connections into which they enter, the degree of closeness and strength of these connections" (p. 29). All these linguistically crucial relations can be found only in the language of the individual, in whose mind one will find the "interlocking image groups, with their multiple interlaced relations, which are relevant to speech activity" (p. 39). The image groups consist of "images" (*Vorstellungen*), that is, traces in the unconscious of physically and consciously perceived utterances.[10] Since the individual psyche is seen as the locus of the associations and connections between language components, we realize why Paul isolates the individual as the primary carrier of a language, and brings the argument to its logical conclusion by asserting that "we must distinguish as many languages as there are individuals."[11]

The isolation of the individual, Paul thought, had the advantage of

[9] Paul is specifically concerned with the generative power of an internalized totality of "image groups," as appears from his interest in kinesthetic and auditory self-monitoring of sound production and from his statement, in connection with (synchronic!) analogy, that speakers are able to form and understand sentences never before encountered. Paul expects a faithful description of an idiolect to reveal to us, "to put it in a popular way," nothing less than the speaker's *Sprachgefühl* (p. 29). As Paul sees it, it is impossible to infer the structure of the idiolect merely from the observation of utterances. "To relate [observed physical facts] to mental ones," Paul writes (p. 30), "is possible only through analogical inferences based on what we have observed in our own minds. Constantly renewed exact self-observation, meticulous analysis of one's own *Sprachgefühl*, is consequently a prerequisite for the training of the linguist."

[10] The "images" are by no means to be understood as pictorial representations, for example, of things nameable by concrete nouns; quite the contrary, every linguistic unit, every class of units, and every relation between classes is explicitly said to have a corresponding image as its mental representation. These images are related by "association" to form groups (pp. 26 ff.), thus yielding a full mental representation of the speaker's linguistic capacity.

[11] The ultimate individuality of language was of course already an important

attaching linguistics to a more general science of psychology. The price
of such isolation, however, was the creation of an irreconcilable opposi-
tion between the individual and society. Paul then had to construct a
theoretical bridge for passing from the unique, individual object of
linguistics to a transindividual entity.

A comparison of individual languages (which we may, at the risk
of terminological anachronism but with little fear of distortion, relabel
"idiolects"[12]) yields a certain "average," which determines what is
actually normal in the language—the Language Custom (*Sprachusus*;
p. 29). For the purpose of later discussion, let us note the following
features of Paul's "Language Custom." First, it is (unlike the idiolect)
an artifact of the linguist—a product of his work of comparing idio-
lects; no independent "existence" is claimed for it.[13] Secondly, a Lan-
guage Custom has no determinate bounds: every grouping of speakers
into dialect groups is arbitrary, without theoretical motivation (p. 38).
Clearly the Language Custom, or "average," resulting from a compari-
son of idiolects A and B would differ from that resulting from a com-
parison of idiolects A, B, and C—and there is no way to decide on the
grounds of Paul's circumscribed theory whether C should be included
or omitted from the comparison. Thirdly, if "Language Custom" were
seriously to be interpreted as an "average," it would be meaningful
only with reference to gradient phenomena; we might argue that $ü$ is
the "average" of u and i, but there is no obvious meaning to an "aver-
age" of, say, *soda* and *pop* as two idiolectal designations of carbonated
beverage. Fourthly, we must note that in postulating the absolute in-
dividuality of idiolects, Paul provides no clues for ranking differences
among idiolects on any scale of importance. It follows, then, that for
Paul the only object of theoretical significance is the idiolect: Lan-
guage Custom is derivative, vague, unstructured; since on his terms

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idea of romanticism; cf. Herder (1772:123–124), as quoted by Sapir (1907:
133–134).

[12] See § 1.22 below.

[13] Paul draws an analogy with the fictional conception of the species preva-
lent at the time: "Nothing has real existence except the particular individuals.
. . . Species, genera, classes are nothing but arbitrary summaries and distinctions
of the human mind" (p. 37).

structure and homogeneity imply one another, no structured object which is transindividual can be conceived.[14]

Change in Idiolect and in Language Custom. We are now ready to see how Paul treats language change. Changes in language can be understood in two senses: (1) as changes in an idiolect, and (2) as changes in Language Custom. Changes in Language Custom, in turn, can arise in two ways: (1) through changes within the idiolects over which a given Language Custom is defined; (2) through additions or subtractions of idiolects from the set of idiolects over which a Language Custom is defined. Suppose we define Language Custom LC_1 for the idiolects A, B, C, D. If idiolect B changes to B', then there results a change in LC_1; alternatively, if idiolect B is removed from the set (e.g., through the death of its speaker), or an idiolect E is added (through the birth or immigration of its speaker), or both, there is also a change in the Language Custom LC_1, for in principle every idiolect contributes something different to the Language Custom as a whole. Since the boundaries of the set of idiolects over which a Language Custom is defined have no theoretical foundation, and since changes in Language Custom are completely derivative (p. 18), it is change within idiolects which, for Paul, has exclusive theoretical interest. (What saves the investigation from being an absolute sociological fantasy is the fact, duly noted by Paul, that sets of idiolects of course often do have *natural* boundaries in the sense of communication breaks among speakers or communities of speakers; cf. p. 40).

What causes changes in an idiolect? There are two mechanisms involved: spontaneous change, and adaptation to the idiolects of other speakers (p. 34). On the intraindividual, spontaneous mechanism Paul has little else to say; he refers just once more to the role of an

[14] Beginning with this view of things one might yet think of salvaging, for study as a transindividual phenomenon, the *common core* of a group of idiolects—that is, not the "average," but that fragment of the Language Custom which is shared by all idiolects. However, Paul wants no part of this, and excoriates "descriptive grammar" for its procedure of recording "which out of a set of grammatical forms and relations are current in a speech community at a particular time, what can be used by everyone without being misunderstood and without striking one's interlocutors as strange" (p. 24). The cardinal sin of such an approach is its concern with "abstractions."

individual's "personal particularities and the peculiar stimulations
(*Erregungen*) of his own mental and bodily make-up" (p. 38), but
it does not occur to him to instantiate any such peculiarities, so that a
serious proposal of correlations between individual traits and idiolect
change is out of the question. The other mechanism of idiolect change,
as we have said, is the selective adoption of features from the idiolect
of one's interlocutors. One suspects that for Paul this, the social mecha-
nism, is the more important one; thus he says summarily in another
passage that it is "solely through intercourse (*Verkehr*) that the lan-
guage of the individual is created" (p. 39).

In view of the relation between idiolects and Language Custom,
which we have already discussed, we can see that Language Custom
changes "through the summation of a series of . . . shifts in idiolects
moving in the same direction"; a new Language Custom is formed from
an accumulation of parallel changes in the idiolects for which it is de-
fined. Now it is clear that this theory says nothing about two other
kinds of change which can be conceived of with equal reasonableness:
(1) qualitative, nongradual changes in idiolects, and (2) nonparallel
behavior of idiolects. If the changes are nongradual, they can hardly
yield to a "summation"; and if the idiolects are not changing in parallel,
what will be the result in the overall Language Custom? But it would
be meaningless to press the question in the context of Paul's theory,
because for him Language Custom with respect to nongradient phe-
nomena (i.e., in effect, with respect to the bulk of language) is not a
construct to be taken seriously.

Childhood and Adulthood. Given the two mechanisms of idiolect
change (and, by extension, of Language Custom change), we may
stop to consider whether an individual is equally liable to idiolect
changes throughout his life. In principle, says Paul, yes: "it is impos-
sible to designate a point in the life of an individual at which it could
be said that language learning has ceased." On the other hand, the
great bulk of language learning (idiolect changing) takes place in
childhood, and the difference in degree is enormous (p. 34). As a re-
sult, Paul feels justified in concluding "that the processes of language
learning are of supreme importance for the explanation of changes in
Language Custom, that they represent the most important cause of
these changes" (*ibid.*).

Unfortunately Paul does not develop this idea into any concrete hypotheses, and a number of questions remain unanswered. For example, if the mechanism of language learning works efficiently and uniformly, we would expect the set of young children's deviant idiolects to make the same small, stable contribution to every Language Custom; it would then be *untrue* that language learning explains changes in Language Custom. If, on the other hand, the learning mechanism works inefficiently, then we are entitled to know why children's mislearning does not have random, mutually canceling effects. In other words, invoking children's incomplete language learning as an explanation of language change is vacuous unless it suggests at the same time a *pattern* of learning failures. This Paul has failed to offer.

Unawareness. We may now go over to the discussion of a puzzle which arises from a combination of basic tenets in Paul's theory. If the significant locus of language change is in the idiolect, and if the idiolect is a psychological representation (the speaker's *Sprachgefühl*), why is it that speakers are not aware of changing their idiolects?[18] For an answer, Paul looks to the supposition that idiolect change takes place by infinitesimal steps (p. 19). But how can there be infinitesimal steps among discrete, quantized phenomena? How could one, let us say, move from *dived* to *dove*, or from *pop* to *soda* by infinitesimal steps? Possible solutions come to mind, and we will see below how other theorists have dealt with the question. Paul's own way out was arbitrarily to narrow the discussion from language in general to such aspects of language as are continuous (rather than discrete) in their design. He thus simply avoided the *general* question, which must of necessity deal with noncontinuous aspects of language as well.

The continuous side of language design with which Paul deals[19] is

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[18] That change is in fact unconscious is for Paul an empirical finding, though he admits that it is "not so generally acknowledged and must still be demonstrated in detail" (p. 18). He is thinking, of course, of the "natural" development of language, not intentional regulatory intervention which may be observed in standardized languages and which is nothing if not conscious.

[19] His main concern is with sound. In his account of semantic change (Chap. IV), where he distinguishes between customary meanings (idiolectally coded) and occasional meanings ("dispersed" acts of reference), Paul also deals with a continuum. Had Paul been interested in the problem of discreteness versus continuity as a feature of language design, he might have both enlarged on the parallelisms between phonetic and semantic change, and realized that they are

now elaborated by a further feature: variable performance. A speaker's
phonetic performance, we are told, varies around an (idiolectally
coded) goal in the way a marksman's shots scatter around a bull's-eye
(p. 54). The mental representations of the speech sound involve both
a kinesthesis (*Bewegungsgefühl*; p. 49) and a sound image (*Lautbild*)
for audio-monitoring (*Kontrolle*; pp. 53, 58). It is an empirical fact
for Paul that these representations are insufficiently precise to guarantee
absolutely consistent performance; for example, what is coded as a
single kinesthesis and sound image (today we would say: a single
phoneme) is manifested as the physiologically discriminable pair of
sounds [n] and [ŋ] in German *Land, Anger*; similarly, a single psy-
chologically coded unit appears as [d] in *Feldes* and as [t] in *Feld*.
Hence, where we can conceive of continuous dimensions of phonetic
space, "there is always a *continuous series of infinitely numerous
sounds*" (pp. 51–52).

This, then, explains fluctuation in performance which is not coded in
the idiolect and is not even perceived by the bearer of the idiolect.

Causes of Change. From here we move to the real crux: why does
the mean of the scattered performances shift? That it *can* shift without
being noticed by the performer is due, says Paul, to the fact that the
sound image for monitoring moves in parallel with the kinesthesis that
controls production (p. 61). But, granting that they shift together,
why do they shift at all? On this crucial question Paul's answer has a
general and a specific part. In general, language develops subject to
constraints of utility:

In the development of Language Custom, purposiveness (*der Zweck*) plays
the same role as that which Darwin attributed to it in organic nature: the
greater or lesser usefulness (*Zweckmässigkeit*) of the resulting patterns
(*Gebilde*) is decisive for their preservation or extinction. (p. 32)

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jointly not representative of the rest of language change. In characterizing
Paul's diachronic phonology as a study of continuous phenomena, we realize, of
course, that subsequent linguistic theory imposed a quantization where Paul
still saw a continuum. However, as should be apparent from the discussion,
Paul's views are not at all obsolete insofar as those phonetic phenomena are
concerned which form a residual continuum even after the discrete structure
has been "extracted" by phonemics.

Now, since an explanation by natural selection is vacuous unless an independent criterion for survival is postulated, Paul invokes, as a specifically linguistic factor, the principle of greater comfort:

It is hardly possible to detect any other cause for the inclination to deviate more to one side than to the other than the fact that deviation in one direction in some respect *suits* the organs of the speaker *better* [*bequemer* ist]. (p. 56)

In cases such as assimilation in consonant clusters (*octō* > It. *otto*), the factor of ease[17] is obvious. Sometimes length and accent may also be involved. Even the fact that "all languages display a certain harmony of their sound systems" (presumably related to different rest positions of the organs among their speakers) is an explanation. Of course, there are many additional kinds of change, especially of the "unconditioned" kind, and Paul seems to realize that the more transparent instances do not yet yield a *general explanation*. But he feels that further psychophysical research is the key; "the investigation of the essence of this greater or lesser comfort is a task for physiology" (p. 57). That the pursuit of comfort by infinitesimal shifts in phonetic performance is indeed the explanation—of that Paul is certain.

Correlations of sound change with climate, soil conditions, way of life, and other environmental factors are unproven, and those involving differences in the anatomy of speech organs are often incorrect and, in any case, indecisive (p. 60). Ease, admittedly, "depends on a variety of circumstances which may be different for each individual," but they "can also affect larger groups" (p. 57). When they do, a sound shift takes place (p. 59).[18]

But if the pursuit of ease is the cause of sound change in idiolects, the fundamental questions arise: why do not speakers go about it more quickly, and why do Language Customs split in that some speakers set out on a particular ease-seeking path whereas others retain their less comfortable pattern? This fundamental question will arise repeatedly in our discussion; we have already alluded to it as the actuation prob-

[17] The German *bequem* means both 'convenient' and 'comfortable.' As a noun, though, 'ease' seems preferable to 'comfort.'

[18] Elsewhere (p. 227), Paul also cites the elimination of morph alternation as a general tendency; presumably this, too, could be interpreted as a pursuit of ease.

lem. For even when the course of a language change has been fully
described and its ability explained, the question always remains as to
why the change was not actuated sooner, or why it was not simultane-
ously actuated wherever identical functional conditions prevailed. The
unsolved actuation riddle is the price paid by any facile and individ-
ualistic explanation of language change. It creates the opposite prob-
lem—of explaining why language fails to change.

Let us see how Paul copes with the actuation riddle.

Conformity. At all times, he says, the performance of a speaker is
under the pressure of different forces to change in different directions.
During stable periods of an idiolect, these forces are in exact balance
and cause the spontaneous deviations from the target to cancel each
other. For example, during a stable period of an idiolect, the scatter
of performances of the sound *a* may be under equal pressure to shift
toward *i* and toward *u*.

Yet it is very improbable that this should be the case at all points and at
all times. Chance alone can easily bring it about that in an area held to-
gether by particularly intensive intercourse one tendency should achieve
preponderance over another. This may happen even if the consensus of the
majority is not conditioned by any particular inner coherence *vis-à-vis* the
individuals remaining outside the group, and even if the causes which impel
the shift into a particular direction are perhaps altogether different for dif-
ferent individuals. The preponderance of a tendency in a limited circle
of this type is enough to overcome the contrary tendencies. (p. 61)

In this passage Paul seems to attribute the actuation of a change to
chance. However, if the beginnings of changes were random processes,
occasional losses of balance would alternate with restorations of bal-
ance, and beginnings of infinitesimal change would alternate with ces-
sations of infinitesimal change. Thus, chance is here invoked illegiti-
mately, since we are out to explain a specific, not a random process. The
substantive theoretical principle which Paul has covertly slipped in is
different—it is what we might call the "avalanche mechanism." But
in the case of avalanches, the stickiness of snow explains why a rolling
mass attracts additional snow; and in explaining avalanches, we may
indeed attribute their actuation to chance (or to some uninteresting
event, such as a skier telemarking in a particular location: cf. Martinet

1955:36). In the case of sound changes as described by Paul, however, no independent reason for believing in an avalanche mechanism is suggested.

There is, in fact, one more hypothesis covertly involved in Paul's theory: the hypothesis that speakers like to conform to the idiolects of their interlocutors. But whether or not this is a true belief, let us establish that it contributes nothing whatever to the explanation of sound change. This is because it is invoked *ad hoc* to explain both initial resistance to change and subsequent yielding to change. As we saw earlier, Paul holds that speakers adapt features from the idiolects of others *selectively*, but he offers no account whatever of their selectivity.

In describing the diffusion of a change from idiolect to idiolect, Paul makes free use of his conformity hypothesis:

Once a definitive shift in the kinesthesis [or any other idiolect feature[19]] has taken place through the elimination of the inhibitions exercised by communication [i.e., speakers' desire to conform to their interlocutors' idiolects], a further small shift is made possible by the continuing effect of the tendency. Meanwhile, however, a whole minority is swept by the movement. The very factors which prevent the minority from getting too far ahead of the general custom also prevent it from remaining significantly behind the progress of the majority . . . The movement proceeds in such small distances that a salient opposition never arises among individuals standing in close intercourse with each other. (p. 62)

Two important empirical claims are introduced here: (1) that the progress of a language change through a community follows a lawful course, an S-curve[20] from minority to majority to totality; (2) that frequency of a form guarantees its exemplariness for a speech community. We will have occasion later to discuss these claims further.

[19] Though Paul's discussion centers around sound change, everything he says here about the diffusion of changes (as distinct from their origin) could equally well apply to discrete domains of language, and his discussion from this point on could be generalized from sound change to language change without distortion.

[20] Compare Osgood and Sebeok (1954:155): "The rate of change would probably be slow at first, appearing in the speech of innovators, or more likely young children; become relatively rapid as these young people become the agents of differential reinforcement; and taper off as fewer and fewer older and more marginal individuals remain to continue the old forms."

The S-curved social trajectory of a change may in principle be located anywhere in a community. But it acquires special interest if it can be correlated with the universal differentiation of speech communities by age. According to Paul, we must distinguish between intra-generational and cross-generational changes. S-curved changes within a generation, he feels, are possible but necessarily minute. They reach major proportions only when the S-curve coincides with a shift in generations. If the change has already engulfed the majority, then the young people will "naturally" follow suit (i.e., they become the tail end of the S-curve). But even if a majority is still holding out, it will eventually die out. Moreover,

the same reasons which drive the older generation to deviate from kinestheses already formed must act on the formation of fresh kinestheses among the younger generation. It may therefore be said that the main cause [Veran-lassung] of sound change is the transmission of sounds to new individuals. For this process, then, the term "change" is not appropriate, if one wants to be completely accurate; it is rather a deviant new formation [Neuerzeu-gung]. (p. 63)

In other words: what for mature speakers is a performance that deviates from the coding of the idiolect becomes, for the children, an idiolect-controlled (nondeviant) performance.

It is easy to see why the notion of generations appealed to Paul, and to many other scholars, as a safe haven in a dangerous theoretical sea. If chronological changes in language can be superimposed on the turn-over of population, the need for a theory of change as such is canceled, since one can then simply think of the speakers of one dialect replacing those of another. (In geographic terms, diffusion of language material by speaker migration offers a similar, atypically easy case.) But a full-fledged theory must be accountable also for changes at different rates and in different directions, other than the replacement of fathers by sons (see § 2.41 below). Moreover, Paul's theory appears to take comfort from an unrealistic idea that the difference between genera-tions is discontinuous. To be sure, generations are discrete within a family, but in the community they form a continuum. A solid theory that is based on age differences must be prepared to treat them as an uninterrupted gradient.

Regularity of Change. When we come, next, to the question of regularity of sound change, we find Paul following not the extreme position of the Neogrammarian manifesto,[21] but a moderate point of view illuminated by the criticisms of Kruszewski. Since the history of this matter is usually presented inaccurately,[22] a slight digression is necessary.

The postulate of completely regular sound laws (i.e., without exceptions that are themselves accountable by non-*ad hoc* phonetic contexts) received its main momentum from Osthoff and Brugmann's reading of Winteler's 1876 monograph on the German dialect of Kerenzen, Switzerland. In the descriptive part of his monograph— which we honor today as a pioneering effort in phonemic analysis— Winteler stated the distribution of allophones in item-and-process terms. (As a Sanskritist—the Sanskritist, in fact, who put the term *sandhi* into European circulation—Winteler had of course studied Pāṇini, so that item-and-process phonology was a natural model for him.) Now, in looking for the most impressive instance of a sound law without exceptions, Osthoff and Brugmann resorted to Winteler's phonology: look at Kerenzen German, they said, where every *n* for example changes to *ŋ* before *k*, *g*—without any exception whatever. Historicists that they were, Osthoff and Brugmann did not notice that they were extrapolating from a synchronic process to a diachronic one.[23] The difference between the two, and the vastly lesser lawfulness of dia-

[21] Osthoff and Brugmann (1878).

[22] The standard treatments of the history of this period were written by scholars who were themselves Neogrammarians in spirit—notably Pedersen, but also Bloomfield. To seek a balanced view in these accounts is like basing the history of war on autobiographies of the victorious generals. Jakobson (1960) has performed a most valuable service by his study of the anti-Neogrammarian Kazan school; unfortunately, his article is as yet available only in Polish. A still broader treatment of the Neogrammarian controversy would consider the dialectological along with the synchronistic-analytical arguments against the doctrine of exceptionless sound laws.

[23] Leskien (1876), who is usually cited as the originator of the Neogrammarian hypothesis, could not possibly find support for it in his indeterminate material. Because historians of this period overlooked the strongest evidence available to Osthoff and Brugmann (viz., Winteler's monograph), they have tended, somewhat apologetically, to downgrade the postulate of exceptionless sound laws to a "hypothesis," and to attribute Osthoff and Brugmann's self-confidence to the exuberance of their youth. A less psychologizing, more strictly scholarly explanation of their self-confidence, however, is the fact that

chronic shifts, was soon afterwards pointed out by Kruszewski (1881);
the difference was lost, however, on the more orthodox Neogram-
marians; it was not understood by Pedersen, and unfortunately also
went unheeded by Bloomfield, for whom synchronic process did not
exist.

Paul *did* understand Kruszewski's point (cf. his references to Kru-
szewski's papers in Techmer's *Zeitschrift* [vols. 1, 2, 3, 5], p. 49), and
as he had no item-and-arrangement prejudices in phonology, he assimi-
lated the distinction easily. He thus distinguishes between sound
"shifts" (*Lautwandel*) and "alternations" (*Lautwechsel*). The former
are shifts in terms of *synchronic* process only, and are taken as com-
pletely regular. The latter are remnants of earlier synchronic processes
which (may) have ceased to function and which have left irregular
residues that must be learned as lists (p. 69). To avoid confusion, we
will hereinafter render Paul's *Lautwandel* by "phonetic rule," roughly
in the sense of Halle (1959). The problem for Paul, then, is not the
absolute regularity of phonetic rules, but the irregular redistribution of
sounds among lexical elements. In other words, how does a productive
phonetic rule of an idiolect get snagged? Can this result from inter-
course with other speakers? Here is Paul's answer:

The only way in which this could be visualized is that an individual would
simultaneously stand under the influence of several groups of persons which
had become differentiated by different sound development [i.e., different
synchronic phonetic rules], and that he would learn some words from one
group, others from the other. But this presupposes a thoroughly exceptional
relationship. Normally there are not [interidiolectal] differences of this
kind in a communication community within which an individual grows
up and with which he stands in much more intimate ties than with the
broader environment. . . . Within the same dialect, therefore, no incon-
sistencies develop, only in consequence of dialect mixture, or, as we shall
have occasion to put it more precisely, in consequence of the borrowing of a
word from a foreign dialect. . . . In the formulation of sound laws [i.e.,
synchronic phonetic rules], we need not of course reckon with such incon-
sistencies. (pp. 71–72)

The weakest link in this argument is the notion "single dialect," be-

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in the synchronic phonetic rules of Kerenzen German the Neogrammarians did
indeed have verified, nonhypothetical examples of exceptionless sound laws.

cause, as we have seen, it has no theoretical standing in Paul's thinking. Indeed, Paul shows some concern about this weakness, for he promises to consider later "the extent to which, and the conditions under which" word borrowing from other dialects takes place (p. 72). Actually, however, in the chapter on language mixture, only a short section is devoted to dialect interference (pp. 402–403), and the question of "conditions" for word borrowing is not even raised.

Phonology and Idiolect Grouping. We took note above of the manner in which Paul slipped from a theory of language change in general to a theory of sound change in particular. We may now examine the paradox that emerges as a consequence of this unmarked narrowing of discussion. Insofar as language change *in general* is concerned, we have learned, idiolects are subject to random development. To be sure, intercourse may cause parallel shifts in group idiolects, but they need not, and as Paul knew from dialectological research, do not in fact result in a hierarchically structured subdivision of the community (pp. 37–42). Idiolect *A* may form a dialectal grouping with idiolect *B* with respect to Feature 1, a grouping with idiolect *C* with respect to Feature 2. There is for Paul no end and no organization to these mutually intersecting principles because (1) the linguist knows of no grounds for a hierarchy of linguistic features, and (2) he has no explanation for the selective diffusion of idiolect features (i.e., no scale of diffusibility). Paul realizes that if there are breaks in the intercourse network —especially absolute breaks caused by migration—a dialect split will emerge; but this is completely "external" to the language, and we might add that it is in any case a highly unusual phenomenon (even if in the history of the ancient Indo-European languages it may have played an important role). Not so in the case of sound change; here there is a *linguistic* basis for grouping two idiolects into a dialect, namely, their sharing of a (complete?) set of phonetic rules. Idiolects *A* and *B* would be assigned to the same dialect if they shared the same phonetic rules, and a word adopted by *A* from *B* would be automatically submitted to the same phonetic treatment.

It would appear, then, that if our goal were a classification of idiolectal phonologies, Paul's theory would offer us a reasoned linguistic criterion for it—at least for a single-level, all-same-or-all-different clas-

sification of idiolects. But if we are seeking a classification not of idio-
lectal phonologies, but of idiolects in their entirety, Paul's theory is of
no use, because it does not guarantee (and could do so only contrary
to factual evidence) that nonphonological differentiation goes hand in
hand with phonological differentiation. It would be perfectly natural,
for example, to find a set of idiolects *A*, *B*, and *C* such that *A* and *B*
share phonologies while jointly differing in their phonologies from *C*;
but *A* and *B* may have numerous lexical and grammatical differences
in points on which *B* agrees fully with *C*.

Paul writes: "The truly characteristic factor in the dialectal articula-
tion of a continuous area is always the phonetic conditions." The rea-
son for this, thinks Paul, is that it is in the formation of phonetic con-
ditions that everything depends on direct personal intercourse. "In
vocabulary and in word meanings, in morphology and in syntax, medi-
ated transmission offers no difficulties." By contrast, according to Paul,
phonetic influence (i.e., diffusion of phonological rules) depends on
intimate and intensive intercourse. Thus, he continues,

much greater differences develop in phonetics than in vocabulary, mor-
phology, or syntax, and the former last more uniformly through long
periods than the latter. . . . Least typical of all is the vocabulary and its use.
Here transmissions from one dialect to another mostly take place [in the
same way] as from one language to another. Here there are more indi-
vidual differences than in any other domain. Here there may also be dif-
ferences [e.g., in professional vocabularies] which have nothing to do with
dialect differences, and which intersect them. (p. 47)

In this passage we face the conceptual difficulty of counting and weight-
ing phonological against other innovations. Are there not perhaps
more lexical innovations simply because there are more words? And
what is the theoretical basis for disregarding highly stable dialectal
differentiations in vocabulary and grammar? One suspects that Paul was
really deceiving himself. The priority he was giving to phonological
criteria of idiolect classification was based, not on the empirically dem-
onstrated manner of their transmission (for this he had no evidence),
nor on their stability (for this the evidence was quite inconclusive),
but simply because phonology, in the sense of a consistently applied
set of phonetic rules, was the *only* domain of language which gave any

hope of quantizing (= imposing discreteness upon) the continuum of the speech community.

To base motivated idiolect classifications on phonology may be a counsel of despair; it may also be justified by further argument, for example, as to the primacy of phonology within language as a whole. While we would disagree with both procedures, we would consider them as legitimate proposals meriting discussion. What makes Paul's approach illegitimate, on the other hand, is its use of a theoretical assumption in the disguise of a factual claim—and, to make it worse, on a factual claim which is incorrect.

Summary. Let us now attempt to restate critically Paul's position on the essential points:

The sole theoretically grounded object of linguistic study is the idiolect, and within the idiolect, the only domain in which change is related to stable performance is phonology (in view of its nondiscrete nature). An individual's usage is in principle consistent, and conforms to his mental representation of it, except that phonetic performances are scattered randomly as about a target. An individual may, by infinitesimal unconscious steps, skew the distribution of his (phonetic) performances as he seeks more comfortable behavior patterns. (No explanation is offered for the slowness with which allegedly more "comfortable" behavior is achieved; i.e., the actuation riddle stands unsolved and even unformulated.) Dialects are conceived as groups of (phonologically) identical idiolects; consequently, dialect change is simply idiolects changing in parallel, and dialect splitting is no more than idiolects changing diversely.

An idiolect or dialect may also change by "borrowing" forms from other idiolects or dialects. Such borrowing is selective, but no explanation is offered for particular selections. Opportunity to borrow from other idiolects depends on exposure to them; however, both borrowing and nonborrowing are attributed to conformity—either with the innovators or the conservers.

1.2. THE NEOGRAMMARIAN HERITAGE

Paul's *Prinzipien* may be said to reflect the best achievements of Neogrammarian linguistics. With his Neogrammarian predecessors and contemporaries Paul shared the virtues of maximum rigor of form-

ulation, an intensive interest in recurrent regularities, a feeling for the
atypicality of standardized languages among the totality of languages,
a concern with phonetic detail, and a desire to view language in the
setting of its functioning in order to understand its development, to
"portray as many-sidedly as possible the conditions of the life of lan-
guage [*Sprachleben*]" (p. 6). Written and revised after the dust had
settled over the sound-law controversy, Paul's book has the further
merit of recognizing the dialectological point of view on language
change. It is therefore not surprising that it became enormously in-
fluential, and though it eventually may have served as a target of anti-
Neogrammarian opposition it functioned as the basic text for more
than a generation of linguists.

1.21. SAUSSURE

The revolutionary effect of Saussure's thought is not belittled if we
assert that in the question of the individuality of language, he owes a
great deal to the Neogrammarian doctrine. For Saussure, the system-
aticity of language (see § 2.0) depends on the existence, within the
individual, of a faculty of association and one of co-ordination (p. 29).
The relations between elements of a language are located in the con-
sciousness of speakers. The following quotation is typical:

Synchrony knows only one perspective, that of the speakers, and its whole
method consists in gathering their testimony; in order to know the extent
to which a thing is a reality, it will be necessary and sufficient to investigate
the degree to which it exists in the consciousness of the speakers. (p. 128)

Indeed, it is the psychological unreality of diachronic and dialectologi-
cal relations that leads Saussure to assign historical phenomena to a
totally different domain of investigation. "The synchronic 'phenome-
non'," he writes, "has nothing in common with the diachronic . . .; one
is the connection between simultaneous elements, the other is the sub-
stitution of one element for another in time—an event" (p. 129). As
a consequence,

diachronic linguistics, in contrast to [synchronic linguistics], will study the
relations connecting successive items that are not perceived by a single col-
lective consciousness, items which are *substituted for one another* but which
do not form a system among themselves. (p. 140; italics supplied)

To guarantee the psychological reality of the object of synchronic

investigation, Saussure further requires that such an object be homogeneous. The object of synchronic linguistics, he argues, is not everything which is simultaneous, but only those simultaneous facts which belong to a single language. The separation of legitimate, that is, homogeneous, objects of study must proceed, "to the extent that this may be necessary, all the way down to dialects and subdialects" (pp. 128–129). Indeed, linguists are put on notice that there are no natural dialects—"there are as many dialects as there are localities" (p. 276). And Saussure adds: "Basically the term *synchronic* is not precise enough; it should be replaced by the (admittedly longish) *idiosynchronic*. On the other hand, diachronic linguistics does not require, but repels, such a breakdown."

It has often been stressed that by distinguishing speech from language, Saussure broke away from the psychologism characteristic of Neogrammarian thinking: he saw language as social and speech as individual. However, let us note that Saussure has nothing concrete to say about the community as the matrix of individual speech performance. In particular, there is nothing in his theory which could accommodate a heterogeneous language while saving it as a legitimate object of synchronic investigation. "A language . . . is of homogeneous nature" (p. 32). And Saussure echoes Paul when he writes: "Among all the individuals thus linked by the use of language [*langage*], a kind of average will be established: everybody will reproduce—not exactly, to be sure, but approximately—the same signs joined to the same concepts" (p. 29). Clearly, Saussure here views heterogeneity within the language custom of a community not as a subject of systematic description, but as a kind of tolerable imprecision of performance. His view is thus again in full conformity with Paul's, who had said that the "great uniformity of all language processes in the most diverse individuals is the essential basis for an exact scientific knowledge of such processes" (Paul, p. 19). We see no evidence that Saussure progressed beyond Paul in his ability to deal with language as a social fact; for him the precondition of dealing with language as a social phenomenon was still its complete homogeneity.

In broaching the cause of sound change, Saussure rejected all explanations which had been advanced (pp. 202–208). Although he was convinced that all changes originate in speech, he nevertheless had no

suggestions for distinguishing, other than a posteriori, between indi-
vidual innovations which enter the language and those which do not
(pp. 138–139). Although he posited two conflicting forces—that of
intercourse and that of provincialism (*clocher*)—to describe an in-
dividual's imitation and nonimitation, respectively, of the speech of
others, the balance of these forces remained a vacuous explanation,
since Saussure could not show (pp. 284–285) that the prevalence of
one force over the other covaried with anything else.

We can today easily assent to Saussure's argument that Old High
German *gesti* 'guests' did not coexist in the consciousness of any speaker
with the Modern German counterpart, *Gäste*, with the result that these
items have therefore never been linguistically opposed. What is missing
in his conception, however, is the possibility of a moment in time when
a more archaic *gasti* and a more innovating variant, *gesti*, did coexist
in the minds of some very real speakers of the language. Similarly,
when Saussure cautions against gathering spatially remote dialects un-
der the heading of a single synchronic description, we can agree with
him easily, but he regrettably omits from consideration the crucially
important case of *neighboring* dialects, whose systems are very much
"in the consciousness" of the same speakers.[24] Saussure's error, it seems
to us, was to equate the juxtaposition of remote stages of a language
with the juxtaposition of stages in general.[25] It is this unjustified gen-
eralization which lay at the basis of his antinomy between the structural
and the historical, an antinomy which has been accepted by the funda-
mentalists of the Geneva School[26] but which virtually all other linguists
have been trying to overcome.

1.22. BLOOMFIELDIAN DESCRIPTIVE LINGUISTICS

In the works of American descriptive linguists we find a varying

[24] Saussure fails to consider this possibility despite devoting a special section
(pp. 265ff.) to the "coexistence of several dialects at the same [geographic]
point" (where else, then, but in the minds of the same speakers?) and another
to the mutual influences of dialects coexisting with literary languages (pp.
267ff.).

[25] Paul was perhaps more farsighted in claiming that the portrayal of
processes from a comparison of language states will be more successful if the
compared states lie as close to each other as possible (pp. 31–32).

[26] Compare Sechehaye 1940: 30ff.; Frei 1944.

level of interest in language diversity within a speech community; what unites this group with that of its Neogrammarian teachers is the lack of interest in the systematic character of the heterogeneous language of a community.

Bloomfield writes:

A speech community is a group of people who interact by means of speech. . . . If we observed closely enough, we should find that no two persons—or rather, perhaps, no one person at different times—spoke exactly alike. . . . These differences play a very important part in the history of languages; the linguist is forced to consider them very carefully, even though in some of his work he is forced provisionally to ignore them. When he does this, he is merely employing the method of abstraction, a method essential to scientific investigation, but the results so obtained have to be corrected before they can be used in most kinds of further work. (1933:42–45)

As a preliminary set of guidelines, this statement would be unobjectionable; what is important, however, is that Bloomfield has no suggestions to make as to the way in which the "abstraction" is to be derived from the description of individual usages, or how it is to be "corrected."[27] Reflecting Saussure's emphasis on *langue* as a social phenomenon, Bloomfield concedes that

we are concerned not so much with each individual as with the whole community. We do not inquire into the minute nervous processes of a person who utters, say, the word *apple*, but content ourselves rather with determining that, by and large, for all the members of the community, the word *apple* means a certain kind of fruit. . . . However [he immediately concedes], as soon as we try to deal accurately with this matter, we find that the agreement of the community is far from perfect, and that every person uses speech-forms in a unique way. (p. 75)

Writing before the major developments in diachronic phonemics, Bloomfield did not yet respond to the possibility that the state of a language may itself function as a determinant of changes within it. Like Paul he therefore puts the whole burden of explaining change on the mechanism of imitating the speech habits of one's fellows. The direction of imitation, Bloomfield believes, is determined entirely by the

[27] Furthermore, the same questions as those which were raised by Paul's conception of "averaging" could be asked regarding Bloomfield's notion of abstraction.

"prestige" of the model (p. 476). Although this is now known to be factually incorrect, it is at least a step beyond Paul's and Saussure's vacuous balance and imbalance of contrary forces. Like Paul, Bloomfield distinguishes true phonetic and analogic-semantic changes, which take place in the speech of individuals, from the diffusion of such changes by the mechanism of dialect borrowing. "The processes themselves rarely escape our observation" (p. 481). "It is useless to ask what person or set of persons first favored [certain] variants. . . . By the time a sound-change becomes observable, its effect has been distributed by the leveling process that goes on within each community" (pp. 480–481). The distinction between the origin of a language change and its diffusion, and the pessimism about observing the origins of language change, steered Bloomfieldian thinking about language change into an antiempirical direction.

An important milestone in the isolation of the individual's language as the legitimate object of linguistic description par excellence was Bloch's "Set of Postulates for Phonemic Analysis," in which the term *idiolect* was first introduced. (Whether his recourse to the prefix *idio-* actually echoes Saussure's *idiosynchronic* is at this moment difficult to determine.) Bloch writes:

The totality of the possible utterances of one speaker at one time in using a language to interact with one other speaker is an *idiolect* . . . As for the words "at one time," their interpretation may safely vary within wide limits: they may mean "at one particular moment" or "on one particular day" or "during one particular year" . . . The phrase "with one other speaker" is intended to exclude the possibility that an idiolect might embrace more than one STYLE of speaking: it is at least unlikely that a given speaker will use two or more styles in addressing a single person. . . . Phonological analysis of a given idiolect does not reveal the phonological system of any idiolect belonging to a different dialect. (1948:7–9)

We see Bloch here executing Paul's and Saussure's atomistic principle of reducing the language of a community to its ultimate homogeneous parts. But we cannot refrain from noting that even this reduction *ad absurdum* is based on a counterfactual assumption that a pair of speakers always stick to the same style. (For evidence to the contrary, see, e.g., Labov 1966:90–135.)

The logic of the Neogrammarian theory, as inherited through Saussure and Bloomfield, was developed most fully by some of Bloomfield's students. We will return to their analysis below (§ 2.1), after discussing the isolation of structure as a factor in language functioning.

1.23. THE PRACTICE OF GENERATIVE GRAMMARIANS

Although generative linguistics has so far touched on historical problems in only a marginal fashion, there are several theoretical pronouncements on record suggesting that the Neogrammarian-descriptivist conception of a homogeneous system as the sole legitimate object of analysis has been adopted by this school of thought. Thus Chomsky writes:

Linguistic theory is concerned with an ideal speaker-listener, *in a completely homogeneous speech-community*, who knows its language perfectly and is unaffected by such grammatically irrelevant conditions as memory limitations, distractions, shifts of attention and interest, and errors (random or characteristic) in applying his knowledge of the language in actual performance. (1965:3–4; italics supplied)

The requirement of homogeneity is here made central: the linguistic competence which is the object of linguistic analysis is the possession of an individual; linguistic theory concerns the community only insofar as the community is homogeneous and insofar as the individual informant is a perfect representative of it. Procedures for overcoming the actual observed diversity of speech behavior are not suggested any more than in the work of Paul or Bloomfield; in harmony with Saussure, but more explicitly, Chomsky declares such diversity to be theoretically irrelevant. Thus he is quite right in saying: "This seems to me to have been the position of the founders of modern general linguistics"; but we cannot agree with his further statement that "no cogent reason for modifying it has been offered." As we will show below, we find cogent reasons for modifying this position in the confirmed facts that deviations from a homogeneous system are not all errorlike vagaries of performance, but are to a high degree coded and part of a realistic description of the competence of a member of a speech community.

2. PROBLEMS OF CHANGING STRUCTURE

2.0. TYPES OF RELEVANT THEORY

For Paul, the theory of language (*Prinzipienwissenschaft*) was, at least officially, coterminous with the theory of language change. After the development of the Saussurean antinomy between the diachronic and the synchronic, however, there arose a place for *two* bodies of principle—theories of language change and theories of language structure. The refinements achieved in the latter area, it turns out from our point of view, had inevitable and important implications for the history of language even where the original motivation of the conceptual advance was other than historical.

In relation to language change, each refinement in the theory of language structure (and the same could be said about refinements in the theory of speech communities) had the following potential effects:

(a) a *reclassification* of observed changes according to new principles;

(b) proposal of fresh *constraints* on change; and

(c) proposal of new *causes* of change.

Effect (a) is easiest to visualize. For example, when a separation between distinctive and redundant features was introduced into phonological analysis, all sound changes could be divided according to whether they did or did not involve distinctive features. Similarly, the distinction between prestigious and prestigeless dialects yielded a fresh classification of innovations depending on whether they moved up or down the prestige "slope." In the wake of most new theories of language, we indeed find papers setting forth the implications of the new ideas for history. However, in offering mere reclassifications of changes previously observed or observable, this type of advance is of limited interest for a *theory of language change* as such.

Far more significant is the possibility that a refinement in linguistic or sociolinguistic theory may allow (b) the hypothecation of *constraints* on change. Thus, a crude theory of speech sounds does not make it possible to assert very much about the actual phonological make-up of languages, but as the theory becomes more refined, the possible gen-

eralizations about how languages are constituted become richer and richer. Even in a completely inductive spirit, it becomes possible to make highly specific statistical generalizations about existing languages; it becomes possible, accordingly, to show whether a given change produces a language state that violates or, more significantly, conforms with the statistical norms. If one's observations of languages are, in addition, tied together by a broader theoretical structure, still greater significance can be attached to interconnected series of changes, and all the more challenging and meaningful becomes the search for "optimization" tendencies in language change.

Of maximum importance is (c) the proposal of new causes of change, based on a theory of language states so firmly established that one change in a language state necessarily implies another change *ex hypothesi*, so that event *A* can be designated a cause of change *B*. In its stronger form, a theory of change would identify *A* as the *sufficient* cause of *B*; in a weaker form, event *A* would appear at least as the *necessary* cause of *B*. It is only rarely that historical linguistics has had glimpses of such causal theories, even of the weaker (necessary cause) variety; but from such achievements as are on record we may draw hope of further advances.

The balance of our discussion is organized as follows: In the present chapter we consider the implications for language change of the structural theory that views language as a system of oppositional relations. The phonological problems here receive special prominence, since this is an area where contrastive and noncontrastive functions of the same substance have been distinguished with considerable success. We then turn (§ 2.4) to the historical implications of the factorial analysis which synchronic theory has applied to linguistic systems. The notion of distinctive features in phonology here receives the bulk of our attention. The theories discussed in Sections 2.1–4 represent important advances over Paul, but they share with him and his successors in American descriptive and generative linguistics the approach to language as a homogeneous, undifferentiated object; such subsystems as are posited within a language are viewed as noncompeting, but jointly necessary and complementary (phonology, grammar, lexicon). In Section 3 we turn to work that breaks with the homogeneity postulate and grapples with language as a systematically differentiated system.

2.1. Contrastive Function of Phonemes

As we saw above, Paul, the Neogrammarian, had no particular pre-
dilection for atomism in linguistics; we noted the structured way in
which he thought of a *Sprachgefühl* as a generative device. But it was
Saussure who came to stress the psychological reality of contrastive rela-
tions in a language and thus was required, in the interest of consistency,
to relegate historical correspondences to another domain, one of the
psychologically unreal. Using a chess game as his well-known analogy
Saussure insisted on disassociating the mutually determined functions
of the pieces (a synchronic fact) from the Persian origins of the game.
(What we miss in his program is an investigation into the changing
rules of chess.)

Soon after the contrast idea came to be applied to the study of sound
systems, mainly in Prague, descriptive linguists found occasion for a
predictable reclassification of observed changes. The required infer-
ences were first drawn by Jakobson (1931), who showed how sound
changes may be grouped into phonemic mergers (dephonologization
of variants) and splits (phonologization of variants).[28] A very similar
analysis was independently provided in America by Hill (1936).[29]

According to our scheme, these classifications in themselves consti-
tute only the lowest-level historical consequences of a new theory of
language. But before we examine their explanatory capabilities, let us

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[28] It is also Jakobson (1928) who must be credited with the bold attempt to
salvage Kruszewski's insights by proclaiming outright what escaped the more
Neogrammarian-minded Bloomfield—that the only sound laws operating with-
out exception in a given language are in fact the laws governing the distribu-
tions of contextual variants of a phoneme in a synchronic system.

[29] Bloomfield himself in 1933 had not yet quite assimilated his prephonemic
outlook on language history to his latterly acquired phonemic approach to de-
scription. Thus, in Sections 20.1–10 of his chapter on phonetic change, the ques-
tion of distinctiveness does not even arise; the presentation follows Paulian
lines quite closely. It is only in the last Section (20.11), which adheres to Paul's
doctrine about intimate contact as a condition for the borrowing of sound-
variant distributions (i.e. phonetic rules), that Bloomfield labels the sound
variants "non-distinctive," or "sub-phonemic." The key structuralist formula-
tion, incidentally, seems stronger than is warranted: "We can speak of sound
change only when the displacement of habit has led to some alteration in the
structure of the language" (p. 367). Granted that structure-altering changes
are more important by some descriptive criterion, why should they be claimed
as the only ones?

note that they also led to formal difficulties. The straightforward application of phonemic quantization to the continuum of language change soon turned up a dialectic puzzle: how do gradual, nondistinctive changes suddenly make the leap into a new distinctive category? Consider Hockett's eloquently baffled account:

Sound change itself is constant and slow. A phonemic restructuring, on the other hand, must in a sense be absolutely sudden. No matter how gradual was the approach of early M[iddle] E[nglish] /æ/ and /ɔ/ towards each other, we cannot imagine the actual coalescence of the two other than as a sudden event: on such-and-such a day, for such-and-such a speaker or tiny group of speakers, the two fell together as /á/ and the whole system of stressed nuclei, for the particular idiolect or idiolects, was restructured. Yet there is no reason to believe that we would ever be able to detect this kind of sudden event by direct observation ... (1958:456–457)

Reflected in Hockett's discussion is the synthesis of the Neogrammarian and Saussurean positions that the language of the idiolect serves as the locus of structural, that is, linguistically relevant, and legitimate facts. But the net result of this consistent synthesis is that a theory of language change becomes removed from empirical foundations almost entirely. It is difficult to accept an explanation through phenomena which are not only unobserved, but unobservable.

Elsewhere we have discussed the consequences of the claim that sound changes in progress cannot be observed (Weinreich 1960; Labov 1963, 1965). In our view, this self-defeating dilemma proceeds from an untenable distinction between the origin of a change and the propagation of the change which Saussure and Bloomfield adopted from Paul.[20] It stands to reason that the transition problem cannot be solved unless intervening stages in the propagation of a change are studied. In the quotation given above, Hockett focuses on the obverse problem: the unobservability of infinitesimal sound change is coupled with the unobservability of instantaneous structural change.

For scholars who feel uncomfortable with such an approach, several alternative solutions come to mind. One is to deny that change takes place within a system and to assert instead that the system (e.g., the dialect) has borrowed the new phenomenon from another dialect (e.g.,

[20] We earlier discussed the place of this distinction in Paul's thinking. The distinction was adopted virtually intact by Saussure (*Cours*, p. 283).

Hoenigswald 1960:72–73). If this formulation still contains dangers of a conceptual mystery (e.g., at which precise moment does the borrowing of the new phenomenon become "total"?), it can be revised further to envisage two coexisting dialects—one with the opposition in question, the other without (cf. Bloomfield 1933:328)—and speakers who fluctuate between the two styles of speech, favoring the "coalescent" dialect in increasing measure.[31] We reconsider this possibility in Section 3.2 as one approach to a more adequate view of linguistic structure.

Another solution is to assert that continuous variation exists within each dialect as a structural element, correlated with some other linguistic or nonlinguistic factor, and that the steady movement of tokens from one categorial class to another is part of the underlying structure (Labov 1966). Thus change would normally occur as one variable moved from a position *within* a given phoneme, to a position *across* phoneme boundaries, to a position *within* a second phoneme, and such a variable would be strictly defined by covariation with other features. (See §3.31 below.)

A second problem which arose in grafting phonemic theory onto the Neogrammarian theory of sound change was the temptation to identify the new analytic distinction, subphonemic/phonemic, with the historical (mutually coterminous) distinctions infinitesimal/discrete, fluctuating/stable, irregular/regular, and unconscious/conscious. Bloomfield, for example (1933:365ff.), thought that nondistinctive changes are observable only by the phonetician who has at his disposal "an enormous mass of mechanical records, reaching through several generations of speakers." But the identification of the dichotomies raises at least two theoretical difficulties:

(a) Granting (for the moment) that nondistinctive changes are not observed by naïve language users, why must the linguist necessarily have "an enormous mass of mechanical records" to determine, let us say, that subgroups of a speech community differ consistently in the use of allophones such as [x] versus [h] or [r] versus [R]? In other words, what deductive reasons are there to believe that nondistinctive variation is necessarily inconsistent or infinitesimal, so that phonetic

[31] Weinreich (1960:332). Corresponding reformulations could be worked out to cover phonemic split.

measurements of enormous masses of recordings are required for its detection?[32]

(b) If the explanation of changes in phonemic structure rests on the distinction between continuous phonetic behavior and discontinuous phonemes, how can we envisage a unified theory which would also encompass grammar, where the nondistinctive elements (morphs) are not continuous?[33]

But regardless of whether or not these theoretical difficulties can be ironed out, we are obliged to take note of empirical evidence which disconfirms the identification of the analytically distinctive with the historically discrete and the psychologically conscious. Thus the largely subphonemic replacement of lingual by uvular *r* in many European languages must have taken place by discrete steps (Hoenigswald 1960: 73); moreover, the distribution of the two variants is by no means the unstable one that their nondistinctiveness would imply. As to awareness, we find that speakers in many parts of the United States are extremely sensitive to subphonemic variants of /θ/ and /ð/, and quick to stigmatize the nonstandard usage of others. Similarly, the subphonemic raising of the vowels of *off, lost* in New York City is a matter of extreme sensitivity and a subject of much overt comment and correction in formal styles. On the contrary, the sweeping change in the repertory of phonemes which resulted from this process—the loss of distinction between *sure~shore, lure~lore*—is quite unnoticed and seems to evoke no social evaluation (Labov 1965, 1966). When we see a comparable absence of social awareness of the coalescence of phonemes illustrated by the massive merger of *cot~caught, hock~*

[32] The same objections could have been made against Paul. Non-distinctive variation has, in fact, been observed by Labov (1963, 1966) through far from "enormous" samples. Some scholars, incidentally, have been far more cautious than Bloomfield and Hockett. Lehmann, for example (1962:148), does not hold that subphonemic changes are unobservable, only that they are not taken into account by scribes rendering their language in phonemic terms. It is for this far-better-formulated reason that linguists have little information about nondistinctive changes of the past; and there is nothing in Lehmann's view to discourage us from phonetically observing nondistinctive changes in process.

[33] It is understandable why Hoenigswald (1960), at work on an over-all theory of language change, speaks of the "alleged" gradual character of phonetic alteration; to unify the conception of morphological and phonological change, he explains even the latter by resort to "dialect borrowing"—a discrete process by definition.

hawk throughout large sections of the United States, we are forced to
.conclude that there is no correlation between social perception and
structural status.

A serious weakness in the empirical foundations of the various
theories of linguistic change considered here stems from their auto-
matic reliance upon cognitive function as the prime determinant of
linguistic behavior. The assumption that perception was determined
only by contrastive (morph-distinguishing) units was never based
upon a sound empirical foundation, but rather upon a large number of
uncontrolled (anecdotal) observations of cases where perception did
match phonemic categories. A growing body of evidence from con-
trolled sociolinguistic studies indicates that perception is indeed con-
trolled by linguistic structure; but it is a structure which includes not
only units defined by contrastive function but also units defined by
their stylistic role, and their power to identify the speaker's member-
ship in a specific subgroup of the community (Hymes 1962; Labov
1966).

Let us see next what *explanatory* possibilities were found by his-
torical linguists in the contrastive function of phonemes which con-
tribute to the solution of the *evaluation problem*. This function made
it clear, for one thing, why phonemes should be rendered as far apart
as possible (de Groot 1931), and this in turn suggested why a shifting
phoneme should "repel" its neighbors in the system if mergers were
to be prevented (Hill 1936). In this matter, Paul had argued the
opposite:

Nowhere is any effort exerted for the prevention of a sound change. For
those involved are not even aware that there is anything of the kind to pre-
vent; after all, they continue in their faith that they speak today as they
spoke years ago, and that they will speak the same way till the end of their
days. (p. 58)

To the extent that Paul was doubting the likelihood of anyone chang-
ing a synchronic phonetic rule, one may go along with him; but of
course in his conceptual framework the assertion automatically carried
over to historical processes as well, and in so doing it became too
sweeping. Other observers were beginning to see matters differently.
Gilliéron understood the clash of homonyms as a dysfunctional phe-

nomenon for which language users had "therapeutic" correctives available to them. Martinet[34] integrated the views of other forerunners with a systematic functionalism in phonetics, and broadened the pathology-therapy conception from individual words to whole sets of words distinguished by a particular phonemic opposition. Our nineteenth-century predecessors would have been horrified at this teleological way of thinking; Martinet's statement of "prophylactic" aversion to phonemic mergers appeared more plausible to structural linguists, since he utilized the concept of the morph-distinguishing function of the phoneme rather than speakers' conscious efforts to avoid misunderstandings (1955:41–44).

For all the dysfunctionality of homonym clashes and mass word mergers, the coalescence of phonemes is plentifully attested in the history of languages. To prevent the preservation-of-contrast mechanism from explaining too much, Martinet adapted Mathesius' concept (1931) of "functional yield" as a kind of variable contrastiveness (1955:54–59). It was hoped that the theory would then permit oppositions from low-functional yield to collapse while still explaining the preservation of high-yield oppositions.

Thus, Martinet put forward one persuasive explanation for the fact that many changes occurred in groups or sequences—a fact that fascinated every linguist from Rask and Grimm on, but which was squinted at by the best of them out of a sound mistrust of "abstractions" or of mysticism in history. Martinet, moreover, solved a large part of the puzzle of "unconditioned" sound changes: the principle of syntagmatic context now found a paradigmatic counterpart, and syntagmatic "ease" (in Paul's terms) could now be matched by a thoroughly plausible notion of "paradigmatic ease" (Martinet 1955:59–62).

But it would be unfortunate if Martinet's achievements were to be accepted as defining the over-all framework for the explanation of linguistic change. The work of Moulton (1961, 1962) and some findings of Labov (1966) have provided empirical foundations for many of Martinet's conclusions which strengthen the less detailed evidence given by Martinet himself and his students. But, even within Martinet's framework, there is a need for detailed analysis to make important

[34] For simplicity we base our references to Martinet's work on his synthesizing book of 1955.

concepts more precise and reliable. Thus, the concept of functional
yield needs a great deal of refinement. There are few quantitative
studies bearing on it, and they suffer from a rather narrow conception
of the frame in which contrasts important for communication must be
maintained. They take a rather simplified approach to language by cal-
culating the yield of oppositions among minimal pairs uttered as iso-
lated lexical items. Other studies of functional yield have also erred by
setting too narrow an environmental frame (following and preceding
element), making it impossible to deal with such phenomena as
"breaking," vowel harmony, umlaut, or "preconsonantal r." We have
every reason to expect that transitional probabilities among phonemes
and the syntactic context (let alone the situational one) furnish vast
amounts of redundancy which variously diminish the value of a con-
trast, and we feel that more complex measures of functional load will
have to be worked out and evaluated before this highly attractive notion
is abandoned.[25]

Ferguson (1959) has suggested that the grammatical structure of
the lower-status member of two languages in the "diglossia" relation,
that is, the variety of language used in less formal situations, will reg-
ularly show fewer distinctions. As far as phonology is concerned, he
indicates that the lower-status system is the basic one, while the higher-
status system is best understood as a sub- or parasystem of the lower.
We now have empirical evidence to show that in one speech community
the most highly systematic phonology, which shows the processes of
linguistic evolution most clearly, is the one used in casual speech with
the minimum number of distinctions and the maximum contextual
support. In the long and ingliding system of vowels in New York
City r-less speech, one can find examples to support a seven-membered
series of contrasts—in the most formal speech. Thus we have:

beard	/ih/			moored	/uh/
bared	/eh/	stirred	/əh/		
bad	/æh/	barred	/ah/	bored	/ɔh/

but the forms to support this system are produced in a most irregular

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[25] See Hockett (1966) and Wang (1967) for critical approaches to this
problem. King (1965) explores the role of functional yield empirically with
negative results, but his environments are unfortunately limited to the immedi-
ately preceding and following segments as discussed above.

and unreliable manner. On the other hand, the most spontaneous speech (among lower-middle-class speakers) will yield a very regular system of the form:

> bear, bared, bad /ih/   moored, bored /uh/
>   stirred /ʌh/
>   barred /ah/

and this system is the product of a regular and rational process of linguistic evolution (Labov 1966: 559–565). Apparently there are motivating forces in linguistic change which can ride roughshod over any tendency to preserve cognitive distinctions.

The consequences of these findings must be built into the functional-yield concept and into a contrast-preserving explanation of serial language change.

Another example: the ancestor of the Yiddish dialects of Central and Eastern Europe distinguished long and short high-front and high-back vowels: $\bar{u}$, $\breve{u}$, $\bar{\imath}$, $\breve{\imath}$. In Southern Yiddish the back series was fronted to merge with the front vowels; in Northeastern Yiddish the long vowels merged with their corresponding short ones. We thus have:

<div align="center">

Proto-Yiddish

</div>

S.Y.   **N.E.Y.**

```
                 zün    'son'
zīn  <
                 zīn    'sons'         zun
                                  ><
                 zün    'sun'
zĭn  <
                                        > zin
                 zĭn(en) 'mind'
```

Now, it could have been argued (in an admittedly circular manner) that the functional yield permitted each two-way merger, but not a four-way merger (Weinreich 1958). Even this circular "explanation," however, is now invalidated by fresh empirical evidence. The most recent research (Herzog 1965:211 ff., 1968) has turned up two areas in which all vowels have merged into a uniform *i* (see Fig. 1): one in North Central Poland, the other in the Northern Ukraine. It turns out, further, that in the region surrounding the second area a shift
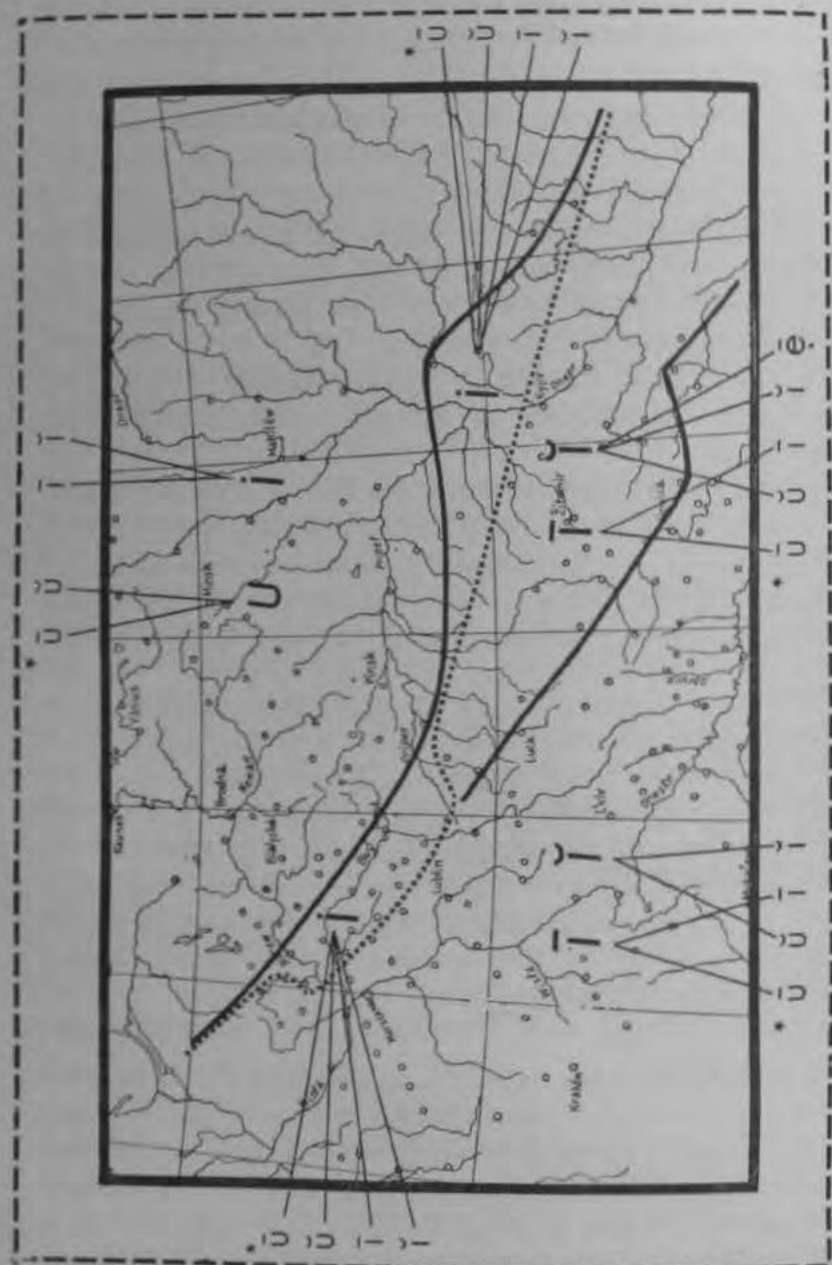
Figure 1. Intersection of /u ~ i/ and loss-of-length isoglosses in the Yiddish of Northern Poland.

of $*\bar{e} > i$ is in geographic complementary distribution with the full collapse of u's and i's. It would appear that still a fifth source of i vowels (producing, e.g., further homonymy with zin 'to see' $< *z\bar{e}n$) would have been too much. In the theory of functional yield as so far formulated, we find no basis for predicting that the merger seen in Northern Polish Yiddish was possible, or that the merger of Ukrainian Yiddish was possible only on condition that $*\bar{e} > i$ did not also take place.

The Yiddish Atlas, designed from the beginning to bear on problems of this kind, is turning up large amounts of relevant materials from phonology as well as grammar and lexical semantics.

It is also worth noting that the homonymy-prevention theory contributes little to the solution of the "actuation riddle." It is, of course, entirely proper to leave room for "further research," and one is entitled to hope that in some (privileged) cases, deep study of language states will explain not only why a change took place at a certain time in a certain direction, but also why it did not take place sooner. Martinet is certainly right in saying (1955:62) that a linguist should not be diverted from his search for causes by the complexity of the problems; but it is not clear that a theory based upon the functional yield of cognitive contrasts can provide the machinery for assessing the full complexity of causal relations within phonological structure. We note that the mechanism of ordered rules developed within a generative framework, which is not dependent upon a set of contrasting units at any level lower than the lexical level, does offer a rich field for searching out such deep-seated relations between superficially unconnected phenomena. But it seems to us unlikely that the actuation problem will readily yield to purely structural investigations, and we expect that their contribution will be confined to the task of stating limitations and elucidating—in part—the mechanism of language change. Solutions to the actuation problem must be expected from other directions.

2.2 GRAMMATICAL STRUCTURE

Revisions of analytical grammatical theory have, again expectedly, led to a reclassification of historical events on record. To take as an example the best-defined post-Paulian system of grammatical analysis—Bloomfieldian morphemics—we find the historical consequences

of the etic/emic distinctions developed systematically by Hoenigswald (1960). We may expect similar extensions of generative grammar to the description of historical events. Among investigations aiming at explanation rather than simple description, two lines of theoretical work, at least, may be cited. The first is connected with the formulation of grammatical universals; the second, with the study of conflicting productive patterns.

Underlying the search for universals is the Humboldtian vision that the languages of the world, in all their morphological variety, are designed to perform the same syntactic goals. This insight gives a theoretical foundation to such findings as the one that the loss of case systems in ancient Indo-European languages has been compensated for by the development of stricter word-order and prepositional systems.

A remarkably rich list of grammatical universals has been proposed by Greenberg (1963b); they are mostly concerned with word order. Recently (1966), he has turned to the examination of the diachronic implications of such universals, with promising results. Furthermore, he has taken a major step in testing certain synchronic universals which fail the test of absolute synchronic application, by examining their role as determinants of the directions of change. For example, he has investigated the claim that semantically unmarked categories (nominative) will tend to be morphologically unmarked, and semantically marked categories morphologically marked. Although there are many obvious counterexamples in Slavic noun declensions, his review of historical developments in Czech shows that any changes which did take place in the last few centuries were in the direction predicted by this rule. Two important modes of investigation are indicated by Greenberg's work: (1) the clarification through empirical means of the abstract claim that synchronic systems have "dynamic" tendencies (see Matthesius 1911), and (2) the use of quantitative methods to replace anecdotal evidence and persuasive argument. Though Greenberg has not presented any over-all theory of language structure or language change, his work is nonetheless extremely important for the empirical foundations of such a theory.

We are encouraged by Greenberg's use of quantitative methods and his ability to isolate significant trends in structure. At the same time,

one must admit that he is necessarily confined to surface structure at
the lowest level of reliability which is common to the descriptions of
languages available to him. It is sometimes argued that one must have
a comprehensive theory of language, or a theory of language change
as a whole, before one can begin to investigate language or language
change seriously. If one holds to this doctrine, one would have to be
extremely critical of Greenberg's workmanlike procedures. But one
might argue that some of the more lasting contributions to linguistics
have been in the form of partial explanations of limited areas of lan-
guage, while comprehensive theories which attempted to account for
everything have not shown the same longevity. We might ask in turn
whether any all-embracing theory can be erected at this time without
the rigidity which rejects new data and new methods. For the historian,
a set of validated universals becomes a constraint on possible changes
in a language. However, it must be admitted that so far grammatical
universals have provided language with an overlong historical tether
that is observed to stretch all too rarely; that is, the universals, especially
those envisaged by Chomsky, are so broad that we are unlikely to find
cases of changing languages which are approaching a possible "viola-
tion." But, of course, this type of linguistic investigation is only in its
infancy, and the future possibilities are quite unsurveyable.

The second line of work referred to above stems from a desire to
escape the vacuities of the Neogrammarian doctrine of analogy. In the
domain of irregular morphophonemic alternation, Paul and his con-
temporaries observed much unpredictable change, which they classi-
fied as "analogical." But as the critics of Neogrammarianism were
quick to point out, "analogy" as an alternative to exceptionless sound
laws not only was itself an *ad hoc* explanation, but also converted the
sound law itself into an *ad hoc* concept. (It is amusing and instructive
to find Osthoff, in the very volume whose preface became the Neogram-
marian manifesto, "explain" some changes in Greek numerals with
the most fanciful and arbitrary appeals to analogy.) Paul was well
aware that "since a form can, by virtue of its shape, belong to several
classes, it is possible to derive the remaining associated forms from it
according to different proportions" (p. 114). Of the various possible
developments, Paul therefore surmised, a form follows that proportion
which has the greater "power" (*Macht*). But since he suggested no

criteria for independently testing the "power" of a proportion, the argument is completely circular: the cause, itself unmotivated, can be known only through its effects. Bloomfield, despite the benefit of decades of additional research, in his discussion of analogical change could report no progress over Paul.

Fresh attempts to systematize linguists' experience with analogy were made after World War II by Kuryłowicz (1949) and Mańczak (1958); they are conveniently summarized by Lehmann (1962:188–192). The general rules formulated by these scholars, with a considerable body of documented evidence, provide new frameworks where previously there was mostly disorder.

Another way out of the free-for-all of analogy was sought by Frei (1929) and Bally (e.g., 1944). In the patterning of mistakes committed against normative French grammar, the Geneva scholars looked for evidence of dysfunctional aspects in the system against which the waves of change had begun to lap. This material would be particularly worthy of reconsideration if it could be extended to cover a variety of dialects in actual use by a given population.

2.3. ARGUMENTS RELATED TO LONG-TERM TRENDS

We have seen that a particular historical datum changes its status when it is viewed in the framework of different theories of language. Thus, the fronting of *u* (as in German umlaut) constitutes a significant change as soon as it happens—in a theory innocent of phonemics. From a phonemic point of view, this fronting is overridden in importance by the loss of the contextual condition (high-front vowels in the following syllable). Examples could be multiplied and ramified at will.

A given datum may also acquire fresh significance if viewed, not through a different theory of language structure, but as part of a different long-range trend. As Meillet put it:

Language changes get their meaning only if one considers the whole of the development of which they are parts; the same change has an absolutely different significance depending on the process which it manifests, and it is never legitimate to try to explain a detail outside of a consideration of the general system of the language in which it appears. (1906a:11)

The concept of *drift* endows the story of language with a meaningful "plot" that plays much the same role as a trend introduced by a his-

torian into the retelling of a sequence of sociopolitical happenings. Considered by itself, for instance, the fluctuation between clause-initial objective *who* and *whom* in English is but another case of allomorph alternation; but seen, as Sapir saw it (1921), as a conflict between two trends—movement of interrogatives to initial position versus specialized word order for the several noun phrases of a sentence—this fluctuation is converted into the last act of a long drama and endows it with enormous suspense.

As a rule, long-range trends have been formulated for one language or language group at a time. An example of a richly documented exploration of this type is Malkiel's paper "Diachronic Hypercharacterization in Romance" (1957–1958), in which polysemous entities are shown to have split up again and again into pairs of signs with separate signifiants (e.g., Lat. *leō* 'lion, lioness' into French *lion/lionne*). Malkiel seems to us to be quite right when he concludes that the study of a trend such as hypercharacterization "endows with rich meaning processes which, viewed in isolation, have traditionally been dismissed as insignificant" (p. 36), and that there is nothing incompatible between the documentation of such a trend and any accepted principles of linguistic theory. On similar grounds, we can appreciate Žirmunskij's work on long-term trends in German and in Germanic (1958). One wonders, however, whether the trends thus studied would not gain in theoretical significance if they were drawn from some independently motivated "schedule" of *possible* trends, rather than detected separately for each group of languages whose data happen to be within the grasp of a given historian (no matter how inspired). That is to say, despite the systematizing value of these long-range trends studied within their separate fields, one has the feeling that they will remain marginal to a comprehensive theory of language unless we can formulate a better *system* of trends.

2.4. DISTINCTIVE FEATURES AND PHONOLOGICAL CHANGE

The imposition of a purely functional conception of the phoneme onto the history of sound change often led to strange results; a radical change like

$$
\begin{vmatrix} t & d \\ & \\ \theta & \end{vmatrix} \quad > \quad \begin{vmatrix} d & ð \\ & \\ h & \end{vmatrix}
$$

fails to qualify as a linguistic change because the repertory of phonemes (i.e., the "structure") had not been affected; after the change, there were still three phonemes, in one-one correspondence with the three before the change (cf. Hill 1936:15; Hockett 1958:380). Such a purely functional view contended that the phonetic realization of the contrastive units was irrelevant to structure; and it thereby obscured the structural character of the most systematic large-scale sound shifts. The difficulty can be avoided, however, if we follow instead the Prague tradition of understanding the phoneme not only in terms of its morph-distinguishing function, but also in terms of its distinctive-feature *structure* (Weinreich 1960:330).

Applied to the history of languages, the structural understanding of phonemes again had, as its first consequence, a reclassification of certain changes. In the paper already referred to, Jakobson (1931) formulated a third type of change in addition to phoneme merger and phoneme split—reinterpretation of phonemes (rephonologization). It became possible to show how an opposition remained invariant while the means of its implementation changed: for example, the shift of Indo-European aspirates and nonaspirates to a corresponding pair of series of voiced and voiceless consonants. The very formulation of such a change was beyond the capability not only of Paul's theory, but also of a purely contrastive phonemics represented by Hockett (1958), where the phonetic realization of the units carried no structural significance.

More substantial advances in the diachronic application of distinctive-feature theory were made, again, by Martinet. First, he enriched the concept of "rephonologization" by a more fully developed and amply illustrated doctrine of the preservation of useful features (e.g., 1955:186–187, 199–211). Even more important was his development of what had long fascinated and puzzled linguists—the symmetry of sound systems. The Neogrammarians were mistrustful of it—again they saw dangers of mystification—and could not quite come to terms with the fact that "all languages display a certain harmony of the sound system" (Paul, p. 57), until Sievers offered a physiological explanation: a different rest position of the organs in speakers of different languages. Sievers' contemporaries welcomed this empirical basis for

the puzzling symmetries—but empirical evidence has not sustained the claim, nor has anyone been able to explain why such a rest position should control the functional realization of sound segments. It was the Prague phonologists who proceeded to a systematic description of these harmonies; and it was Martinet who attempted a major *explanatory* step by arguing that the conflict between the asymmetrical geometry of the speech organs and the (presumably) psychophysical economy of symmetrical utilization of distinctive features guarantees a permanent instability of sound systems. Martinet's illustrations of actual oscillations of systems between symmetry and asymmetry (1955:88 ff.) provide solid arguments for his theory, which must be included in any explanation of linguistic change, even if it still leaves the actuation riddle untouched.

The development of distinctive-feature theory also made it possible for the first time not merely to characterize sound systems in terms of presence or lack of certain sounds (or sound classes), but also to suggest necessary implications; for example, if a language has affricates, it will also have homorganic fricatives. The most ambitious attempt along these lines is Jakobson's (1941), and in the atmosphere of a reviving interest in universals of language, the search for implications seemed to be the most promising line of work in phonology (e.g., Ferguson 1963). Of course, the list of valid implications is still extremely shaky, so that attempts to use alleged universals as constraints on reconstruction (e.g., Jakobson 1958) remain highly controversial (cf. S. W. Allen's discussion, *ibid.*). But the possibilities along these lines are surely still far from exhausted.

2.41. GENERATIVE PHONOLOGY AND THE ECONOMY OF GRAMMARS

As in all the cases previously cited, the development of a new format of linguistic description—"generative phonology" (Halle 1959, 1962)—implies a restatement and reclassification of changes long on record. In particular, the formulation of phonetic redundancies in terms of ordered rules makes it possible to describe the differences between some phonological systems in terms of the same rules differently ordered (Halle 1962; Keyser 1963; Saporta 1965). Correspondingly and predictably, some changes can now be described as reversals of

order among existing rules. There was a time when sound changes
were being reclassified under the headings of "*additions* of phonemes
to the inventory, *deletion* of phonemes, *substitution* of phonemes,
transposition of phonemes." We presume that a repetition of this sim-
plistic exercise in relation to *rules* (addition, deletion) is not to be
taken as the chief contribution of generative theory to historical lin-
guistics. For regardless of the merits of generative phonology in the
description of language changes, it is far more important to see whether
it offers any new perspectives in the *explanation* of changes.

The most general statement of the application of generative phonol-
ogy to historical explanation is still that of Halle (1962); it has served
as the point of departure for a number of recent discussions of his-
torical change(e.g., Closs 1965 on syntax). Many of the issues raised
by Halle are both constructive and penetrating; yet consideration of the
empirical foundations of Halle's viewpoint reveals serious causes for
concern. We argue (§1.23 and elsewhere) that the generative model
for the description of language as a homogeneous object is needlessly
unrealistic, and we contend that it is quite pointless to construct a theory
of change which accepts as input descriptions of language states that
are contrary to fact and unnecessarily idealized. We will now take up
three aspects of Halle's argument which illustrate these limitations
most clearly: (1) the isolation of the individual parent-to-child rela-
tionship from the speech community, and the use of this relationship
as a model of linguistic change; (2) the isolation of specific historical
developments from their social context; and (3) the application of
distinctive features to concrete examples of change in phonological
space.

(1) *The parent-to-child model of linguistic change.* Halle's ap-
proach to what we have called the transition problem (see §3.1 and
ff.) postulates "discontinuities in the grammars of successive gen-
erations" (1962:65) and cites Meillet's parallel views on this point.
Halle's conception necessarily isolates the individual speaker-learner
and the individual parent-model from the speech community. The
mechanism he proposed for linguistic change might be diagrammed as
follows:
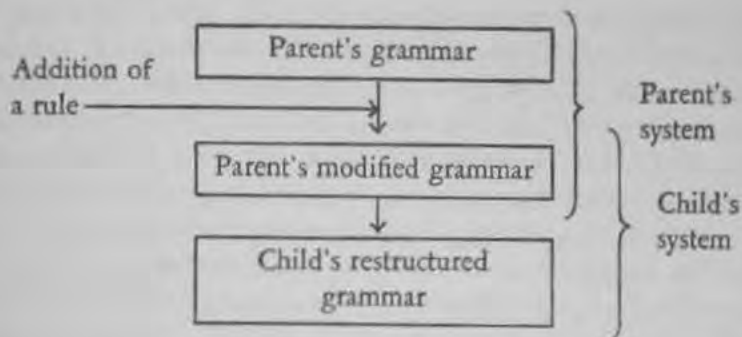
The image of the parent-to-child relationship as a model for language change is a plausible one, in the context of a structural model based on the study of individuals (or of a "homogeneous community," which is simply an individual under a group label). Furthermore, it seems clear that children do restructure their grammars not once, but many times, as they mature (Miller and Ervin 1964; Bellugi 1967). But the model depends upon the unexamined assumption that the children's grammars are formed upon the data provided by their parents' speech. Yet there is a mounting body of evidence that the language of each child is continually being restructured during his preadolescent years on the model of his peer group. Current studies of preadolescent peer groups show that the child normally acquires his particular dialect pattern, including recent changes, from children only slightly older than himself.[86]

In the light of this consideration, it is apparent that Halle's model

[86] In the various empirical studies of speech communities referred to in this paper, it has been found regularly that children of "first generation" parents do not differ in their dialect characteristics from children of families that have lived in the same area for many generations, even when the parents' dialect is markedly different from the local one. Thus the majority of the Lower East Side native speakers in the New York City study were second generation users of English—that is, English was not their parents' native language—but this fact was not inconsistent with a uniform and regular evolution of the basic vernacular of New York City (Labov 1966). There are two situations where parents' language may indeed be taken as the definitive model for children's language. One is in the isolated household—rural or urban—where the child cannot or may not play with other children. The other is in the direct transfer of a prestige feature from parent to child in the variety of careful speech used for scolding and correction (see Labov 1966b).

leaves many questions unanswered. Does the added rule originate
within the community? Do all parents in the community add the rule?
If not, what happens when a child from a family that has added the
rule speaks to a child from a family without it? The alternative situ-
ations implied here would undoubtedly affect the course of linguistic
change in different ways, which cannot be anticipated within Halle's
framework. He, like Paul, posits a discreteness of generations which
cannot be supported unless one ignores the fact that children derive
their language input from many sources. If we now suppose that the
preadolescent child can construct and reconstruct a simplest grammar
as his experience grows, it is apparent that structural changes produced
by his parents' late rule addition may never appear in the child's final
adolescent grammar. The very fact that the child can restructure his
grammar means that there is little point in looking at the parents' lan-
guage as the model for change in the child's grammar. Radical dif-
ferences between parent and child are then not evidence of the dis-
continuity of language change, but rather of the social distance between
the generations.

A further weakness of Halle's model is the implication that a change
is completed within a generation, the product of a specific relation
between parents' and children's grammars. But this implication is not
borne out by the empirical evidence of change in progress (cf. Gauchat
1905; Hermann 1929; Reichstein 1960; Labov 1963, 1966). These
investigations have described changes that continue in the same direc-
tion over several generations. Persistence in the direction of change
suggests that these changes are variables which have been evaluated in
the same way by the speech community over a considerable period of
time (see §3.3). A continuous process of transfer within the peer
group, from children slightly older to children slightly younger, is
consistent with such middle-range developments; but proposals for the
automatic restructuring of the parents' data by the child do not show
us any reason why the process would be repeated in successive gener-
ations.[57]

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[57] Finally, it is worth noting that the problems presented here are not irrele-
vant to the historical status of the switching rules suggested in recent genera-
tive treatments of the Great Vowel Shift. If we consider that switching rules
are in any way parallel to the changes that did take place, there would be some

(2) *Application to historical examples.* When Halle deals with an actual historical example, the isolating character of his basic model emerges even more prominently. Having separated the individual speaker from his group, Halle devises structural arguments to relate several individuals (= idiolects), without regard for the historical evidence available on the process of change within the speech community. He discusses the case of Early Modern English ę̄ in *meat, sea, beat* as an example of a mutation in the order of the rules which operate upon an underlying structure preserved from earlier stages of the language.

It is true that students of English have been puzzled for some time by the apparent reversal of a completed merger: in early sixteenth-century London, the word classes of *mate* and *meat* had apparently merged and were opposed to the class of *meet*; but in the seventeenth century the system emerged in its modern form with *mate* opposed to *meat* and *meet*:

	I (16th century)		II (17th century)
	meet	$\longrightarrow$	{meet} {meat}
	{meat} {mate}		mate
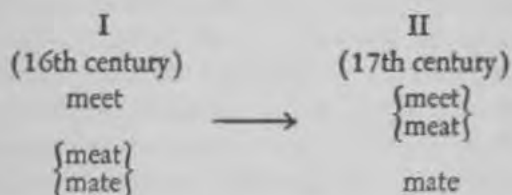
Halle uses this example to argue that merger at the (contrastive or bi-unique) phonemic level is not irreversible. But his example—no matter how hypothetical—loses its force in the light of the rich evidence brought forward by Wyld (1936) and Kökeritz (1953), which shows that the Systems I and II alternated in London for a considerable period, and that the social significance of the conservative and innovating rules must have been well known to most Londoners. In Shakes-

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obvious problems of communication between speakers who pronounced *sigh* as [siᶦ] and *see* as [seᶦ], and speakers who reversed these two pronunciations. One might argue that switching is possible between two successive and discontinuous generations, although this hardly seems consistent with Halle's restriction on mutual intelligibility as a constraint upon change (1965:66). But if we think of successive age levels transmitting linguistic tradition in a continuous pattern, then switching rules seem even more remote from processes which can occur in the actual process of linguistic change.

peare's texts, for example, Kökeritz finds ample support for the notion
that the conservative system was identified with refined and aristocratic
speech—as well known to the London commoner, no doubt, as Re-
ceived Pronunciation of the BBC is to Londoners today. Such speak-
ers must master both the old and the new systems, at least in their
perception. Whether we view the process as prolonged bidialectalism
or as inherent structural variability (see §3.3 below), we must assume
that some speakers of all ages were competent in the phonology of
both I and II (Halle's rules [14] and [22–23]). Surely an empirical
solution to the transition problem must take precedence over argu-
ments based on the manipulation of isolated structures; to ignore
empirical evidence, even in a hypothetical example, represents a step
backward in the explanation of change (in effect, to the position of
Saussure; see §1.21 above).

(3) *Application of distinctive-feature theory to sound shifts ob-
served in progress.* Studies of phonological changes in progress suggest
that Halle's proposals are inadequate in not being able to account for
frequently observed modes of transition. We have considerable quan-
titative evidence to show that there is close covariation between the
movement of low vowels in a front-and-back direction and mid vowels
moving to a higher or lower position. Moulton (1962) showed that
the position of the low-center vowel in Swiss dialects was a function
of the structure of mid- and low-mid-vowels in the front and back
series. Labov (1966:529–535) established that in a single speech
community the position of /ah/ was narrowly determined by the rel-
ative heights of mid-vowels /eh/ and /oh/. These quantitative rela-
tions imply the steady movement of a vowel along one dimension in
coordination with other vowels moving along other dimensions—over
several generations. Qualitative evidence of many recently completed
changes suggests the same pattern; in Yiddish dialects, the shifts $u > y$
[$> i$] and $\bar{o} > u$ are systematically related (Herzog 1965:170); in
many American dialects, we find similar on-going processes of the
fronting of /ah/ with accompanying raising of /æh/ toward /eh/.[38]

[38] This development is especially advanced in such Northern cities as Buffalo,
Detroit, and Chicago. A fifteen-year-old Detroit speaker, for example, was
amused to find that New Yorkers say "[baɪl] for [baɪl] ('bottle') and [bætl]

This evidence shows that the mechanism of change is not a sudden substitution or addition of higher-level rules, but rather the application of a continuous function to phonological space at a level where continuous values are possible. Furthermore, it shows that the two-dimensional model of the vowel quadrangle, based on articulatory positions of the tongue, provides the framework within which such changes operate, and that direct measurements of distance between low and mid-vowels are a factor in the rules operating (see §3.3 below). However, the distinctive-feature framework in which Halle is operating resolves the phonological space into independent dimensions. None of the definitions of gravity and compactness provided so far will give us a theoretical motivation for the covariation of gravity and compactness among consonants, and no such evidence has appeared. Thus if the historian of language should accept the distinctive-feature matrix, he loses the possibility of describing in a coherent way a series of shifts moving around the periphery of the vowel trapezoid.

Despite the three limitations discussed above, there remains a strong intuitive appeal in Halle's view of the role of children's rule-forming behavior in language change. We cannot ignore the obvious point that preadolescent children do construct grammars independently and may restructure them many times. But the parent-child hypothesis obscures rather than clarifies the empirical question as to whether change is continuous or discontinuous. The critical point for examination is whether we can locate any linguistic discontinuity in the succession of age groups in a given community.

Such a realistic investigation of discontinuity can proceed from a theoretical model which constructs grammars for heterogeneous speech communities. We argue that while linguistic change is in progress, an archaic and an innovating form coexist within the grammar: this grammar differs from an earlier grammar by the addition of a rule, or perhaps by the conversion of an invariant rule to a variable rule (see §3.3 below). If we adopt a view similar to Sturtevant's (1947: Chap. VIII), we would expect social significance to be eventually attributed to the opposition of the two forms. At some point the social and linguistic issues are resolved together; when the opposition is no

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for [biʔl̩] ('battle'). In one group of working-class Chicago boys, we find *John* realized as [jæn], *locks* as [læks] and *that* as [ðɛⁱt].

longer maintained, the receding variant disappears. This view of
change fits the general observation that change is more regular in the
outcome than in process. We can expect that abrupt change or dis-
continuity will occur at the point of resolution. It is at this point that
we might expect a sudden restructuring of the grammar: a search for
linguistic discontinuities in the succession of age groups would there-
fore be a necessary first step if Halle's suggestion is to receive firm
empirical support.

3. LANGUAGE AS A DIFFERENTIATED SYSTEM

3.0 A SYSTEMATIC APPROACH TO HETEROGENEOUS STRUCTURES

We now return to the fundamental question raised in Section 0: if
a language must be structured in order to function efficiently, how
does it function as the structure changes? We will propose a model of
language that avoids the fruitless paradoxes with which theories of
homogeneous structure have encumbered historical linguistics.

We have seen that for Paul as well as for Saussure, variability and
systematicity excluded each other. Their successors, who continued to
postulate more and more systematicity in language, became ever more
deeply committed to a simplistic conception of the homogeneous idio-
lect. They provided no effective means for constituting a speech com-
munity out of several such idiolects, nor even of representing the be-
havior of a single speaker with several idiolects at his disposal. Neither
did they offer an effective method for constituting a single language
out of chronologically disparate homogeneous stages. Yet most lin-
guists acknowledge the evidence which demonstrates that language
change is a continuous process and the inevitable by-product of lin-
guistic interaction.

The paradoxes have been deeply felt. Hockett, for example, exhibits
a painful sensitivity to the difficulty of reconciling the fact of change
with the categorial nature of homogeneous structure. On the one hand,
he asserts that the process of sound change is too slow and too gradual
to be observed except by its effects; on the other hand, he maintains
that the process of structural change is instantaneous and hence equally
unobservable by its effects. One can follow, as we have done, the his-

torical developments that led to these extreme positions, but few linguists can rest content with an explanation of change which depends upon the joint unobservability of all the processes involved.

We have suggested (§0) that the solution to the fundamental question lies in the direction of breaking down the identification of structuredness with homogeneity. We have proposed, instead, that a reasonable account of change will depend upon the possibility of describing orderly differentiation within language. In this section we will present such a model of language structure, together with some of the data which support it; we will then outline a strategy for a study of language change which rests upon these empirical foundations.

3.1. THE TESTIMONY OF LINGUISTIC GEOGRAPHY

From the beginning, the findings of linguistic geography have been used by historical linguists to bolster their theoretical viewpoints, but seldom has the evidence provided the proof that was desired.[39] If the isoglosses for each word involved in a sound change should coincide, the Neogrammarian hypothesis would receive strong support. But the painful fact is that they rarely coincide, even when they do aggregate to form loose bundles. The contention that each word has its own history reflects our inability to predict or even account for the ways in which one word precedes another across the dialect geographer's maps. Nevertheless, this evidence is presented in the standard Neogrammarian texts alongside unqualified pronouncements of the unexceptionable nature of sound laws (Bloomfield 1933:341, 361).[40]

Historical linguists also hoped that isoglosses would support the firm division of linguistic territories into hierarchically ordered sets of languages, dialects, and subdialects. Here again the evidence has been disappointing: an unselected set of isoglosses does not divide a territory into clear-cut areas, but rather into a crosshatched continuum of finely subdivided fragments. Bloomfield reviews this problem (1933: 341), but his own criteria for selecting the most significant isoglosses

[39] Compare Osthoff and Brugmann's reading of Winteler.

[40] One approach to reconciling the facts of dialect geography with the uniformity of sound laws is to argue that the attested fluctuations are the results of borrowing and reborrowing from one regular dialect to another. The process of sound change then drops out of the class of observable phenomena. (Cf. Weinreich 1960:330 for a critique of Hockett's development of this theme.)

for dialect classification have not proved successful in empirical re-
search (Weinreich 1968).

It was also hoped that dialect geography would provide support for
the notion that there is a negative correlation between "structuredness"
and communicability of linguistic phenomena. For Paul, for example,
everything in language was infinitely communicable by social inter-
course, and everything in a language responded freely, without re-
sistance, to outside influence—*except* phonological rules. In quite a
similar vein, Saussure felt that the wave model enlightens us about the
primordial laws of all the phenomena of differentiation (p. 287), al-
though we must presume that for purposes of reconstruction, that is,
again in phonology, Saussure would retain the *Stammbaum* which
postulates the mutual dependence of particular innovations.

The negative correlation between structuredness and communicabil-
ity was a perfectly natural extrapolation for a socially agnostic theory
of language. However, the correlation was never more than hypothet-
ical and it now appears that it was factually incorrect. Evidence was
given above that phonemic mergers expand outward, and this tendency
seems to be very general indeed.[41] It might be argued that the spread
of mergers represents the loss of structure, rather than the transmission
of structure. Yet evidence for the communication of structural fea-
tures is broader than this. The studies growing from the Yiddish Atlas,
for example, are turning up such interesting examples as the transmis-

[41] One can observe the expansion of mergers in a great many areas of the
United States on the basis of the Linguistic Atlas records completed a genera-
tion ago. The merger of the low-back vowels in *hock* and *hawk*, *Don* and
dawn is expanding beyond the core areas of Eastern New England and West-
ern Pennsylvania. Systematic observations of the same merger in the Western
United States indicate rapid expansion and solidification. The merger of /i/
and /e/ before nasals is expanding outside of the South, and has been observed
as far north as Gary, Indiana. Many distinctions before *r* are being lost in
areas where they were quite firm a generation ago: /or ∼ ɔr/ in *hoarse* vs.
horse, *pork* vs. *storm*, is one of the most striking examples, in the South as
well as the North. The distinction of /hw ∼ w/ in *which* vs. *witch*, *whale* vs.
wale displays a comparable instability, despite the fact that it is supported by
spelling. The chief exception to this tendency is the advance of *r*-pronunciation
into previously *r*-less areas, restoring in some regions the distinction between
god and *guard*, *sauce* and *source*. The advance of this prestige pattern, sup-
ported by mass media, is discussed below (§3.3). Note that in the great ma-
jority of *r*-less areas, most of these distinctions have been maintained by vowel
quality.

sion of a reoriented gender system from Northeastern to Central Yiddish (Herzog 1965:101–118). While the over-all advance of the Northeastern pattern manifests the loss of the neuter category, we also observe the importation of a new "intermediate" category which is the result of the borrowing of abstract concord relations rather than direct borrowing of lexical items. Moreover, this transition area shows the communication of a new constraint upon gender assignment involving the mass-noun:count-noun opposition which did not exist in the borrowing dialect (p. 103).

We do not mean to deny that a synchronic structural dialectology is possible: as an analytic exercise there is nothing wrong with it; but as the facts come in, such a mechanical extension of structuralism is becoming increasingly unilluminating as an account of the way languages develop.

We are not claiming, of course, that all innovations are equally communicable; if they were, there would be no intersecting isoglosses and no enduring dialect differentiation. We are merely denying that the synchronic structure of language furnishes us with the principal criteria for differential communicability.

The network of isoglosses which proceeds from a study of dialect geography often represents the synchronic equivalent of the *transition* problem—that is, the route by which a linguistic change is proceeding to completion. An understanding of the relation of these isoglosses to linguistic change frequently depends upon a solution to the *embedding* problem—that is, their relation to the linguistic systems and histories of the speech communities involved. It is more probable that a given isogloss represents a linguistic change in progress if its location cannot be accounted for by the linguistic or historical context. We can distinguish four types of isoglosses in terms of such "accountability."

(1) The isogloss or bundle of isoglosses coincides with a social or political (or geographic) boundary, representing the limits of the pattern of communication which led to the diffusion of the linguistic feature. The major discontinuities in the Yiddish of Northern Poland show several such boundaries (Herzog 1965:246–252). The isogloss bundle separating North Central from Central Yiddish coincides with a number of well-known political boundaries of the sixteenth century. The linguistic boundary between Northeastern and North Central

Yiddish coincides with a social discontinuity which is less obvious: the line along which Lithuanian Jewish settlers from the North met Polish Jewish settlers from the South and Center when this area was opened to Jewish settlement in the sixteenth century. This bundle of isoglosses is also a major division in the area of nonverbal culture.

(2) The location of the isogloss is accounted for by its systematic relation to other isoglosses which bundle with it. The clearest cases are those of linguistic *incompatibility*: where the advancing change represents a feature that cannot be simply added or subtracted from the system of the neighboring dialect encountered across the isogloss bundle. We observe such an example in the spread of a monophthongization of *aj* from Central Yiddish into the southern Ukraine, so that *hajnt* 'today' became *ha:nt*, and *majlexl* 'little mouth' became *ma:lexl*. The diffusion of this changing feature ended abruptly at just that point where the distinction of length was lost in the Northern Ukraine. If the monophthongization had continued, the monophthong would have coincided with short *a* in that region, so that *hant* would have represented both 'today' and 'hand,' and *malexl* would have represented both 'little mouth' and 'little angel' (Herzog 1968: Fig. 7).[42]

(3) The location of the isogloss is not accountable by any linguistic or social factors, but the direction of movement is predictable on linguistic grounds. Figure 1 shows an example of two such "free" isoglosses: on the one hand, the merger of *i* and *u* moving from southwest to northeast, and on the other, the merger of *i:* and *i*, *u:* and *u* moving from northeast to southwest. A general constraint upon linguistic change discussed above, that mergers expand at the expense of distinctions, leads us to posit the directions of the changes from the synchronic facts alone. It has of course been observed that the direction of movement can be predicted in many cases from geographic and configurational factors of dialect maps.

(4) The location of the isogloss is not accountable by either linguistic or social factors, and the direction of movement is not predictable. Many individual lexical isoglosses have this character. It may

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[42] Length was subsequently lost in the Southern Ukraine as well. Consequently, *a*(<*a:* <*aj*) does occur in *hant* 'today' and *malexl* 'little mouth.' However, original short *a* has moved to short *o*, and 'hand' is *hont*, and 'little angel' is *molexl*.

prove true that in all these cases we are dealing with items carried by mobile individual speakers along lines of trade and transit, rather than a steady diffusion of the linguistic feature from one neighboring speech community to another by more frequent and predictable patterns of communication.

The problem of accounting for the geographical transition of dialects across a territory thus appears to be symmetrical with the problem of accounting for the transition of dialects through time in one community. In each case, there is a contact between speakers with different systems. If we are to solve the mysterious paradoxes of change outlined above, it will be necessary to analyze the processes which occur in such contact situations in terms of how a speaker can understand and accept as his own the structural elements in the speech of others.

3.2. LANGUAGES AND DIALECTS IN CONTACT

A close study of the transition problem inevitably leads us to consider the transference of a linguistic form or rule from one person to another—more specifically, from one linguistic system to another. The simplest mechanism was that proposed by Paul in which the transference takes place between two isolated, homogeneous idiolects. For Paul, "language mixture" (including dialect mixture; p. 402) arises when two individuals, each by definition speaking his own idiolect, communicate with each other. When this happens, "the speaker influences the language-relevant imaginations (*Vorstellungsmassen*) of the hearer" (p. 390). There thus takes place either intercourse of nonidentical idiolects, or modification of idiolects by mutual influence.

No matter how we consider this model of language change, it seems unworkable; it neither matches empirical observations nor does it provide a reasonable model to satisfy our native intuitions. The problem is seen at its clearest in the rapid transference characteristic of pre-adolescent verbal culture. In the Boston area, children claim a share of cake or candy from their friends by saying "Allies," "Cokes," or "Checks." If a child from Providence or New York City should move into the Boston area, and attempt to claim a share by using an alien claiming term, we would reasonably expect his attempt to be rejected. Yet by one means or another, the claiming term "Thumbs up" spread to Boston and other Northern cities in the late 1950's, and displaced

the local terms. The direct influence of one speaker on another in the process of communication is clearly counter to the apparent self-interest of the recipient.

In Paul's account, command of two idiolects is considered only for historical purposes (as an explanation of the influence of one idiolect on another). No synchronic properties of bi-idiolectalism as such—neither analytic nor psychological nor social—are proposed for investigation. Thus Paul's theory allows *shifts* to other idiolects as well as interidiolectal *influences*, but not switching between idiolects.[13] If we abandon the individual homogeneous idiolect as a model of language we can suggest a more intelligible mechanism of transfer. It seems reasonable that the transfer takes place when Speaker *A learns* the form or rule used by Speaker *B*, and that the rule then coexists in *A*'s linguistic competence along with his previous form or rule. Change then takes place *within* the complex linguistic repertoire of *A*: one type is the gradual disfavoring of the original form at the expense of the new one, so that it moves to the status of "archaic" or "obsolete."

Bloomfield's treatment of Dutch sound changes showed a clear advance over Paul in this respect:

Every speaker is constantly adapting his speech habits to those of his interlocutors; he gives up forms he has been using, adopts new ones, and perhaps oftenest of all, changes the frequency of speech forms without entirely abandoning old ones or accepting any that are really new to him. (1933: 327–328)

The fact that Bloomfield was willing to entertain the possibility of a more complex model of transference indicates a general recognition of the importance of stylistic alternations in linguistic behavior. Studies of linguistic change in progress regularly uncover this type of alternation (Kökeritz 1953:194 ff; Labov 1963; Reichstein 1960). Every dialect atlas provides many examples of the archaic/innovating opposition within the competence of individual speakers. But we can also point to a distinctly different mechanism of change which can occur simultaneously with this one. When Speaker *A* first learns a rule, *q*, from *B*, it is not to be expected that he will learn it perfectly. Influenced by his

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[13] In the study of language contact, too, one distinguishes between back-and-forth switching and once-and-for-all shifting; cf. Weinreich 1953: 68–69.

own system, *P*, and without the full range of *B*'s experience which supports *B*'s system, *Q*, *A* acquires a rule, *q′*, of a somewhat different sort—a phonological rule with features altered, a lexical rule with different privileges of distribution, or a grammatical rule with some special conditions lost. Thus, in this initial transference, a second type of change has already taken place. But the more profound and systematic change is to be expected *after A* has acquired *B*'s rule. Within the single repertoire available to *A* (containing *p* in *P* and *q′*) we can anticipate an accommodation of *p* and *q′*—normally, an assimilation of *q′* to the features characteristic of *p* so that eventual insertion of a modified *q″* into the system *P* is possible. This process has been noted many times in the phonological adjustment of loan words. When *Trauma* was borrowed from German, the uvular *r* automatically became an American voiceless apical; but in the ensuing period of adjustment we can observe the /aw/ shifting to /ɔ/ in conformity with the general rule which restricts /aw/ before labial consonants. Yiddish * štik* 'piece, antic' was borrowed into New York City English in approximately the same phonetic form; but in the Negro community, where š does not occur in initial clusters,[44] the form shifts to [stɪk], homonymous with *stick*, with a number of semantic consequences.

When a traditional Negro speaker from the South migrates to the North he acquires the general term *common sense* which is only a partial match for his native term *mother-wit* or *mother-with*.[45] The two coexist as archaic *mother-wit* versus innovating *common sense*, but as the alternation between the two is resolved in favor of *common sense*, the modifier *mother-* shifts from its original meaning 'native, original' to the general 'female parent.' Thus some young Negro

[44] Thus *Schneider* appears as [snaɪdə], *shmuck* as [smʌk], and *shnook* as [snʊk].

[45] The term *mother-wit* is archaic or learned in the speech of whites, but is a matter of everyday use for Southern Negro speakers. Although it is equivalent to *common sense* in representing everyday, practical wisdom not learned from books, it differs in its firmer connection with the concept of native, innate intelligence; most white speakers do believe that one can acquire more common sense as one gets older. One of the remarkable facts about the *mother-wit ~ common sense* opposition is that whites are uniformly ignorant of the Negro use of *mother-wit*, and Negroes are uniformly ignorant that whites do not use the term. (Data proceed from semantic investigations associated with Labov 1963 and 1966.)

speakers, asked if men can have mother-wit, look puzzled and answer "No."

The study of languages in contact confirms the notion that stable long-term coexistence is largely an illusion, perhaps promoted by the existence of a relatively stable (or even dissimilating) lexicon and morphophonemics. Gumperz' investigation of the long-standing intimate contact of Marathi and Kannada in Kupwar (1967) shows the most radical adjustment of semantics, phrase structure, transformational component, and phonetics of the two systems. On the other hand, the vocabulary and grammatical morphemes are so patently differentiated that there can never be any doubt in a given sentence whether Marathi or Kannada is being spoken. Languages which are obviously different in surface structure have in fact become so similar that mechanical translation appears to be quite feasible through a simple dictionary look-up procedure.

Gumperz' findings were the products of a close study of bilingualism within its social context; his approach to the development of Marathi was a study not only of the transition problem, but also of the embedding problem. The objects which Gumperz analyzed were not the standard Marathi and Kannada described in textbooks but the coexistent systems which were in use within a specific social context. Part of the solution to the embedding problem for a particular language change is of course the study of its structural interrelations with the linguistic elements that surround it; but the solutions to these problems have often been artificial and unsatisfactory, since they compared structures which were not in actual contact in any real social situation. In principle, there is no difference between the problems of transference between two closely related dialects and between two distantly related languages.

In examining the linguistic changes which take place within the speech of bilingual or bidialectal individuals, we may look to purely structural factors; but the isolation of structure has failed signally to solve the problem of specifying bilingual interference. As Weinreich noted:

Of course, the linguist is entitled to abstract language from considerations of a psychological or sociological nature. As a matter of fact, he SHOULD pose purely linguistic problems about bilingualism . . . But the extent, di-

rection and nature of interference of one language with another can be explained even more thoroughly in terms of the speech behavior of bilingual individuals, which in turn is conditioned by social relations in the community in which they live. (1953:4)

We can now turn to the more specific examination of the contact situation and the systematic nature of the style alternation which is posited here.

3.21. COEXISTENT SYSTEMS

It was suggested above that we find a certain amount of contact between any two regional dialects: some speakers who control both dialects actively, and a larger number who have passive knowledge of the neighboring dialect but active command of only one. We also find in most speech communities distinct forms of the same language which coexist in roughly the same proportion in all of the geographic subregions of the community. This is the case not only in urban areas such as New York City, London, or Paris, but also in rural communities such as Hemnes, Norway, or Martha's Vineyard, Massachusetts. These coexisting forms may be known as "styles" but also as "standards," "slang," "jargons," "old talk," "cultural levels" or "functional varieties." In terms of the model of a differentiated language system that we are developing, such forms share the following properties:

(1) They offer alternative means of saying "the same thing": that is, for each utterance in A there is a corresponding utterance in B which provides the same referential information (is synonymous) and cannot be differentiated except in terms of the over-all significance which marks the use of B as against A.

(2) They are jointly available to all (adult) members of the speech community. Some speakers may be unable to produce utterances in A and B with equal competence because of some restriction in their personal knowledge, practices, or privileges appropriate to their social status, but all speakers generally have the ability to interpret utterances in A and B and understand the significance of the choice of A or B by some other speaker.

Throughout the 1920's and 1930's, one can trace a general tendency for linguists in both Europe and America to draw away from the simple psychological unity of the idiolect as posited by Paul. Mathes-

ius and his colleagues in Prague used a multilayer approach to characterize systems coexisting in the same community. Jakobson (1931) declared style switching to be a permanent fact which does not compromise the systematicity of each style as an object of linguistic description. In the United States we have seen that Bloomfield envisaged the coexistence of archaic and innovating forms in the same speaker. Furthermore, Bloomfield was fully capable of correcting his own earlier view that the complexities of "good" and "bad" speech styles are artifacts of literate cultures; when confronted with the Menomini situation, he recognized that hierarchically organized styles are the product of general social processes (1927). Confronted with this growing awareness of the heterogeneity of the language used by each individual, Bloch proceeded to develop a notion of the idiolect which represented only one of the possible systems within individual competence (1948:7).

Today it may seem naïve for Bloch to have imagined that he could avoid facing the facts of heterogeneity by limiting the idiolect to one speaker and one listener. If Bloch's idiolects were indeed to achieve homogeneity, then topic, situation, and even time would have to be rigidly controlled (Ervin-Tripp 1964; Labov 1966:90). As the awareness of the complexity of linguistic behavior grew, the domain of the idiolect shrank—eventually to the vanishing point.

Not every American linguist was devoted to the separation of the homogeneous linguistic objects from the heterogeneous life situations in which they were located. Some were not averse to discovering *within* the idiolect a multiplicity of layers. Fries and Pike, in their article "Coexistent Phonemic Systems" (1949), raised the possibility that systematicity and variability were not mutually exclusive. Although the writers restricted themselves to phonology, everything they said about coexistent systems could have been extended, *mutatis mutandis*, to the rest of language. Fries and Pike's paper did not deal with a really substantial example of competing subsystems; the Arabic elements in Swahili discussed by Harris (1951), for example, have much more internal coherence than the few Spanish elements labeled by Fries and Pike. But their paper marks a real advance because they did more than set these elements aside as extraneous: they saw that there could be a rich variety of systematic relations within such complex

mixed systems. Nor was their innovation purely synchronistic: the implications for history were quite clear to the authors, even though they were cautious to state them as mere "assumptions":

In the process of change from one phonemic system to a different phonemic system of the same language, there may be a time during which parts of the two systems exist simultaneously and in conflict within the speech of single individuals. . . . It is impossible to give a purely synchronic description of a complex mixed system, at one point of time, which shows the pertinent facts of that system; direction of change is a pertinent characteristic of the system and must also be known if one wishes to have a complete description of the language as it is structurally constituted. (pp. 41–42)

Strange as it may seem, however, Fries and Pike's significant departure was hardly utilized in concrete historical work. True, their scheme did become a keystone for the study of bilingualism—specifically for "dialinguistic" descriptions serving as a kind of specification of the competence of bilingual speakers (e.g., Haugen 1954, 1957; Weinreich 1953, 1957b). However, even though contact theory was perfectly capable *in principle* of handling pairs of chronologically "marked" dialects as well as the contact between more dissimilar systems (Weinreich 1953:2, 94–95), it does not seem in fact to have occurred to anyone that the theory could serve as a socially realistic basis for the investigation of language change.[46] Nor was there any rush to test Jakobson's view that style switching was a permanent feature of language against the few existing studies of sound change in process (Gauchat 1905; Hermann 1929).

The most detailed and reliable descriptions of such coexisting forms have been provided by scholars working in the Near East and South Asia. A rich body of qualitative, descriptive data on social and stylistic levels was developed by Ferguson, Gumperz, Bright, McCormack, Kelley, Ramanujan, Levine, and others, and Ferguson and Gumperz succeeded in assembling this material into a coherent set of principles which have been supported by further studies (Ferguson and Gumperz 1960; see especially the "Introduction"). Bright and Ramanujan (1964) were the first to develop a specific hypothesis on differential directions of language change based on a multilayer model of socio-

[46] An outstanding exception is a paper by Pulgram (1961).

linguistic structure. Gumperz went considerably beyond simple description in his study of Hemnesberget: here, for the first time, we have controlled data on natural groups within the community which demonstrate conclusively the mechanism of switching between strata which are functionally available to all members of the community (1964). Friedrich (1966) has now provided the most detailed explication of parallel change in complex social and linguistic systems. These empirical studies have confirmed the model of an orderly heterogeneous system in which the choice between linguistic alternants carries out social and stylistic functions, a system which changes with accompanying changes in social structure.

In order to assure the sharpness of our orientation, let us note that history is not the only direction in which the coexistent-system approach to language can be bent; the motivating interest for its development actually lay elsewhere. Mathesius, whose views on the inherent variability of the component subsystems are considered below, provided a synchronic distinction between portions of a vocabulary having different historical origins (1934)—an application which coincided with Fries and Pike's stimulus. The multilayer conception can also be used for purely analytic purposes to represent a language as a "diasystem" composed of member dialects (Weinreich 1954). For the theory to be of significance to historical linguistics, on the other hand, we have specified that the layers which it encompasses, while functionally distinct, be nevertheless functionally available to a group of speakers.

We insist on *functional distinctness* for two reasons. First, the layers must be in competition, not in complementarity. The coexistent phonological subsystems discovered in English, Mazateco, or Czech vocabulary are complementary—there is not, as a rule, a choice of rendering the same word in either system. They do not, consequently, constitute the layers in which the observer of change is interested.[47] Secondly, it is necessary to provide a rigorous description of the conditions which govern the alternation of the two systems. Rules of this

[47] This is not to deny that occasional "spot" competitions arise between members of complementary systems, for example, *defendable—defensible*, *mustáche—mústache*. When they do, corresponding dynamic consequences may be drawn.

sort must include extralinguistic factors as governing environments (Geertz 1960; Martin 1964) since the parallel subsystems all satisfy the linguistic conditions. The rules themselves, exclusive of their external environmental elements, must provide a *linguistic* description of the relations governing units matched across layers. If the coexisting subsystems have internal consistency, as discussed, for example, by Gumperz (1964:140, with further references), then a set of rules will share the same over-all external environments.[48]

We wish also to insist on the availability of the layers to a real group of speakers. Any pair of dialects can be brought under the heading of a single "diasystem"; the operation may be carried out even on areally noncontiguous dialects, and may serve a useful purpose in reconstruction. But it is only when a pair of dialects are jointly available to a group that switches back and forth between them—even if some members of the group only *hear* one of the styles and never speak it— that the multilayer formulation is relevant to an understanding of language change.[49] In urban societies, we find typically that the many

[48] In his analysis of a historically stabilized type of bidialectalism ("diglossia") Ferguson (1959) made a start toward the linguistic characterization of variables. He thus went further than was to go Gumperz (1964), who also contributes extremely valuable data but—perhaps out of a hesitation to compromise the structural rigor of linguistics by extending it to multilayer objects—postulates a "verbal repertoire" whose structure "differs from ordinary [one-layered] descriptive grammars" (p. 137). The differences between the two dialects of Hindi and Norwegian are sampled by Gumperz through loose lists, without an attempt to show by some kind of "diasystemic" formulas the presence of familiar relations, such as two-to-one phoneme correspondences or case syncretisms across the layers. Note, however, that in more recent work on the Marathi–Kannada situation in Kupwar (1967), Gumperz has explored the systematic relations of the two systems much more deeply, and his concept of a single linguistic repertoire has taken on greater solidity. For further articulation of the *diglossia* concept, see Fishman (1967); note that there are diglossia situations where the layers are *not* jointly available.

[49] For a comparable distinction between bilingual societies (without necessary presence of bilingual individuals) and bilingual mother-tongue groups (with bilingual individuals present by definition), see Weinreich (1953:88–89). In his Preface to that book, Martinet laid down a blueprint for a theoretical unification of three topics of study: language contact, dialectology, and style shifting. Unfortunately this unification remained unimplemented all too long. Moulton (1962) has judiciously criticized the idea of diasystemic formulas on the grounds that they would in practice be unmanageably complex. However, under the socially realistic requirement of joint availability of layers, as set

strata *are* available to the population as a whole, at least in the passive
sense: their competence includes the ability to decipher alternate ver-
sions of the code.

The multilayer conception of language, initiated by Mathesius and
Jakobson in Prague, developed by Fries and Pike in America, and cur-
rently applied more systematically to sociolinguistic studies by Gum-
perz, has opened new horizons for the theory of language change. It
replaced the concept of dialect borrowing—in principle a momentary
and accidental event—with the concept of style switching—in prin-
ciple a durative and recurrent phenomenon. It thus made unnecessary
the abortive search (envisaged, e.g., by Paul and Bloomfield) after
pure dialects undergoing change without interference. In short, it jus-
tified the study of language change *in vivo* and made it unnecessary to
rely on the past, which—no matter how richly recorded and ingen-
iously studied[50]—can never replace the present as a laboratory for the
linguist.

The Subjective Evaluation of Code-Switching. The great majority of
the investigations of heterogeneous speech communities have been
studies of linguistic behavior: the authors have aimed at separating
the various levels and determining the conditions for the speakers'
choice or alternation among them. Some predictions of the course of
language change in multilingual communities have relied entirely upon
a second source of data—demographic factors (Deutsch 1953)—but
most discussions introduce a third source—social attitudes toward lan-
guage (Kelley 1966; Rona 1966). A series of systematic investigations
of such attitudes have been carried out with considerable ingenuity by
Lambert and his colleagues (1960, 1967) with extremely regular re-

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forth here, we would ordinarily be dealing with no more than two or three
layers at a time; in such a case the complexity of the description would be less
likely to get out of hand. It should be added that multilayer statements can be
couched in any descriptive format; for a generative approach to a syntactic
multilayer phenomenon, see Klima (1964).

[50] Among the works which we have most admired are Kökeritz (1953) for
English and Fónagy (1956) for French. These brilliant studies are based on a
conscious recognition that the well-documented, socially conditioned fluctua-
tions which they trace belong to the central mechanisms of language change,
not to some marginal process of "dialect mixture." Compared to these analyses,
the schematicism of the Neogrammarians and of some modern structuralists,
generative or otherwise, is surprisingly antihistorical.

sults: the subjective correlates of language alternation appear to be more uniform than behavior itself.

Lambert's basic technique employs "matched guises"—the same speaker is heard at different times speaking French and English, or Hebrew and Arabic, or English with and without a Jewish accent—and the subjects rate these voices for a series of personality traits, without being aware that they are rating the same person twice.[51] There can be no doubt that deep-seated sets of social attitudes are powerful factors in determining the course of language history in multilingual communities; the case of India (Kelley 1966; Weinreich 1957b) is sufficient witness. In a further series of investigations, Lambert found that English-Canadian students who participate in the set of negative attitudes toward French speakers have much greater difficulty in learning French than students in the United States (1967:101–102).

Thus the study of the *evaluation* problem in linguistic change is an essential aspect of research leading to an explanation of change. It is not difficult to see how personality features unconsciously attributed to speakers of a given subsystem would determine the social significance of alternation to that subsystem and so its development or obsolescence as a whole. But the effect of social values on the internal development of a linguistic system is a more difficult matter, which we will consider in the following section.

3.3. VARIABILITY WITHIN THE SYSTEM

The heterogeneous character of the linguistic systems discussed so far is the product of combinations, alternations, or mosaics of distinct, jointly available subsystems. Each of these subsystems is conceived as a coherent, integral body of rules of the categorial, Neogrammarian type: the only additional theoretical apparatus needed is a set of rules stating the conditions for alternation. While these rules may be quite complex (Geertz 1960; Martin 1964), they do not interfere with the isolation of one or the other subsystem—a procedure which is typical

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[51] Thus the basic data consist of the *differences* between personality ratings given to the same person in two different guises (i.e., French-speaking vs. English-speaking). These reactions are thus evaluations of the use of a language or dialect as a whole. For subjective reactions to individual linguistic variables, see below.

of the traditional approach to nonstandard dialects. It has been assumed that the linguist can abstract one level or subsystem of such a complex without losing any information necessary for linguistic analysis, and many studies which isolate one of several such jointly available systems were carried out under the further assumption that the only possible basis for description is a homogeneous, invariant system. Thus we find that Sivertsen, in her excellent study of Cockney English (1960), abstracted from the actual data to provide a homogeneous account of a Cockney independent of any alternations with coexisting systems. Bailey did the same in her penetrating account of Jamaican Creole Syntax (1966). In both cases it was assumed that the variable elements in the data were the products of dialect mixture—irregular insertions of the standard language with which speakers were in contact. The consistent system of Cockney or Creole was identified as the set of variants which were *most different* from the standard language.

Although isolating studies of this sort may provide valuable starting points for linguistic analysis, in our opinion they offer no rational basis for the explanation of linguistic change. Such abstractions are no doubt more *consistent* than the actual data, and thus more amenable to the writing of rules without exceptions. On the other hand, if one attempted to describe how a speaker of Cockney or Jamaican English actually used the language, there would be many puzzling and uninterpretable inconsistencies in the data. Such inconsistencies would be interpretable within a more adequate model of a differentiated language applied to the entire speech community, which includes variable elements within the system itself.

3.31. Linguistic Variables within the System

There is no doubt that the differentiated model of a speech community presented so far is not entirely adequate to account for the complexity of observed structure. It is true that in many cases we find regular code-switching between two integrated structures, as in switching from Canadian French to English. Such switching implies strict *co-occurrence* between the linguistic elements and rules concerned. A *code or system* is conceived as a complex of interrelated rules or categories which cannot be mixed randomly with the rules or categories of

another code or system. So, for example, one would normally say in Jamaican Creole *im tired a tired* or in standard English *he's tired, that's all*, but not *he's tired a tired*.

Strict co-occurrence is often claimed for the rules of a dialect, but proof is seldom provided. A minimum requirement would be for the analyst to state that over a sufficiently large population of sentences A and A' were associated in the same sentence, and B and B', but that no instances of A and B' or A' and B were found; however, this type of statement is seldom supplied.

Since it often happens that the details of the alternating situation do not support such a claim, the analyst is forced to maintain that the speakers can switch codes in the middle of a sentence, a clause, or a phrase, not once but several times. For example, it is claimed that in the speech of young Negro children in Northern cities, the copula does not appear in the present tense, as in *You a swine!*[52] Yet for all speakers in this community the copula *is* will appear frequently in this portion. It is not uncommon to find in the most excited peer-group interaction, utterances such as *Make believe this is a team and this a team!* To claim that this and hundreds of other such examples are instances of code-switching would be an artifact of the theory and hardly an inescapable conclusion demanded by the data.

To account for such intimate variation, it is necessary to introduce another concept into the mode of orderly heterogeneity which we are developing here: *the linguistic variable*—a variable element within the system controlled by a single rule.

The inherent variability of linguistic phenomena was of considerable interest to members of the Prague Circle. In 1911 Mathesius demurred from Paul's requirement that languages necessarily be studied under the aspect of their homogeneity. Linguists have forgotten, Mathesius argued, that the homogeneity of language is not an "actual quality of the examined phenomena," but "a consequence of the employed method" (p. 2). In reality language is characterized by synchronic[53] oscillation in the speech of individuals. The systematic (coded) aspect of this oscillation Mathesius called "potentiality":

[52] Compare Stewart 1966. For an analysis of the syntactic arguments, and data on linguistic variables in Negro speech, see Labov and Cohen 1967.

[53] We follow the translator-editor, J. Vachek, in interpreting Mathesius' "static" as "synchronic."

If any dialect were absolutely constant from the phonetic viewpoint, this would imply the constancy of its inventory of sounds and of the phonetic make-up of each individual word; on the contrary, phonetic potentiality of a dialect implies potentiality of the inventory *and/or* of its distribution in words. (p. 23-24)[54]

Mathesius was careful to make clear that the potentiality which he discusses is primarily a *synchronic* phenomenon. There can, of course, also be dynamic (diachronic) oscillation, but

the dynamic [= diachronic] issues can only be solved after a more thorough research in individual languages has firmly established which phenomena can have been regarded in them, at the given time, as constant and which as potential. Only then will one be in a position to ask how long a potential phenomenon α can still have been regarded as basically the same phenomenon, only slightly affected by a shift of its potentiality, and when one must have already admitted the existence of a new phenomenon β, replacing α. The necessary investigations will be very difficult, but after they have been carried out we shall be better informed of the fundamentals of what is going on in language than we have [been] so far. (p. 31)

Mathesius' examples show a clear recognition of the transition problem that we have outlined above; however, they do not show that he had succeeded in the integration of his notion of "potentiality" into a systematic description of language. These examples show a near-random distribution of length or oscillation of grammatical options—variation without direction. The emphasis is on the variability of the individual rather than the regularities inherent in such variation.

Prague School writers have continued in the last two decades to develop their interest in variability and continuous change. We are particularly impressed by the papers of Neustupný (1961, 1966), reformulating the views of V. Skalička, which present penetrating criticisms of the rigid categorial framework normally employed by linguists. Neustupný insists on the recognition of the complex character of linguistic categories and the importance of marginal and

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[54] Among Mathesius' antecedents, according to his own comment, special honors go to Daniel Jones, who had characterized different styles of English in phonetic terms.

peripheral elements; he does not fail to underline the importance of these concepts for the theory of language change:

Closed classes do not allow for the transition from one phoneme to another . . . It was not by accident that R. Jakobson . . . postulated for phonological change the character of absolute leaps. However, the described method can explain the change by the inner, gradual development within a phonological class or group of classes. (1961:6)

The studies of historical change carried out by Vachek (1964a) have contributed a great deal to our understanding of the role of peripheral elements and their lack of systematic integration, in terms thoroughly consistent with Martinet's views. But despite our profound theoretical sympathy for the position of the Prague School, it must be conceded that they have not presented their views with a formal precision adequate for the complexity of the linguistic data. Nor have they developed empirical methods for work within the speech community which would allow them to investigate the processes of continuous change in a convincing manner.[55] It is therefore understandable that these writings have not had the impact upon the American scene which their theoretical importance would warrant. Certainly it is not enough to point out the existence or importance of variability: it is necessary to deal with the facts of variability with enough precision to allow us to incorporate them into our analyses of linguistic structure.

A linguistic variable must be defined under strict conditions if it is to be a part of a linguistic structure; otherwise, one would simply be opening the door wide to rules in which "frequently," "occasionally," or "sometimes" apply. Quantitative evidence for *covariation* between the variable in question and some other linguistic or extralinguistic element provides a necessary condition for admitting such a structural unit. *Covariation* may be opposed to *strict co-occurrence*, or co-occurrence may be conceived as the limiting case of covariation. Proof of strict co-occurrence relations may in fact emerge from a quantitative investigation of the type which provides proof of covariation. All rules may be considered to be of the form:

[55] Vachek, for example, relies upon a miscellaneous collection of phonetic observations in the literature to argue that [W] (voiceless [w]) has been and is now opposed to [hw] as a social and stylistic variable (1964a:29–46), and this proposal forms the basis of his analysis of the linguistic developments.

$$(1) \qquad A \rightarrow g[B] / X \begin{bmatrix} \\ \overline{} \\ Z \end{bmatrix} Y$$

$$(2) \qquad g[B] = f(C, D, E \ldots),$$

where B is one or more features of A, and C, D, and E are linguistic or extralinguistic variables. The expression $g[B]$ is the *linguistic variable* defined by the rule, usually denoted (B). Thus the variability of r-pronunciation in New York City can be represented:

$$(3) \qquad /r/ \rightarrow g[r] / - \begin{Bmatrix} K \\ \# \end{Bmatrix}$$

$$(4) \qquad g[r] = f(\text{Style, Class, Age}).[56]$$

In (3), the category $/r/$ is rewritten as the variable (r) in final and preconsonantal position, equivalent to the frequency of the constricted consonant $[r]$, a function of style, class, and age level of the speaker.

The usual categorial rule has the value of g set at 1. When entire systems of variables covary together, then the value of the controlling function g is identical for each rule which differentiates the systems. The value of g may also be idiosyncratic for a particular variable, but related to other variables in a more or less regular manner. The heterogeneous system is then viewed as a set of subsystems which alternate according to one set of co-occurring rules, while within each of these subsystems we may find individual variables which covary but do not strictly co-occur. Each of these variables will ultimately be defined by functions of independent extralinguistic or internal linguistic variables but these functions need not be independent of one another. On the contrary, one would normally expect to find intimate covariation among the linguistic variables.

The Transition Problem. Any close study of the transition from one linguistic system to another will require the determination of the value of a linguistic variable. It is possible, of course, that a linguistic change might occur as a discrete step—a simultaneous mutation of grammars on the parts of great numbers of speakers, despite the difficulties set forth above (§ 2.41). However, the changes which have been studied closely (e.g., Gauchat 1905; Hermann 1929; Reichstein 1960; Labov

[56] A previous rule develops the $/r/$ in *bird, work, shirt* in a different direction and so (3) does not apply to this class (Labov 1966:337–342). A somewhat different set of formal conventions for variable rules, embodying the same general principles, is presented in Labov, Cohen, Robins, and Lewis (1968);

1963, 1966) show continuous transitions in the frequencies and modal values of forms. Thus we can write for Gauchat's community of Charmey:

(5) $\varepsilon \rightarrow g[\varepsilon^1]$
(6) $g = f(\text{time})$,

that is, the variable (ey) representing the diphthongization of [ɛ] is a function of time. The independent variable of time is often inferred only from a study of distribution across age levels; this was indeed the case with Gauchat who actually showed only that:

(6') $g = f(\text{age})$.

Hermann's work, a generation later, gave the data needed to move from observations of age levels to statements about real time, for the diphthong [ɛ¹] did in fact become quite general throughout the population. In other cases, detailed quantitative studies of distribution across age levels have served to supplement more fragmentary observations made a generation earlier to provide the necessary anchor point and distinguish age-grading from the process of linguistic change.

If the linguistic variable were a simple distribution across age levels, then the process of transfer from one group of speakers to another somewhat younger would be a mysterious fact, easier to note than to explain. We might posit an intricate series of borrowings (Bloomfield 1933:403) or argue with Halle that the grammars of younger speakers are reconstructed along simpler lines with consequent mutations in the rules (§ 2.41 above). However, the cases that have been studied most carefully show the variable as a function of style as well as age, even in the early stages. We find that uneducated speakers, who show little self-consciousness and no correction in formal styles, will still show a stylistic differentiation between archaic and innovating modes. For example, working-class speakers in New York City use slightly higher vowels in *coffee, more, lost* in emphatic and affective expressions, even though they do not shift to lower vowels in formal style as middle-class speakers do (Labov 1966:256). We thus observe in their speech the

these deal primarily with the two-valued consonantal variables of nonstandard Negro English with considerable contextual and grammatical conditioning, embedded in a more comprehensive set of sixteen phonological rules of English. The rules given here symbolize the relations of multivalued variables within a Cartesian vowel space, relations which are currently being investigated in an instrumental study of sound change in progress.

differentiation of innovating and archaic variants of this variable
(oh):

(7)   (oh) = f(age, style).

The Embedding Problem. Linguists are naturally suspicious of any
account of change which fails to show the influence of the structural
environment upon the feature in question: it is reasonable to assume
that the feature is embedded in a linguistic matrix which changes with
it. Furthermore, we can argue that external factors have less effect
upon a feature which is a member of a system in equilibrium than
upon isolated features. Detailed studies of intimate covariation among
linguistic variables in process of change provide the most persuasive
empirical evidence of such systematic effects, although accounts of
completed changes are not without value in this respect.

Thus in the New York City vowel system (Labov 1966:507 ff.) we
find a variable (ah), representing the degree of backing of the long
and ingliding vowel in *father, pa, car, guard, bar.* This variable is a
function of another linguistic variable (oh), mentioned above. We can
represent this covariation by the abbreviated notation:

(8)   (ah) = f(oh).

This expression can be related to a more analytical feature notation at
the point where the binary set of features in a generative phonology is
replaced by a smaller set of linear dimensions. For reasons outlined
above (§ 2.41), the distinctive feature apparatus must be replaced here
by a homogeneous set of dimensions which define locations in phono-
logical space; however, we cannot outline the quantitative basis for
such dimensions here and we will therefore retain the binary. The rules
given below apply only to tense vowels generated in an *r*-less system
after [r] becomes vocalized and preceding vowels lengthened, and
therefore the features [+tense, +vocalic, −consonantal] are under-
stood for each segment operated on.[57]

[57] In order to interpret Rules (11) and (13), one must understand the feature
[grave] as equivalent to the dimension of fronting and backing, and [compact]
as equivalent to the dimension of height; these two dimensions form a Car-
tesian space in which the distance between two pairs of coordinates can be
interpreted as a straight line. The use of the variable notation is here, naturally,
extended beyond the binary choice of + or − to indicate a linear series of
values in the same manner as the treatment of stress in current generative pho-
nology. Whether or not the dimensions indicated by the features are continu-

Rule (9) defines the variable (oh) and (10) defines (ah). The systematic status of (ah) is established by (11), for without this relationship, (10) would merely state that the gravity of (ah) varies.

(9)
$$
\overset{5}{\begin{bmatrix} +\text{comp.} \\ -\text{diff.} \\ +\text{round} \end{bmatrix}} \rightarrow \overset{(\text{oh})}{\begin{bmatrix} \alpha\text{ comp.} \\ \\ \rho\text{ round} \end{bmatrix}} \quad / \quad \left[+\overline{\text{grave}} \right]
$$
where $1 \leqslant \alpha \leqslant 5$ and $\rho = \kappa\alpha$

(10)
$$
\overset{\bar{\text{a}}}{[+\text{grave}]} \rightarrow \overset{(\text{ah})}{[\gamma\text{grave}]} \quad / \quad \begin{bmatrix} +\overline{\text{comp.}} \\ -\text{diff.} \\ -\text{round} \end{bmatrix}
$$

(11) $\qquad \gamma = f[\alpha] = \sqrt{\kappa^2 - \alpha^2} \qquad$ i.e., $(\text{ah}) = f(\text{oh})$

Rules such as (11) are not predictions about individual utterances of individual speakers. A large number of small effects contribute to a base level of fluctuation which makes such predictions impossible. But the level of fluctuation or random variability is relatively low: (11) applies to small numbers of utterances of small numbers of speakers,[38] in such a way that the mean value of the variable approaches the limit predicted by the rule. Thus, (11) is a rule of grammar of a speech community, not of an idiolect.

When we look further into the system of long and ingliding vowels, we find that (ah) can be determined by a simpler and more precise rule involving a third variable (eh), the height of the vowel in *bad*, *bared*, *dance*. We can replace the distance formula of (11) with

(12)
$$
\overset{\bar{\text{æ}}}{\begin{bmatrix} +\text{comp.} \\ -\text{diff.} \end{bmatrix}} \rightarrow \overset{(\text{eh})}{[\beta\text{ comp.}]} \quad / \quad \begin{bmatrix} -\overline{\text{grave}} \\ -\text{round} \end{bmatrix}
$$

(13) $\qquad \gamma = f'(\alpha, \beta) = \kappa'(\beta - \alpha)$ i.e., $(\text{ah}) = g'((\text{eh})(\text{oh}))$.

Rule (13) states that if (eh) is higher than (oh), then (ah) is relatively front; but if (oh) is higher than (eh), (ah) moves to the back.

Vowels from other subsystems are involved as well: (ah) exhibits

---

ous or discrete is not decided here and is not crucial to the argument. (11) represents the finding that the distance between (ah) and (oh) tends to be constant.

[38] We find, for example, that stable values consistent with the over-all consistent structure are derived from groups as small as five speakers with five to ten utterances apiece (Labov 1966:113–131, 207–237).

strict co-occurrence with (ay), which represents the backing of the first element of the vowel in *my, side*.[59]

(14)   (ah) = (ay) = $g'((eh)(oh))$

No additional feature rule is needed here, since (ay) is equivalent to [γ grave] and is therefore governed by (11). Similarly, (oy), the height of the vowel in *boy, Lloyd*, shows strict co-occurrence with (oh) in the basic vernacular, and requires no additional treatment.[60] However, the relationship between (ah) and (aw), the backing of the vowel in *now, mouth*, is not that of strict co-occurrence. As a consequence of its structural position in the back-upgliding subsystem, (aw) covaries inversely with (ah) and (ay) (Labov 1966:540). In its simplest form, this relation would be expressed by (10'), which would precede (10):[61]

(10') $[+\text{grave}] \rightarrow [-\gamma\text{ grave}]$ / $\begin{bmatrix} +\overline{\text{comp.}} \\ -\text{diff.} \\ -\text{round} \end{bmatrix} \begin{bmatrix} -\text{comp.} \\ +\text{grave} \\ +\text{round} \\ -\text{cons.} \end{bmatrix}$

The extraordinary complexity of the relations of (ah), (oh), and the rest illustrates the wealth of arguments of a purely linguistic nature that can be brought to bear upon the problems of linguistic structure within a differentiated model. But here there are strong implications for the theory of linguistic change as well. It appears that the first step

[59] Before voiced obstruents and finally (ay) has the same tense nucleus as (ah). Data on the situation before voiceless obstruents are still inadequate to specify these rules more precisely. The binary notation used for the variables here is infelicitous, since it seems certain that (ay) and (oy) are to be analyzed as two segments in the phonemic output, but (ah) and (oh) as one segment.

[60] Note, however, that in formal styles (oh) and (oy) are treated differently. The irregular correction of (oh), lowering from [ʊ³] to [ʊ] or even [ɑ] does not apply to (oy) at all. It is therefore not uncommon to hear utterances such as [ðæts ʊl mɑi bʊ¹] but *boy* never appears as [bʊ¹]. The rules given here apply to the most systematic level of New York City dialect, the basic vernacular; as noted below, social correction applies less systematically to particular words or sounds, but rarely to features.

[61] Considerations of simplicity would lead us to this ordering. However, further study of the fronting of (aw) may indicate that it is less advanced and is a later generalization of (ay). Note that this dialect differs from the Philadelphia dialect used by Chomsky in that *my* and *now* originally shared the same low-center nucleus and were not differentiated as [mɑ¹] and [næ⁰]. For the systematic basis of the fronting of [aw] in terms of the back-upgliding subsystem, see Labov (1966:540).

in this complex chain of relations was the raising of (eh), entailing the parallel (generalized) raising of (oh), which led to an associated backing of (ah) and a parallel raising of (oy). The backing of (ah) and the raising of (oy) induced a backing of (ay), which was accompanied by an opposing fronting of (aw). This sequence of events can be supported by figures on their distribution through various age levels of the population, and independent evidence from earlier studies (Labov 1966:559–564).

This chain of events indicates that structural relations within language do not have the immediate, categorial, and instantaneous character which is sometimes implied in discussions of the homogeneous model. It is true that the raising of (eh) led to the raising of (oh): we recognize front-back symmetry as one of the near-universal conditions of linguistic change. But the generalization did not take place instantly; on the contrary, three or four decades passed before the raising of (oh) was in full swing. The associated changes show similar lags which can be traced in the data. Therefore we see that some structural relations are more remote and less compelling than others: an obvious, common-sense conclusion, but one which is difficult to handle without including linguistic variables in our view of structure.

The internal relationships of (eh), (oh), (ah), (ay), (aw), and (oy) are complex enough to satisfy any request for a demonstration of the systematic character of phonological systems. But they do not explain the process of linguistic change involved. Given the fact that γ is dependent upon α and β, and that α is indeed partly dependent upon β, we must still account for the behavior of β. This variable shows the most complex behavior and the greatest number of determinants. It is not possible to say that it is in turn dependent upon another linguistic variable. The system of changes is not mutually determining; the evidence rather points to

(15)   (eh) $= [\beta \text{ comp.}] = f(\text{age, style, class, sex, ethnic group})$.

While α, the variable feature of (oh), is not independent of β, it also shows a wide spectrum of social determinants by a similar rule (Labov 1966:254–258, 292–315). Linguists who wish to avoid the study of social factors will not be able to penetrate very far into this system: there is a social matrix in which the change is embedded as well as a linguistic one. Relations within the social context are no less complex

than the linguistic relations just outlined, and sophisticated techniques are required for their analysis. But for various reasons linguists have not pursued the explanation of linguistic change in this area with the energy and competence required. In the following section we will briefly consider the historical background for this reluctance.

The Embedding Problem: the need for social realism. One of the earliest and most eloquent claims for the role of social factors in language change was made by Meillet:

Language is an institution with an autonomy of its own; one must therefore determine the general conditions of development from a purely linguistic point of view; . . . but since language is [also] a social institution, it follows that linguistics is a social science, and the only variable element to which one may appeal in order to account for a linguistic change is social change, of which language variations are but the consequences—sometimes immediate and direct, and more often mediated and indirect. (1906a:17)

If the nineteenth century's attempts at explanation of language change had come to naught, Meillet felt, it was because the laws with which it operated stated only the possibilities, not the necessities of development. The stronger form of explanation would come from an analysis of social conditions.
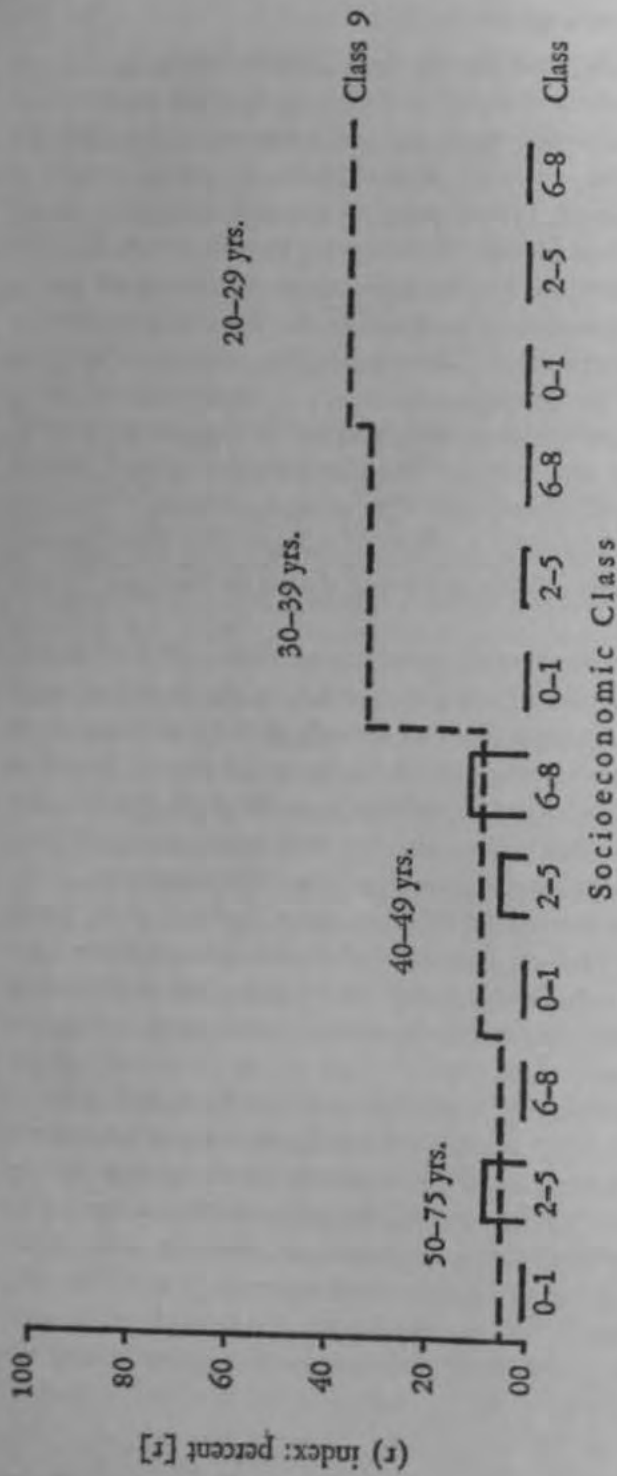
But for Meillet this remained, to a large extent, a set of desiderata. When he did venture into a concrete exploration of social factors, it was in the "easy" domain of lexical change (1906b); even there, he dealt with the remote past, and the only socially determined process he considered was the formation of specialized trade vocabularies and their infusion of words into the general circulation.

In the historical study of more intimately linguistic domains of language, phonology and grammar, reference to social factors was of course never completely absent from the literature: Wyld (1936), for example, based his conclusions about the history of English on letters and documents written by a broad range of the English social classes; Joos (1952) argued cogently that the prestige-marking differential is essential to explain "phonetic drift" in Old French; the exemplary works of Kökeritz (1953) and Fónagy (1956) penetrate deeply into the social context of historical change. But from the vantage of orga-

nized, cumulative linguistic theory, these contributions remained peripheral. In the citadels of theory an inverse proportion was developing between the degrees to which an explanation of change could be regarded as linguistic or social. Meillet (1906a), not yet inured to structural purism, pleaded for linguistic-*cum*-social explanations; more modern theorists, flushed with the increasing success of structural explanations, understandably looked on excursions into the social matrix of language as amateurish by comparison. An extreme position was taken by Kuryłowicz: "One must explain linguistic facts by other linguistic facts, not by heterogeneous facts. . . . Explanation by means of social facts is a methodological derailment" (*Lingua* 1.84 [1948]; quoted by Pulgram 1961:324 n.). For Kuryłowicz, even the influence of other languages was irrelevant: "the substratum theory . . . has no importance for the linguist." A different, and less recalcitrant, delimitation of the domain of linguistics was drawn by Martinet (1955: 190–195).

The distaste for amateurish sociologizing shown by Kuryłowicz, Martinet, and others may have been justified by the facts then available. We believe, however, that, as a result of the recent studies of complex sociolinguistic structures and language change in American English which we have cited, a position of sociological agnosticism in structural linguistics has become obsolete. Sociological factors, solidly formulated, have now been adduced to explain distributions and shifts in linguistic phenomena which, from a structural point of view, would have been seen as random. It would follow that the enlightened linguist examining language change will find it difficult to avoid enlarging the area of his competence, or enlisting colleagues to bring in new sources of reliable data.

A number of the linguistic variables that have been studied reveal a complex *sociolinguistic structure*, in which the value of the variable is determined by several social and linguistic factors as suggested by the schematic rules (3)–(4) above. The interpretation of the data in terms of language change depends upon the entire sociolinguistic structure, and not merely upon distribution in apparent or real time. The variable (r) in New York City provides one such example of the complexity of the data required for the analysis of linguistic change in progress.

Figure 2. Class stratification of (r) in casual speech by age group for adult native New York City speakers.

0-1 lower class
2-5 working '
6-8 lower m. class
9. upper '' ''

Figure 2 shows mean (r) indexes—the frequency of constricted (r) in final and preconsonantal position—for a number of subgroups among adult native New York City speakers in casual speech. Along the horizontal axis are four age groups, each one subdivided into socioeconomic levels 0–1, 2–5, and 6–8, informally labeled "lower class," "working class," and "lower middle class." The level of the highest socioeconomic group, 9, "upper middle class," is indicated by a dotted line. There is no clear over-all trend toward an increase in r-pronunciation; the great majority of New Yorkers remain r-less, as one can hear at any time on the streets of the city. What Figure 2 shows is an increase in the *stratification* of (r): the distance between the upper middle class and the rest of the population is increasing. For the older age groups, there is no particular pattern in the distribution of (r): for the younger groups, [r] has evidently acquired the social significance of a prestige pronunciation.[62]

Figure 3 summarizes the situation for all age groups, and adds data on a wide range of styles. The horizontal axis shows casual speech, style *A*, on the left, followed by styles in which more and more attention is given to speech: at the extreme right is the context of minimal pairs in which the phonological variable itself is the focus of attention (*god* vs. *guard*). The status of [r] as a prestige marker is here indicated by the general upward direction of all subgroups from informal to formal contexts. Class 6–8, in particular, shows an extremely rapid increase, surpassing Class 9 level in the most formal styles. (For further detail, see Labov 1966:237–249, 342–355.)

How is a socially agnostic linguist to react to these facts? That we are dealing with a change in progress is apparent from the half-generational differences displayed in Figure 2 and independently confirmed in many other ways which we cannot develop here. That the behavior of socioeconomic subgroups is differentiated is also established (see pp. 154–204 for sampling procedures). A linguist excluding sociological factors would have to deal with New York City

[62] This change seems to mark a sharp break between those born in 1923 and before, and those born after. The overt development of the new prestige pattern appears to have followed shortly the end of World War II. It is not uncommon today to find teen-age upper-middle-class speakers who are almost consistent r-pronouncers. For the problem of the penetration of the entire population by a prestige pattern, see Labov (1966b).
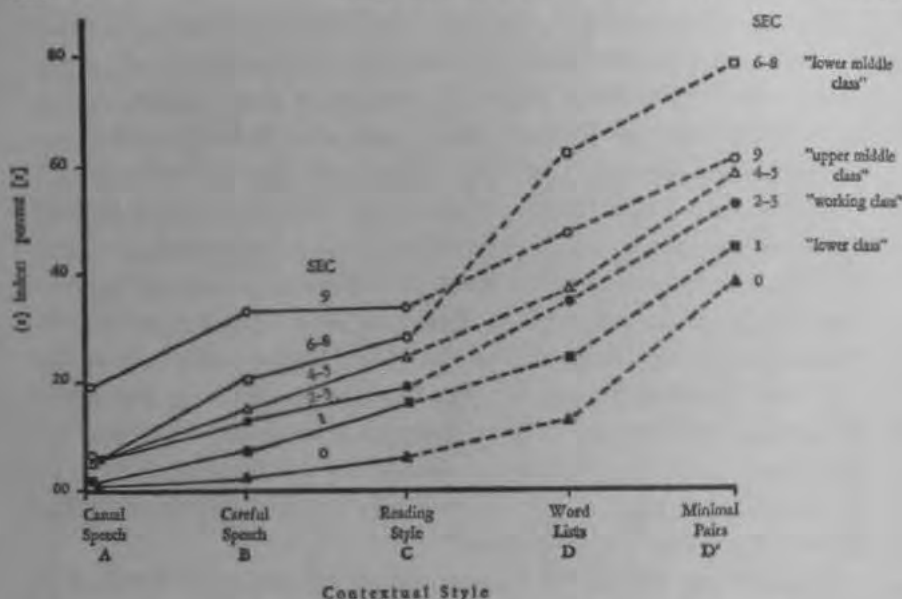
Figure 3. Style and class stratification of (r) for adult native New York City speakers.

English as a cluster of separate dialects which happen to be changing in parallel, or, ignoring socioeconomic differentiation, consider it as a single object characterized by massive free variation.[93] Either attitude, however, would deprive him of the most obvious *explanation* for the behavior of the majority—the fact that the change is originating in the highest ranking subgroup. And surely the behavior of each class in the several styles is not an indifferent switching between *random* styles: the character of (r) as a prestige feature is confirmed by the entire network of stylistic and social inequalities.

Finally, to underline the high level of structural organization in this diagram, we may consider what appears at first glance as a random irregularity of Figure 3: the "cross-over" of Class 6–8 in styles D and D'. In any one case, such a feature would remain an inexplicable deviation from the structural pattern. However, we observe the second highest status group switching ranks with the highest status group in formal style in two other cases of linguistic change in progress as

[93] This was in fact the alternative selected by several investigators of New York City speech in the 1940's and '50's (Labov 1966:35–38).

well.[84] This "hypercorrect" pattern also recurs in Levine and Crockett's independent study of r-pronunciation in North Carolina (1966: 223, cf. Table 7, "Education"). There is reason to believe that such hypercorrection is an important mechanism in the downward transmission of a prestige pattern and the completion of the linguistic change (Labov 1966b).

The Evaluation of Linguistic Variables. In Section 3.14 we considered the evaluation problem in relation to alternating codes or coexistent systems within a heterogeneous language structure. The evaluation of individual linguistic variables poses some special problems, but considerable progress has been made in their solution (Labov 1966:405 ff.). The "matched guises" presented to listeners must be controlled so as to differ only in the single linguistic variable under consideration.

The social evaluation of (r) in New York City has been studied in detail: the results indicate an extraordinary degree of agreement in subjective reactions to (r) as a [middle-class] prestige norm. But this agreement is characteristic of the younger age group only. For subjects over forty, there is considerable variation in subjective reactions; but *all* of the subjects under forty agreed in their [unconscious] positive evaluation of [r]. Comparison of Figure 4 with Figure 2 shows that this categorical change in evaluation coincides with the increase in the stratification of (r) discussed above. The change is more striking in the dimension of social evaluation than in the pattern of linguistic behavior.

In Figure 4, we are dealing with systematic evaluative reactions to linguistic features which the listener cannot consciously perceive. Such systematic evaluation is regularly associated with linguistic variables which show stylistic and social stratification. For some variables, the level of social awareness is so high that they are prominent topics in any discussion of speech. These linguistic "stereotypes" are not related to linguistic behavior in any one-to-one fashion; they are sharply focused on individual lexical items rather than on abstract categories. Correction in formal styles associated with such stereotypes is extreme

[84] This is the case with (eh) and (oh) discussed above, but not with (th) and (dh), which are not involved in change.
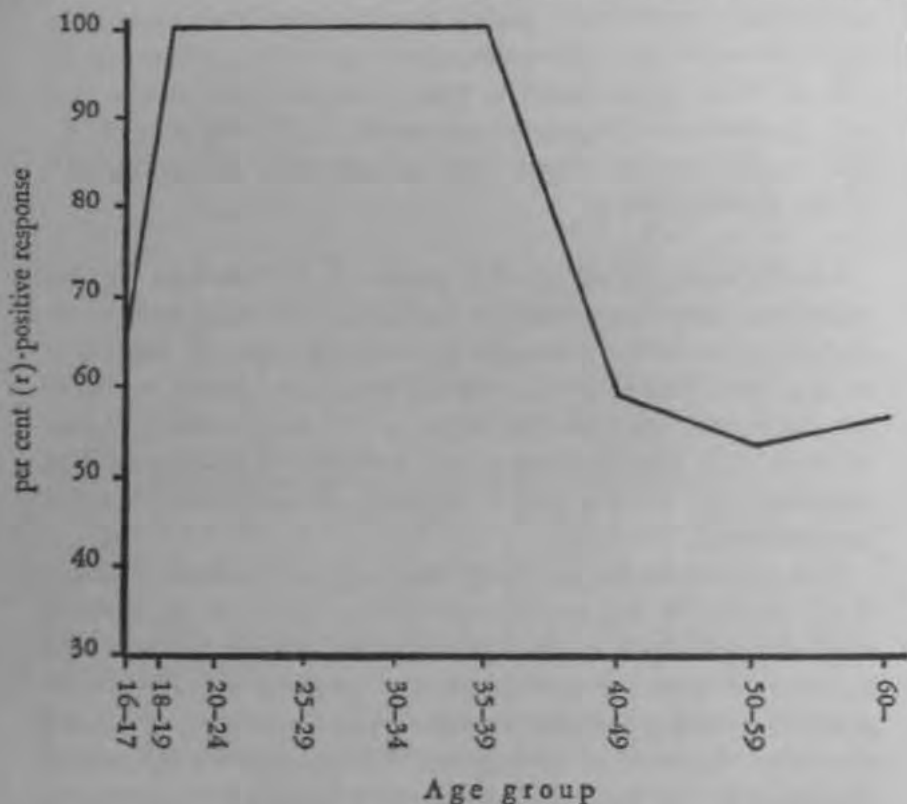
Figure 4. Subjective evaluation of (r) by age among native New York City speakers.

but bizarre in its distribution, and is accompanied by considerable psychological tension for most speakers: the results are the unsystematic and unstable set of contrasts characteristic of formal language. Thus the words *aunt* or *vase* inserted in a reading text are the cause of extraordinary vacillation and confusion;[65] heavily marked (oh) vowels in *office*, *chocolate*, and *coffee* are lowered irregularly to [ɒ] and thus contrast weakly with [a] and [ɔˑ]; speakers who use [əˑr] in *bird* ridicule the stereotype [ɔɪ] which they perceive in others' *boid*; many who use high percentages of lenis stops for interdental fricatives

[65] Thus one speaker resolves the confusion: "These large ones are my [vɑziz] but these small ones are my [veziz]."

are deeply hurt if they are told that they have trouble with their *dems* and *doses*.[66]

The investigation of such social perceptions provides a rich body of data on late stages of language change, although it does not reveal the more systematic aspects of linguistic evolution. Overt social correction is sporadic, since, when a linguistic variable acquires social significance, speakers substitute the prestige norm for the basic vernacular as a control in audio-monitoring. The disjunction between production and perception, as studied through self-evaluation tests (Labov 1966:455–480), provides one more route to the analysis of change in progress. The study of overt statements about language yields many insights into the social factors which bear upon language change, and into the sources of irregularity which disturb the course of sound change; but to relate these data to the evolution of the basic vernacular is a matter which requires a detailed knowledge of the speech community and considerable sociolinguistic sophistication.

3.4. EMPIRICAL PRINCIPLES FOR THE THEORY OF LANGUAGE CHANGE

In the third part of this paper we have presented certain empirical findings which have significance for the theory of language change, and also certain conclusions drawn from these data as to the minimal complexity of a theory of linguistic structure which can account for this change. We are also concerned with methods for relating the concepts and statements of a theory of change to empirical evidence— that is, evidence based on rules for intersubjective agreement. In this final section, we will summarize certain principles concerning the empirical foundations for the theory of change; we will organize the discussion, as we have done previously, in terms of the problems to be solved.

The Constraints Problem. We have indicated that one possible goal of a theory of change is to determine the set of possible changes and possible conditions for change; to the extent that such a program springs from a close study of changes in progress, we believe that progress can be made. One such general constraint appears to apply

[66] See Whyte (1943:346) for a dramatic example.

to areas where a two-phoneme system is in contact with a merged one-phoneme system: we argue that, except under certain special conditions, the direction of change will be in favor of the one-phoneme system. We observe, as another example, many cases of correlated chain shifts where peripheral [tense] vowels rise, but none in the reverse direction.

We can also note that not every combination of linguistic and social factors has been observed in studies to date, nor has every possible combination of linguistic variables been observed. In no case, for example, have we found a variable which originated as a social stereotype with stylistic stratification and at a later stage emerged as a social variable without stylistic shift.

The Transition Problem. All of the changes submitted to careful empirical scrutiny so far have shown continuous distribution through successive age levels of the population. Between any two observed stages of a change in progress, one would normally attempt to discover the intervening stage which defines the path by which Structure *A* evolved into Structure *B*.

We find that the theory of language change can learn more from so-called transitional dialects than from "core" dialects (Herzog 1965. 1–5). Indeed, it stands to gain by considering every dialect as transitional. Consequently, there is no need to distinguish between intra-dialectal change and mixture of [jointly available] dialects.

By viewing some subsystems or variables as marked by the feature archaic/innovating, the theory of language can observe language change as it takes place. From observation *in vivo* it can learn things about language change that are simply lost in the monuments of the past.

This transition or transfer of features from one speaker to another appears to take place through the medium of bidialectal speakers, or more generally, speakers with heterogeneous systems characterized by orderly differentiation. Change takes place (1) as a speaker learns an alternate form, (2) during the time that the two forms exist in contact within his competence, and (3) when one of the forms becomes obsolete. The transfer seems to take place between peer groups of slightly differing age levels; all the empirical evidence gathered to date indicates that children do not preserve the dialect characteristics of their parents,

but rather those of the peer group which dominates their preadolescent years.

The Embedding Problem. There can be little disagreement among linguists that language changes being investigated must be viewed as embedded in the linguistic system as a whole. The problem of providing sound empirical foundations for the theory of change revolves about several questions on the nature and extent of this embedding.

(a) *Embedding in the linguistic structure.* If the theory of linguistic evolution is to avoid notorious dialectic mysteries, the linguistic structure in which the changing features are located must be enlarged beyond the idiolect. The model of language envisaged here has (1) discrete, coexistent layers, defined by strict co-occurrence, which are functionally differentiated and jointly available to a speech community, and (2) intrinsic variables, defined by covariation with linguistic and extralinguistic elements. The linguistic change itself is rarely a movement of one entire system into another. Instead we find that a limited set of variables in one system shift their modal values gradually from one pole to another. The variants of the variables may be *continuous* or *discrete*; in either case, the variable itself has a continuous range of values, since it includes the frequency of occurrence of individual variants in extended speech. The concept of a variable as a structural element makes it unnecessary to view fluctuations in use as external to the system, for control of such variation is a part of the linguistic competence of members of the speech community.

(b) *Embedding in the social structure.* The changing linguistic structure is itself embedded in the larger context of the speech community, in such a way that social and geographic variations are intrinsic elements of the structure. In the explanation of linguistic change, it may be argued that social factors bear upon the system as a whole; but social significance is not equally distributed over all elements of the system, nor are all aspects of the system equally marked by regional variation. In the development of language change, we find linguistic structures embedded unevenly in the social structure; and in the earliest and latest stages of a change, there may be very little correlation with social factors. Thus it is not so much the task of the lin-

guist to demonstrate the social motivation of a change as to determine the degree of social correlation which exists, and show how it bears upon the abstract linguistic system.

The Evaluation Problem. The theory of language change must establish empirically the subjective correlates of the several layers and variables in a heterogeneous structure. Such subjective correlates of evaluations cannot be deduced from the place of the variables within the linguistic structure. Furthermore, the level of social awareness is a major property of linguistic change which must be determined directly. Subjective correlates of change are more categorical in nature than the changing patterns of behavior: their investigation deepens our understanding of the ways in which discrete categorization is imposed upon the continuous process of change.

The Actuation Problem. The over-all process of linguistic change may involve stimuli and constraints both from society and from the structure of language. The difficulty of the actuation riddle is evident from the number of factors which influence change: it is likely that all explanations to be advanced in the near future will be after the fact. If we seriously consider the proposition that linguistic change is change in social behavior, then we should not be surprised that predictive hypotheses are not readily available, for this is a problem common to all studies of social behavior (Neurath 1944). Such considerations should not prevent us from examining as many cases as we can in enough detail to answer the problems raised above, and put these answers together into an over-all view of the process of change. One such proposal for the ways in which social factors bear upon linguistic features in a cyclical mechanism is based upon repeated patterns observed in a few well-studied cases (Labov 1965).

It is suggested that a linguistic change begins when one of the many features characteristic of speech variation spreads throughout a specific subgroup of the speech community. This linguistic feature then assumes a certain social significance—symbolizing the social values associated with that group (cf. Sturtevant 1947:81 ff.). Because the linguistic change is embedded in the linguistic structure, it is gradually generalized to other elements of the system. Such generaliza-

tion is far from instantaneous, and change in the social structure of the community normally intervenes before the process is completed. New groups enter the speech community and reinterpret the on-going linguistic change in such a way that one of the secondary changes becomes primary. From such alternations of linguistic and social change proceed the extraordinary complexity of the sociolinguistic structures found in recent studies. The advancement of the linguistic change to completion may be accompanied by a rise in the level of social awareness of the change and the establishment of a social stereotype. Eventually, the completion of the change and the shift of the variable to the status of a constant is accompanied by the loss of whatever social significance the feature possessed. The high degree of regularity which sound change displays is the product of such loss of significance in the alternations involved, and the selection of one of the alternants as a constant.

3.5. SOME GENERAL PRINCIPLES FOR THE STUDY OF
LANGUAGE CHANGE

Whether or not the particular mechanism of language changes suggested above holds true in most cases is not the important issue here. The aims of this paper are to put forward certain proposals concerning the empirical foundations of a theory of change. We have presented some empirical findings which such a theory must account for, and conclusions drawn from these findings as to the minimal complexity of linguistic structure involved; we are very much concerned with the methods for relating the theory of change to empirical evidence in ways that will lead to intersubjective agreement. Certain general statements about the nature of language change may be taken as central to our thinking on these problems:

1. Linguistic change is not to be identified with random drift proceeding from inherent variation in speech. Linguistic change begins when the generalization of a particular alternation in a given subgroup of the speech community assumes direction and takes on the character of orderly differentiation.

2. The association between structure and homogeneity is an illusion. Linguistic structure includes the orderly differentiation of speakers

and styles through rules which govern variation in the speech community; native command of the language includes the control of such heterogeneous structures.

3. Not all variability and heterogeneity in language structure involves change; but all change involves variability and heterogeneity.

4. The generalization of linguistic change throughout linguistic structure is neither uniform nor instantaneous; it involves the covariation of associated changes over substantial periods of time, and is reflected in the diffusion of isoglosses over areas of geographical space.

5. The grammars in which linguistic change occurs are grammars of the speech community. Because the variable structures contained in language are determined by social functions, idiolects do not provide the basis for self-contained or internally consistent grammars.

6. Linguistic change is transmitted within the community as a whole; it is not confined to discrete steps within the family. Whatever discontinuities are found in linguistic change are the products of specific discontinuities within the community, rather than inevitable products of the generational gap between parent and child.

7. Linguistic and social factors are closely interrelated in the development of language change. Explanations which are confined to one or the other aspect, no matter how well constructed, will fail to account for the rich body of regularities that can be observed in empirical studies of language behavior.

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