Research Guide on Language Change

Trends in Linguistics
Studies and Monographs 48

Editor
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Preface

Habent sua fata libelli.

This volume was planned in the early 1980s by the Subcommittee on Language Change of the Modern Language Association of America at the initiative of a number of historical linguists who wanted to provide a general guide for the student and scholar to the various aspects of language change. A group of prospective participants was contacted and manuscripts were collected by Edgar C. Polomé to whom the task of editing the volume had been entrusted. In 1983, an eye operation paralyzed the activity of the editor for several months, and with the pressure of other commitments and requested changes in the editing procedure, continued work on the collected contributions was considerably delayed. When the editorial work could finally be restarted in 1987 with the active help of Linda Giles, the need to update the papers appeared evident. This was done in the course of the academic year 1987–1988, and it is this updated version which is now presented here, providing a wide coverage of the various well-established aspects of language change and the relevant methodology.

We wish to thank all the authors most heartily for their extreme patience and most responsive collaboration with us in revising and completing this work. We also express our gratitude to Cynthia Sanches, Eric Dwyer, and Kathleen Linnes for their work in retyping manuscripts and assistance with data verification during the editing process. Personally, I would like to extend my deepest thanks to Linda Giles without whose gracious help, scrupulous attention, and indefatigable work the careful editing of this work would have been impossible.

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I. Introduction

Language change and the Saussurean dichotomy: Diachrony versus synchrony

Edgar C. Polomé

It is obviously a truism to state that ever since man directed his attention to language, he was puzzled by the problem of its diversification — hence the story of the tower of Babel. As the idea of the original unicity of language prevailed for centuries, a sound reaction against referring every term to an Hebraic etymon only came in the eighteenth century, when Turgot (1756: 103) scoffed at those who would compare the Greek perfect passive participle ἡμμένος of ἅπτω 'fasten to' with Hebr. *hemmen* 'arranged, joined'. Change was ascribed to "an internal necessity", and it was assumed to be regular, respecting the internal organization of the language (Ducrot—Todorov 1979: 7). Scholars in the nineteenth century believed, with Jacob Grimm, that language attains its highest degree of development at the eve of the historical period when its process of decay starts. As the Old Indic religious language was assumed to be still very close to the protolanguage, this view was illustrated by referring to the richness in forms (*Formenfülle*) of Vedic, contrasted with the deflection of Modern English. As for the elaboration of the "ideal" protolanguage, August Schleicher assumed that it went successively through three phases characterized by 1) isolated monosyllables, 2) agglutination, 3) inflections (cf. Arens 1969: 253). The positivistic approach of the Neogrammarians changed these views in the last quarter of the nineteenth century by stressing that linguistic change could not simply be described: its causes had to be explained. They concentrated on sound change, as the "phonetic laws" operated mechanically — exceptions being accounted for by hitherto undiscovered "laws" or by psycholog-

ical explanations, especially the association of ideas involved in the analogical process.

Having provided a magistral synthesis of the Neogrammarian achievements in the field of Indo-European vocalism and laid the foundation for the "laryngeal theory" at the age of 21 with the *Mémoire sur le système primitif des voyelles dans les langues indo-européennes* (Leipzig 1879), Ferdinand de Saussure felt in his later career the need to question the time-honored principle that the former state of the language is the underlying grammatical structure of the new state, and that the phonetic laws, linked with the needs of communication, are responsible for the progressive breaking up of the system. It is indeed assumed that a change like an accent shift placing a strong dynamic accent on the root syllable will entail the progressive loss of the case and verbal endings, and this deflection will, in turn, promote a change from a synthetic (i. e., inflectional) to an analytical syntactic pattern (with "function words" like prepositions or auxiliaries), as it can be exemplified, for example, in the diachronic development of Germanic or Indo-Aryan. After probing into the nature of language in the first part of his *Cours de linguistique générale* (Paris 1916), Saussure came to the conclusion that there was an inner duality in linguistics illustrated by the strictly synchronic approach of the *Port Royal Grammar* describing the state of French under Louis XIV and the method of comparative Indo-European philology attempting to reconstruct the past. For him (1959: 83), "the opposition between the two viewpoints ... is absolute and allows no compromise". To demonstrate this, he resorts to the case of the German plural with umlaut:

(a) in Old High German, masculine -*i*-stems like *gast* 'guest' would form their plural in -*i*, but this -*i* would produce umlaut of the *a* of the stem, hence: **gasti* → *gesti*, later *geste* → modern German *Gäste*;

(b) in modern German, a set of terms forms its plural on the pattern of *Gast, Gäste*.

Synchronically, the alternation *a : ä* in Modern German is simply a mechanism to indicate the plural. On the other hand, in Old High German, the change of *a* to *e* under the influence of the *i* of the following syllable is a phonological process which has nothing to do with the pluralization of nouns, since the same umlaut occurs, e. g., in the 3rd person singular present in *tragit* → *tregit* → Mod. German *trägt*. Obviously, diachronic facts have not changed the system: "modification does not affect the arrangement but rather its elements" (1959: 84).

Having strongly stressed the dichotomy between synchronic and diachronic linguistics, Saussure remarks at the very beginning of the third part of his *Cours* (1959: 140): "What diachronic linguistics studies ... is relations between successive terms that are substituted for each other in time." Broadening this statement to mean "successive grammars" one comes indeed very close to the way generative grammar considered language change. However, one should avoid hasty generalization. Robert King (1969: 85) presents the generative model of language change through three generations schematically as in Figure 1.

Generation 1   Grammar ─────────► Speech Output

Generation 2   Language Acquisition Device ◄───

                              Optional Grammar ─────────► Speech Output A

                                          +

                                   Innovations

                              Adult Grammar ─────────► Speech Output B

Generation 3   Language Acquisition Device ◄───
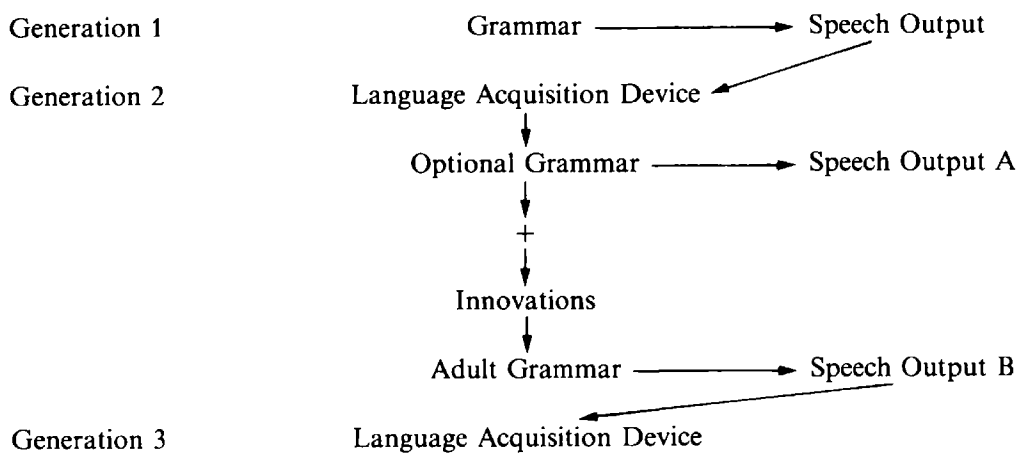
Figure 1. King's model of language change

Saussure (1959: 149) remains rather skeptical about the transmission of "faulty pronunciations" through children, but what he has in mind are sporadic changes, and not the process of rule simplification implied by King's matrix.

Saussure (1959: 98) states explicitly that "each change is launched by a certain number of individuals before it is accepted for general use", but not all innovations receive this recognition! As long as they remain individual, the linguist may safely ignore them, since they only actually enter his field of investigation after adoption by the whole community. Therefore, Winfred Lehmann (1968: 15) considers that, for Saussure, the "speaker" is ineffective in initiating and even controlling language change. A typical illustration of this view is Saussure's analysis of the process of *analogy* as "the chance product of an isolated speaker", when he shows that it actually depends on the linguistic mechanism linking productive forms stored by the language and "arranged according to their syntagmatic and associative relations". This

contrasts clearly with the position of Jerzy Kuryłowicz (1966: 171), who stresses that the innovation is due to causes external to the linguistic system and that its start and spread depend on the social context.[1]

For Saussure, language is a social institution that has its own internal arrangement. He frequently compares the latter to a game of chess: in language, one moves from one synchronic state to the next, as one moves only one piece at a time on the chessboard. But, just as one move can change the whole game, and sometimes the chessplayer may not even foresee all its effects, one change in language may have a repercussion throughout the whole system. However, "each move is absolutely distinct from the preceding and the subsequent equilibrium" (1959: 89): whichever way a state in the game has been reached is irrelevant, and someone looking at the chessboard at that very moment can assess the situation without having to know what happened just seconds before. Similarly, as speaking operates on a synchronic level, the changes that affected successive states of the language are irrelevant to it. Thus, the reference to *parole* (speaking) stresses the radical difference between diachrony and synchrony in Saussure's mind.

He constantly points out that any individual creation in language is doomed unless it is taken over by the speech community, at which time the individual loses all control over it. Thus, "a language never exists apart from the social fact" (1959: 77), and while society insures its continuity, language is, like all social institutions, subject to change in time. But when it comes to specifying the external social forces responsible for change in language, Saussure doubts the effects of political and economic upheaval on linguistic stability: for him (1959: 150), the changes that Latin underwent during the turbulent period of the Germanic invasions are "self-generated", and not ascribable to external conditions. In the light of Pulgram's views (1958: 317−23) on spoken and written Latin, Saussure is obviously right in assuming that the relative stability of classical Latin is due to external factors, such as the cultural influence of the Roman intelligentsia on the imperial court, on the administration, on law and education. With the breakdown of this influence and the accession of different social strata to power, the spoken language which had developed unhampered by external factors since the early Republic became prominent and was ultimately the source of written "Romanic" and of the "Romanic" vernaculars — an evolution which is clearly illustrated by the texts presented by Ernst Pulgram (1978). Saussure's doubts about the belief

that turbulent periods in the history of a nation precipitate linguistic evolution, are accordingly justified, and they would deserve further investigation, together with his statement (1959: 150) that what actually takes place is a return of the language to its "free" (= uncontrolled) state and its natural evolution.

An important point that Saussure makes in this context is that "it is impossible to cite a single period — even among those where language is in a deceptive state of immobility — that has witnessed no phonetic changes" (1959: 151). Language is in constant flux through time, modifying and renewing itself. It is, therefore, rather unlikely that the language pattern of ancient Europe would have remained practically unchanged between 4500 B. C. and 1000 B. C., as it has been assumed to accommodate some archeologically based diffusion theory (cf. Renfrew 1987: 164). But it has long been recognized that geographical spread favors diversification through time: Saussure himself devotes the fourth part of his *Cours* to this topic and recognizes (1959: 208) that, within a unilingual coherent whole, geographical continuity goes together with perpetual differentiation, each innovation spreading over its particular area. The importance of linguistic geography for historical linguistics has indeed been abundantly illustrated since the formulation of the "wave theory" by Johannes Schmidt in 1872 (cf., e. g., Anttila 1972: 304—09; Bynon 1977; 192—95). However, as Lehmann (1973: 137) rightly points out, the study of dialect geography has shown that a bidimensional model, reflecting the areal subdivisions of a language and the social and occupational groups speaking it, is inadequate, even when supplemented by a time dimension.

The reason for the inadequacy of the models Saussure used is the awareness of the extreme complexity of the social conventions involved in language that sociolinguistic studies have given us. The pioneering work of William Labov (1966, 1972) has provided a new insight into the social stratification of language and, by a proper evaluation of the linguistic variables, it has broken through the Saussurean dichotomy of synchrony and diachrony to demonstrate the synchronic reflection of historical change[2] and analyze the mechanics of language change within its social setting. Apropros of the latter, Hans H. Hock (1986: 661) makes a very important observation: to the extent that the Labovian studies on analogical and syntactic change are focused on change in progress, they show a direct correlation between social factors and linguistic change — which, again, runs contrary to the

Saussurian contention that external events have only an indirect influence on the internal development of the language.

I would conclude, with Lehmann (1968: 19—20), that, while recent work clearly departs from Saussure's views and his sharp dichotomies, his achievements cannot be disregarded, and his challenge has definitely triggered responses providing a better insight into the problems of language change[3].

Notes

1. The problem of analogy has remained a disputed issue: the principles presented by Kuryłowicz in his 1949 article (1966) are summarized and supplemented in his book on *The inflectional categories of Indo-European* (Heidelberg: Carl Winter, 1964), 38—55. For a discussion of Kuryłowicz' views, cf. Anthony Arlotto's chapter on "Analogy" in his *Introduction to Historical Linguistics* (Boston: Houghton, Mifflin, 1972, 130—148). A different approach is presented by Witold Mańczak in his "Tendances générales des changements analogiques" (in *Lingua*, 7 [1958], 298—325, 387—420). On Kuryłowicz versus Mańczak, cf. Anttila 1977: 76—80.

   About the treatment of "analogy" in transformational grammar, cf. King 1969: 127—134; Anttila 1977: 87—110; Hock 1986: 238—279.

   About analogy in general, see Anttila 1977; Hock 1986: 167—279.
2. That synchronic grammar may recapitulate history was recognized by transformational-generative grammar, with the reservation that it might not do so in rule ordering, and that historical evidence should not be considered as directly relevant to the evaluation of synchronic grammars (cf. King 1969: 101—104).
3. Unless we subscribe to the rather extreme critical skepticism of Roger Lass in his challenging book *On explaining language change* (Cambridge Studies in Linguistics, vol. 27; Cambridge [U. K.]: Cambridge University Press, 1980)!

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Linguistic reconstruction:
The scope of historical and comparative linguistics

Werner Winter

As long as there has been a scientific study of language, there has been an interest on the part of linguists in two major aspects of the field. One may be expressed by the simple question: What is a language like? — the other, slightly more involved, by: What made a language like it is now?

The first question covers all kinds of synchrony-oriented investigations, no matter whether they are concerned with rather simple matters, for example, the inventory of certain words or parts of paradigms, or whether they involve a high degree of abstractness and sophistication, say, regarding preconditions or purposes of some facets of language use.

The second question is directed toward the history and prehistory of a language. Simple observation tells us (though usually in rather marginal contexts) that languages do not remain stable through time, but that they change. It is thus not trivial to ask the second question; and as was the case with synchronic investigation, diachronic study of language is very complex in itself.

First of all, there is the painstaking analysis of information derivable from attestations of the language under consideration that date from the past. Except for very recent data, all attestations will be through the medium of writing. The written record is never an exact rendering of its spoken counterpart; a writing system imposes limitations on what can be recorded of the properties of a language. Different writing systems are characterized by different degrees of appropriateness for the expression of features of spoken language; some of such features are almost always suppressed even in otherwise quite reliable tran-

scription systems, mainly because the competent speaker-reader can be expected to supply these features when converting written language back to its spoken form. No person studying the texts of a language of the past now can be a fully competent native speaker of this language. Historical study of a language thus requires, to be sure to varying degrees, an interpretation of the data even if one's immediate goal has been set fairly low — if, for instance, one is only concerned with the question: What may this written text have sounded like? The more demanding one's questions, the more difficult one's analysis of written texts becomes, for all texts from times past have one property in common: not only are they imperfect as renderings of actually spoken language, but beyond that, they are always incomplete as far as their coverage of relevant phenomena is concerned, and no gap in the pattern of information can be closed by resorting to the knowledge of a competent native speaker. Therefore, not only the interpretation of what is found in the texts requires the development of frequently rather elaborate hypotheses, but one's goal of discovering and evaluating the patterns of this early language can only be attained to the extent that one can come up with sound hypotheses concerning the unattested parts of these patterns. Direct observation is out of the question, and so is the use of an informant; instead one has to use one's own understanding of the properties observed to extrapolate assumptions about the unobserved and unobservable.

Work on data extracted from written documents of the past one may call historical linguistics in the narrow sense of the word. To start with, such work need not be, and often is not, diachronically oriented; like living languages, dead ones may be described and analyzed in their own right. But whenever a dead language can be connected with a living one, there will be a strong tendency to look at the two together, particularly if the dead one can be considered an antecedent of the living language. When this is done, diachronic analysis replaces its synchronic counterpart.

For generations of linguists, such diachronic analysis constituted the first and most important task of all scientific linguistic activity. The reason behind this attitude is to be seen in the conviction that to know the origins and the subsequent stages in the development of a phenomenon meant to understand and therefore to be able to explain it. The fact that for later generations there was a complete reversal of attitude need not concern us here — the reason being that since languages could be observed to be fully functioning here and now,

used most successfully by speakers totally unaware of any aspect of the earlier history of their language, there was obviously no need for bringing in history as a basis for explanation of linguistic phenomena, the more so as there were very many languages indeed without a written record of past stages in their development. By now, a certain *modus vivendi* has been reached: Both points of view are seen to have their own merit — the one helps elucidate problems the other cannot accommodate, and a judicious use of both approaches contributes much more than an obstinate fixation on one of the two could ever achieve.

Very early in the development of linguistics as a science, it was noticed that the domain of diachrony-oriented linguistics could be significantly, even dramatically, extended by studying not only the history of a language as attested in written documents, but its prehistory as well.

On first consideration such as step seems preposterous to propose and to take. Human speech is a most perishable commodity, and what evidence could there possibly be that would have preserved for us even mere traces of earlier properties of a language if we were to reach out beyond the realm of written materials?

However, there is one very important characteristic of human language that enables us to extend the domain of linguistic research to preliterate periods of the past. It can be stated as a general rule that the sign inventories of all natural languages are made up essentially of entities that are arbitrary, that is, only very rarely are properties of what is referred to in the world around us reflected in the form of linguistic signs (even where onomatopoeia and iconicity are involved one observes relatively strong variation from one language to the other). For a language to function as a means of communication, it is necessary that the basically arbitrary signs be conventionalized within each speech community — no speaker can be permitted to use only signs of his own choosing, or else he could not have partners who would understand him and whom he would understand.

If signs are arbitrary to start with, then agreement, complete or partial, between signs denoting the same feature or features of the universe in two or more different languages must be in need of an explanation. Apart from the relatively few cases of only partial arbitrariness, all instances of agreement or similarity require language-internal explanations.

There are essentially three types of explanation:

a) Similarity may be due to chance. Chance agreement does occur, but only very rarely; agreement of this type is virtually nonexistent between forms of some complexity. All in all, once we find a moderately high number of cases of complete or even only partial agreement, chance can be ruled out as a possible cause.

b) Similarity may be due to borrowing. If signs in two languages show a very high degree of similarity, and if the two languages are known to be, or to have recently been, in contact, borrowing is very likely to provide the explanation for the agreement. However, the more remote in time a borrowing process took place, the more difficult it becomes to prove borrowing, as all components of a language, whether they were borrowed or not, are affected by language change as time goes on, and differences between foreign and native elements tend to be obscured. Moreover, once reliable historical documents are no longer available, we cannot claim with ultimate assurance that the contact condition for borrowing really can be met.

c) Similarity of items in two or more languages compared, finally, may be due to these items deriving from a source which represents an earlier stage in the development of the languages compared, common to all of them. By showing such properties, languages are said to indicate genetic relationship, and the shared features are called inherited. The use of these terms from life sciences, as is so often the case with the adoption of an outside terminology, should be taken with a grain of salt: A parent language and a daughter language are not individual entities clearly set off against each other, but more or less arbitrarily selected subcontinua in an unbroken chain of development; features proper to this chain are not inherited, but among the features acquired by a learner in his language-acquisition process, these happen to be the ones that originated within the language (or language group) itself, while borrowed items are those taken over by means of an interlanguage acquisition process. (It goes without saying that the picture drawn here is slightly idealized — in most instances, borrowed items will have been part of the language of the learner for a while, so that what really happens here is also an intralanguage acquisition process.)

The task of the prehistorian of language is the conversion into a coherent picture of observations about similarity that can only be explained by positing an earlier common source for the "inherited" components of the languages under investigation. To do this, the

prehistorian has to go through a number of carefully planned and executed steps. His immediate goal is to replace the vague notion of similarity or loose partial agreement (a notion which forms the basis of work in lexicostatistics, glottochronology, and large-scale comparison aimed at establishing distant relationships among languages and language groups) by the much more stringent one of regular correspondence. Ideally, this will imply the discovery of regularities of the type "If, in language L_1, a feature X in an environment Z_1 is matched, in language L_2, by a feature Y in an analogous environment Z_2, then the agreement will recur in all other occurrences of the environmental class Z_1, Z_2". Any deviation from this regularity will have to be explained, either by determining deviant syntagmatic conditions or by proposing the influence of different paradigmatic conditions. Insistence on systematic, recurrent agreement not only does away with the impressionistic notion of similarity but can also afford to go much beyond this by allowing for regular correspondence between overtly divergent features, thus eliminating a recourse to similarity altogether except as a crude heuristic device.

The steps to be taken are as follows:

First is a collection of data. Incidences of recurrent agreement are brought together in lists, with all variation, however slight, given proper attention: the application of the principle "Only one set of correspondences per list" is likely to result in a fairly large number of such lists.

Next comes an evaluation of the status of the lists. A close study of the environments is carried out, aimed at determining whether some lists can be interpreted as syntagmatically conditioned variants of other lists, thereby reducing the number of lists of the first order. Ideally, a further reduction should take place by the identification of paradigmatically determined variants; as a rule, however, this task cannot be properly fulfilled, since that presupposes a rather thorough knowledge of the grammatical and semantic paradigms of the languages investigated. Usually, one has to be content with suspecting paradigmatic interference whenever syntagmatically conditioned deviation can be ruled out and when − an important practical consideration − a deviant correspondence chain is much less frequently attested than its suspected regular counterparts are.

Once the number of correspondence chains of the first order has been reduced to manageable proportions, each of the chains which are not thought to be variants of others, is labeled. Each label is to be

used only once. In principle, anything unambiguous would do for a label; thus it would be perfectly conceivable to use numbers for labels.

However, a very useful and generally accepted convention rules out this possibility: the label chosen is to be maximally similar to the parts of the chain it identifies. Therefore, if the features subsumed under a chain are sounds, the label should be the symbol for a sound. The sound denoted by the label must not deviate more than absolutely necessary from the sounds contained in the chain. Hence, if all sounds in a correspondence chain are identical, the sound indicated by the label must be identical with the sounds forming part of the chain. If the members of the chain are not identical, the sound denoted by the label should still be maximally similar to all of them, that is, the difference in terms of distinctive features between member sounds and label sound should be kept to the bare minimum. Obviously, with a mixed chain, there can be more than one choice for a label; however, a limitation arises immediately from the requirement that each label only be used once. Thus, the obligatory selection of labels made for unmixed chains will automatically reduce the number of alternatives available for mixed chains; a further reduction will become obvious as we proceed further in our discussion.

The requirements of biuniqueness and of maximal similarity of chain labels to the members of their respective chains are sufficient for an enumeration of all chains observed in an unambiguous and reasonable manner. However, were we to stop at this point, we would only have described regular correspondences existing between languages observable and observed; we would not have taken a very decisive step beyond achrony.

This step rests on the introduction of a most important assumption: If we think that the large-scale recurrent agreement we observe is the result of a common descent of the languages from which we have drawn our data from some (possibly quite remote) "parent language", then our labels could be taken to be representations of entities to be ascribed to this parent language.

This assumption immediately introduces another requirement to be made for the choice of our labels: Not only must they be used only once and be maximally similar to the members of the chains they stand for, but they must also be relatable to one another in such a way that they can be interpreted as parts of one reasonable pattern. The evaluation criteria, at this point, are typological: the more outlandish a pattern which has resulted from the labeling process, the more likely

it is that some of the labels need adjustment; which may mean that rejected alternative labelings now have to be reintroduced in order to make the overall pattern appear more natural.

With the last two steps we will have passed the threshold between "mere" comparison (which would be on a level with typological and with contrastive-confrontative comparison) and comparative reconstruction. We no longer merely describe what is there in one language in relation to what is found in its "sister languages", but we now make claims as to how what is there got to be there, and as to what was there at an earlier stage in the development of this language group that would help account for what is met in the directly observable languages. We have left the domain of formulas expressing regular correspondences and reached the point where achronic formulas are taken to be reflections of facts about a stage or stages of a language of times long past. This protolanguage is considered similar to observable languages in that it is endowed, through our reconstruction, with properties that seem fitting for natural languages, although we can never be quite sure that the results of our reconstruction will not lead us here or there beyond the confines of what is known from languages observed; still, information about languages in actual use provides an extremely helpful yardstick against which to measure our reconstructions.

We should not disregard the fact that there are several limitations to our ability to reconstruct languages of the past in all their richness.

First of all, the comparative method can be applied only if there is something to compare. Features of the protolanguage lost in the course of later developments in all daughter languages are irretrievable in this context; even where we find an isolated phenomenon without a match in the related languages, we cannot apply comparative techniques, and we cannot, again only in the present context, determine whether it is a lone survivor of the past, a recent development in this particular language, or a borrowing from a source not identified − the latter two possibilities having no bearing upon the reconstruction of the protolanguage. It is in some ways even more troublesome, when we find features attested only in languages of which we have reason to suspect that they were spoken in adjacent territories at some time between the periods of the protolanguage and of the observed languages, so that we cannot be sure whether the features shared were inherited or were taken over by intrafamily borrowing. Thus, ideally, only widely attested forms from languages nonadjacent to one another

should be used as a basis for reconstruction. In the case of Proto-Indo-European, no major difficulties arise from the application of this principle as long as we are interested in the reconstruction of the phonological system and paradigmatic morphology of late Proto-Indo-European (although even in the realm of paradigmatic morphology, different interpretations of the sets of facts are possible), but what can be reconstructed of the lexicon, rich as it appears to be, seems to be only a sadly deficient fragment of the Proto-Indo-European vocabulary. This state of affairs is, of course, not surprising at all, since we know from the closer inspection of languages with a long written tradition the extent to which the vocabulary of a language is subject to change, and how much of an old vocabulary is affected not only by morphological reshaping, but also by outright loss.

It is therefore not unusual that our endeavors in comparative reconstruction end up with highly incomplete results. Worse, time and again we find ourselves confronted with findings that contradict each other. At this point, the question becomes important: Is it possible to transcend the limits of comparative reconstruction? Or, to put it differently: Are there ways to reconstruct aspects of earlier stages of a language or a language group other than comparative reconstruction?

Areas of applicability for such reconstruction can easily be named: There are observable language isolates, that is, languages with no known genetically related counterpart. There are languages too distantly related with one another to permit a normal application of the comparative method. Finally, in applying the comparative method we will sooner or later reach an impasse where the result of our reconstruction becomes an isolate itself; it would be highly desirable to be able to extend reconstruction further, both in time depth and by the elimination of conflicting results of comparative reconstruction.

Comparative reconstruction, given the proper data, is easily enough performed whenever either no change has occurred or the change has been along regular lines; deviations caused by syntagmatic conditioning can also be fairly well accommodated. Changes without regular occurrence, such as paradigmatically conditioned ones ("analogical" changes) as well as largely abrupt changes, such as dissimilation or metathesis, permit a comparative reconstruction only in terms of probability — such and such an item is unlikely to show a regular development; it is likely that the form attested was influenced by such and such a property of the language in which the change occurred. This type of argumentation is possible whenever information about

the regular development to be expected can be extracted from the comparison with related languages.

Reconstruction within the bounds of one language is possible both by internal comparison and by interpretation of paradigmatic data.

The technique mentioned first is constantly applied in the production of grammars and dictionaries. Once the regularities have been discovered in related paradigms, there is generally little reluctance about projecting attested features into gaps in surviving paradigms. Neither the complete paradigm of Latin *amāre* 'to love' nor that of Greek *paideúein* 'to educate' can be lifted from surviving Latin and Greek texts; still, there is generally no objection to the use of the filled-up paradigms as standard parts of elementary Latin and Greek grammars. The reasoning implied can probably be stated as follows: "The forms attested make Latin *amāre* and Greek *paideúein* seem perfectly regular verbs; it is therefore extremely likely that the forms missing should agree with actually attested forms from parallel regular paradigms".

There are, of course, limits to this type of projection. Once attested forms cannot be unambiguously assigned to just one paradigm, all claims about missing forms can only be made with proper caution, indicating either that a reconstructed form is only one of several possible ones or, to preferably, listing alternatives explicitly.

In other cases, internal properties of paradigms give rise to hypotheses about their earlier shape. The following examples will illustrate this point.

In present-day German, two nominal suffixes correspond to English *-hood*, namely, German *-heit* and *-keit*; both form abstract nouns from adjectives. German *-keit* has a more limited distribution than *-heit*: *-keit* is found after adjectives ending in *-bar, -ig, -lich, -sam* (plus some in *-el* and *-er*); *-heit* is used elsewhere. The identity of function and the similarity of form make it tempting to propose that *-heit* and *-keit* are just variants of the same suffix, with *-keit* being the conditioned form. A change from *-h-* to *-k-* is not regular; however, an assimilatory process is conceivable. There is only one environment for *-keit* in which assimilation could have taken place, namely in the position after the suffix *-ig* /-ik/. *-keit* would then have spread to other environments so that it is now no longer a phonologically conditioned variant, but one that requires an enumeration of conditioning morphological elements. The only fault to be found with the above hypothesis is that it has been misdated: the development posited did not occur in Modern High

German as assumed, but in Middle High German, and even the spread of -*keit* is to be assigned to this period.

While in the case of German -*heit* : -*keit* recent variation can be reduced to older identity, another example will show that assumptions can also be made about a state of affairs prior to present uniformity. Taking again Modern German data, we find that the stems of strong preterite forms are identical in singular and plural, but that they show two types of vocalism — either with an -*a*- or without it. We thus find *er sang* : *sie sangen* 'he sang : they sang', *er ritt* : *sie ritten* 'he rode : they rode'. In some cases both -*a*- and non--*a*- vocalism are found with the same verb: *er ward* : *sie warden* :: *er wurde* : *sie wurden* 'he became : they became' (*ward* : *warden* is more archaic and no longer used in everyday language). An inspection of the data shows that, while it seems likely that at an earlier stage in the development of German there was an alternation between -*a*- and non--*a*- stem in the strong past tense, details of the distribution can no longer be easily determined. However, some archaisms still in use provide help: While 'he sang' is always *er sang*, we find for 'they sang' a variant of *sie sangen* in a rhymed proverbial saying *Wie die Alten sungen, so zwitschern die Jungen* 'As the old ones used to sing, so the young ones twitter'. Protected by the rhyme, *sungen* could survive longer than in normal speech; its survival gives us a chance to posit -*a*--less forms as belonging originally to the plural, while the domain of -*a*- forms was the singular of the strong preterite. To prove this state of affairs, it would have been much easier to consult older Germanic languages and to work through comparative reconstruction; the present argument merely serves to show that the absence of older outside data would not have made an internal reconstruction impossible.

When dealing with the earliest proto-language reconstructed for an entire language family (or, for that matter, with any other language isolate), internal reconstruction is the only means enabling us to penetrate into a more remote past (typological evidence can at best be suggestive). By positing earlier patterns, we, firstly, try to resolve difficulties arising from conflicting reconstructions (as, for instance, when Indo-European words for 'fire' showed forms ending either in -*r* or in -*n*, a "heteroclitic" *r/n* stem was reconstructed long before actual evidence for such an alternation in this word became available in Hittite). Secondly, we attempt to simplify our statements by subsuming overtly differing phenomena under one common formula which may or may not require positing directly unattested elements condi-

tioning the difference. A prime example of the latter approach is de Saussure's proposal to reinterpret Proto-Indo-European full-grade long vowels as results of the coalescence of a nuclear short vowel with a following lengthening consonant subsequently lost. This analysis had the tremendous advantage that now all subtypes of ablaut in Proto-Indo-European could be treated alike and that canonical forms of roots could be set up; along with this, however, went the disadvantage that the Proto-Indo-European system of vowels apparently had to be reduced in an unreasonable way.

The example mentioned last shows some of the problems which internal reconstruction of otherwise inaccessible stages of languages has to face: A hypothesis is introduced because it provides for a more reasonable pattern than that found in a language isolate either observed or reconstructed. The hypothesis can only be evaluated in terms of what it achieves — greater simplicity, greater consistency, greater naturalness of patterns than those that provided the input for positing the hypothesis. Outside confirmation can only be found where internal reconstruction is applied to nonisolates. Hence, we will not be able to get beyond the point where acceptance or rejection of an internal reconstruction depends on whether the hypothesis offered is considered plausible or not.

We have to conclude that, while important tasks of linguistic reconstruction can only be tackled by resorting to internal reconstruction, its results are often handicapped by the fact that there can be no proof from outside the original chain of argument. It is not surprising that important proposals made in this context, such as that of de Saussure concerning what later came to be called the laryngeals, keep meeting with much more scepticism than is usually reserved for findings of the comparative method. This cannot just be the result of a greater novelty of the method of internal reconstruction (because it is not new at all), but must be due to the different quality of the two types of reconstruction: Claims by comparativists can be falsified by data from observable languages, while claims of scholars applying the methods of internal reconstruction to language isolates can only be subjected to the test of plausibility, and this test is a highly subjective test indeed.

II. Aspects of Language Change

Synchronic manifestations of linguistic change

Franklin C. Southworth

Linguistic change manifests itself synchronically as variation, which can be defined in a somewhat general way as a relationship between different ways of expressing the "same" thing. Traditional historical linguistics recognized that many of the changes noted between successive (historically attested) linguistic stages appear to be the results of processes which can be observed in contemporary variation, e. g., /élm/ ~ /éləm/ (cf. Latin *Periclum* → *periculum*, Sturtevant 1961: 63, see also Lass 1980: 11). From the time of Saussure until the late 1960s, it was assumed by the great majority of linguists that the "omnipresence of ongoing change" (Bailey 1973: 33) which is observable in all real-life linguistic data must be ignored in synchronic description (apart from the possibility of allowing "free variation"; see Bloch — Trager 1942: 42).

This picture has now substantially changed, as a result of numerous careful studies of linguistic variation, most of which have been directly or indirectly inspired by the work of William Labov. (There are still, one should add, many linguists who work in the Saussurean framework.) The new framework, which Bailey (1973: 21 — 35) refers to as the "dynamic paradigm" in contrast to the older "static paradigm", assumes that variation is inherent in all natural languages, and that any attempt to eliminate it by restricting the field of observation to a limited class of speakers, or to limited contexts of speech, will lead inevitably to a failure to accomplish the linguist's goal of describing linguistic competence. The reason for this is that grammars of idiolects, or of single-style speakers, or of arbitrarily selected groups of individuals, can only be incomplete and unintelligible fragments of a larger whole which Labov calls the "grammar of the speech community"

(1972 a: 247). Such a grammar would presumably describe what Bailey (1973: 27) calls "panlectal competence" — a type of competence which allows for variation in one's own speech as well as between different individuals, and which enables speakers of different varieties of a language to interact with each other (Labov 1972 a: 223 – 237; Bailey 1973: 34 – 35; Weinreich – Labov – Herzog 1968: 187 – 188; Sankoff 1978).

The older view of linguistic description seeks to describe some ideal form of a language.[1] Its practitioners seek "good speakers" as sources of data, and are often obliged to fall back on themselves as informants. The new view, as described above, differs mainly in that it substitutes a more complex ideal. Bailey, for example, speaks of individuals' linguistic competence "asymptotically approaching" panlectal competence over their lifetimes (1973: 27). There can be no doubt, however, that this new and more complex view accounts for many phenomena which cannot be dealt with by the older one.

What does all this have to do with linguistic change? First, workers within the "dynamic paradigm" would claim that all change (or almost all change, at any rate) begins as variation, and the studies referred to below (e. g., those in Labov 1980) provide a strong justification for such a claim. Second, it is claimed that the processes and the causes of linguistic change can never be understood by looking at old documents or by reconstructing proto-languages, but only by observing ongoing change — i. e., contemporary linguistic variations, their trajectories, and their social concomitants.[2] Not unexpectedly, linguists working within this framework are developing descriptive models which can be applied both synchronically and diachronically — thus breaking another Saussurean taboo. Bailey, for example, claiming that *"the function of time in defining synchronic language patterns cannot be ignored in valid descriptions of language"* (1973: 32, his italics), proposes the development of "dynamic or time-based models" which will be "suitable for either historical or descriptive analysis" (1973: 31). Various applications of this proposal are represented by works cited below.

The following paragraphs contain brief discussions of the principal types of linguistic variation discussed in the literature, arranged in order of (the author's) convenience, with comments on their diachronic implications. Since most of these phenomena are treated in greater detail elsewhere in this guide, no attempt is made to provide exhaustive references.[3]

Dialect variation (including both regionally and socially differenti-ated varieties), particularly phonological variation, has been the main focus of recent sociolinguistic literature. (For recent summaries see Chambers — Trudgill 1980, Hudson 1980.) The recent development of quantitative methods for the study of phonological variation (Labov 1969, 1971, 1972 a: ch. 8, 1980; Cedergren — Sankoff 1974; Trudgill 1974 c; Anshen 1978) has been motivated by a concern for developing more precise models of linguistic change, as well as more accurate descriptive statements and a better understanding of synchronic vari-ation in all its aspects, structural as well as social. The approach to describing variation favored in most recent work involves adaptations of the generative model (Chomsky — Halle 1968), modified to include variable rules (Labov 1969; Bickerton 1971; Cedergren — Sankoff 1974; Bailey 1973; Bailey — Shuy 1973; Fasold — Shuy 1975; Labov 1971; Romaine 1981).[4]

Social dialects (as opposed to purely regional varieties) have occu-pied a central role in recent work on variation, the black English vernacular being perhaps the most-studied non-standard variety (see Labov et al. 1968; Labov 1972 b; Wolfram 1969; Fasold 1972; among others). Studies of this have been concerned with questions of change (see, for example, the studies of copula deletion by Stewart 1967, 1968; Labov 1969; Baugh 1980) as well as descriptive problems.

Labov (1980: 253 — 255), following Kroch (1978), points out that a number of studies of linguistic change in progress in Western societies have identified the upper working class or the lower middle class as the innovating groups in the process of phonological change. Studies of social dialects in South Asia indicate that upper and lower dialects innovate independently (Gumperz — Ferguson 1960, see also Labov 1972 a: 296 — 297). Ramanujan (1968) suggests that in societies char-acterized by caste divisions, high-caste groups may innovate as a means of avoiding lower-caste behavior. The opposite is also true: Lower-caste groups may innovate to avoid the accusation of imitating higher-caste speech (Southworth 1975: 192).[5]

Differences between male and female speech are mentioned by various authors (Labov 1972 a: 243, 301 — 304; Trudgill 1974 a; Cham-bers — Trudgill 1980: 71 — 74, 97 — 98; Lakoff 1973, 1975; Crosby — Nyquist 1977). Chambers and Trudgill note that women tend to use more forms conforming to the standard (in Western societies) than men do (1980: 97), which often puts them in an innovating role vis-á-vis men of their own class (Labov 1972 a: 303), though in traditional

societies they are usually found to be more conservative linguistically than men of their own group (Trudgill 1974a). (See also Steckler— Cooper 1980.)

Variation in the speech of different age groups in a population is significant as an indication of change — the presumption being that older speakers retain earlier linguistic stages, as a result of their usage (or rather, their capacity to restructure) having become "frozen" at some time in their teens (see Hockett 1950). Thus, variation in age is often referred to as "change in apparent time", as opposed to change in real time. The relationship between age and variation is complex, however; Labov notes, for example, that lower-middle-class speakers "even in middle age ... tend to adopt the latest prestige markers of the younger upper-middle-class speakers" in formal contexts (1972a: 134).[6] Payne (1980) makes it clear that the "freezing" of usage in youth is also complex, and does not happen all at once: She suggests the age of eight as the "cut-off point" for acquisition of phonetic conditioning rules of a new dialect (i. e., children moving into a new area after that age have a lower chance of completely acquiring the new patterns). Further, "children do not freely restructure and/or reorganize their grammars up to the age of 14 but ... do have the ability to add lower level rules" (1980: 175).[7]

Stylistic or register variation, often linked to a particular standard form of a language, is important not only for its own sake, but also for the ways in which it interacts with other kinds of social variation (Labov 1972a: chs. 4—5). Labov (1972a: ch. 3) has presented a method for obtaining reliable and consistent data on stylistic variation, in order to resolve what he calls the "observer's paradox" (1972a: 209— 210). More informal levels are generally found to be more innovative phonologically, lexically, and grammatically than the more formal levels, though the latter are often more innovative in the use of foreign words, using more of them and showing a better approximation to the original phonetics (like the elite dialects which they are based on — see above). By the same token, the "higher" (H) forms of diglossia are more conservative phonologically than the "lower" (L) forms (Ferguson 1959) — though from another point of view, those varieties which are consciously archaized or resurrected (e. g., Hebrew, classical Tamil, and to a lesser extent Hindi and Urdu) could be considered innovative.[8]

The focus in most of the works cited above is primarily on phonological change. Examples of discussions of the relevance of variation

in the study of syntactic change are to be found in Labov 1972c; Poplack 1980; Klein 1980; Steever et al. 1976; Ashby 1981; Naro 1981; Sankoff—Cedergren 1976.

The language of professional and other social subgroups — such as the military (Jonz 1975), criminals (Maurer 1940; Yenne 1927), drug addicts, prostitutes (James 1972), entertainers, college students (Morse 1927), teenagers, cowboys, sportsmen (Craven 1980), homosexuals (Farrell 1972) — is often distinguished by innovative uses of lexical items (most commonly imaginative metaphors and restrictions of the semantic range of a term), some of which find their way into the speech of other groups and occasionally into the standard dialects. (See also Belasco 1979; Chaika 1980.)

Change resulting from language contact may appear as differences between the speech of bilinguals (and those who interact with them) and others lacking such contact. Such changes are usually traceable if the language contact situation is known, and even in many cases where it is not — though Polomé (1981) cautions against the creation of "ghost languages" on the basis of presumed non-indigenous vocabulary in ancient languages. Weinreich's (1974) general discussion of language contact phenomena has not been superseded, though there have been many individual treatments of these phenomena, some of them with important theoretical implications.[9]

Pidgin-creole continua also offer evidence of change when basilectal and acrolectal forms are compared. The original changes which created the pidgins are still poorly understood, but the process of decreolization involving the interaction of basilect and acrolect has recently been studied in considerable detail (see discussion in Hancock, this volume).

Certain types of lexical relationships which are not usually considered under the heading "variation" do nevertheless conform to the definition given at the beginning of this section, and may therefore be briefly mentioned here. Under this heading can be included most metaphors, extensions of meaning, and narrowing of meanings (especially if they are "live" in the sense that both the old and the new senses coexist synchronically). Recent contributions to the study of metaphor include Bickerton 1969, Abraham 1975, Sapir—Crocker 1977, and Stross 1975. For other recent discussions of variation and semantic change, see Lehrer 1978, Kroskrity 1978, Kay 1975, Kristol 1980, Maurer 1980.

The types of variation mentioned above are part of the process of change. Another type of variation, long familiar to historical linguists,

involves synchronic irregularities which are a residue of earlier change
— such as *keep-kept, foot-feet,* as well as cases like *twitch-tweak, dwarf-
twerp,* etc. See Drinka (this volume) for further discussion of this type
of variation.

Notes

1. See Chomsky 1965: 3 for a very strong statement of this ideal.
2. See Labov 1972 a: ch. 9, 1980; Weinreich — Labov — Herzog 1968. For the signifi-
   cance of the study of variation to historical linguists, see Lass 1976; Anttila 1972:
   47 — 56, 191 — 193; Anderson 1973: 173 — 178.
3. It may be noted here that not all variations imply change; for example, some do
   not "go to completion" — see Nunberg 1980 for an example. Furthermore, many
   cases which fit the above definition of variation do not involve change in the sense
   that variant A replaced variant B in some context, e. g., paraphrases like *aunt* and
   father's/mother's sister. Even in a more restricted sense of change, in which A → B
   (i. e., A "organically becomes" B), certain cases would not fit, for example suppletive
   alternants. The broad definition of variation is used here merely to provide greater
   scope for this discussion.
4. See King 1969: 29 — 32, and Chambers — Trudgill 1980: 38 — 45 for discussions of
   the need to modify the earlier proposal of Weinreich (1954) for describing dialect
   variation.
5. For the effects of ethnic group membership on linguistic change, see Labov 1972 a
   (esp. chapters 1, 4, 5, 7, 8, and pages 296 — 298); Labov 1972 b; and Laferriere 1979.
6. See other entries under *age* in index (Labov 1972 a: 337); also Hudson 1980: 15 —
   18, 148 — 152; Chambers — Trudgill 1980: 89 — 90, 165 — 167, 179 — 180.
7. See also Labov's comments (1980: xviii) on Payne; and Hinton 1980.
8. See Ferguson — Gumperz 1960; Das Gupta — Gumperz 1968; and Southworth 1975
   for discussion of some relevant South Asian cases.
9. See, for example, Bokamba 1977; Caskey-Sirsons — Hickerson 1977; Midgett 1970;
   Thananjayarayasingham 1973; Ts'ou 1975; Southworth 1982: 28 — 33.

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Evidence of language change

Irmengard Rauch

I. Linguistic evidence: inviolable

Data are untouchable. No amount of manipulation can alter evidence; it is what it is. However, its use in linguistic inquiry has varying purposes. So, for example, Toon (1976: 71), while observing that "the study of the texts containing the earliest recorded English has a long and distinguished history", appears to lament that "interest in abstract grammars has shifted the attention of English historical linguists farther and farther from the manuscript data, the most interesting of which was relegated to the waste pile of free variation and optional rules". In point of fact, abstract grammarians do not set aside manuscript data, but by appealing to the principle of Ockham's Razor they tend to shift the focus from the data to the hypothesis relating to the evidence. Thus, for example, Botha (1981: 284) writes:

> Data that can be explained on the basis of a hypothesis therefore do not prove or demonstrate the truth of the hypothesis. However, the data explained by a hypothesis may be regarded as evidence for the hypothesis. Data constitute *evidence* for a hypothesis if these data — as presented in the minor premiss(es) of one or more arguments — indicate inconclusively that the hypothesis could possibly be true.

It is to be noted that the shift in focus does not contradict Cohen and Nagel's (1934: 394) statement that "no single proposition dealing with matters of fact is beyond every significant doubt. No proposition is so well supported by evidence that other evidence may not increase or decrease its probability", since the key word is "other" evidence not "additional" evidence.

Published research on language change accordingly exhibits both what we may label as philological (here meaning overtly textbound)

and abstract approaches. They are obviously not mutually exclusive; indeed, they are mutually dependent, and all researchers employ both approaches but in varying proportions. Botha (1981: 302) nicely defines internal linguistic evidence as consisting of "linguistic data about objects within the grammarian's linguistic reality. These linguistic data concern the utterances of a language and the linguistic competence underlying these utterances." The sources of internal linguistic evidence are then as follows: a) the native speaker's intuitions ("intuitive evidence"), b) the linguist's grammatical hypotheses ("hypothetical evidence"), and c) the grammarian's intuitions ("theoretical intuitive evidence") (1981: 302–303). The philological methodologists assume as *a priori* to their work these three types of evidence, while the abstract methodologists certainly recognize, for example in their "theoretical intuitive evidence", the link to the philological methodologist's category of grammarian's evidence or the filiation of reconstruction to evidence from hypotheses.

The linguistic hypothesis that "languages change", which underlies both methodologies, is deceptively simple and would lead one to believe that evidence in language change is by and large empirical and belongs to empirical or scientific theories. Burks (1963: 37) distinguishes between evidence which "usually relates to particular events not observed under controlled conditions and not repeatable" and "scientific evidence [which] generally relates to laws and repeatable events". Indeed, linguists have long been intent on discovering the "laws" of language change; the Neogrammarian hypothesis was a landmark breakthrough in this direction. However, what does the linguist do with Toon's (see above) "waste pile of free variation and optimal rules", in particular where semantics and pragmatics play dominant roles? Stephens (1968: 80) tells us that

> Statements that are framed, and offered, in such a form so that they can be related to evidence, we might term "empirical discourse" or "scientific discourse". Statements ... which cannot be related to evidence if one does not first grossly change or even distort them, we might term "humanistic discourse".

The linguist may, nevertheless, prefer to cling to his scientific bonds, citing perhaps Cohen and Nagel (1934: 394):

> Science is thus always ready to abandon a theory when the facts so demand. But the facts must really demand it. It is not unusual for a theory to be modified so that it may be retained in substance even though "facts" contradicted an earlier formulation of it. Scientific procedure is therefore a mixture of a willingness to change, and an obstinacy in holding on to theories apparently incompatible with facts.

What is eminently clear from any and all arguments is that despite one's predilection for method, whether philological or abstract, whether scientific or humanistic, the evidence always withstands individual conviction.

Language change is more readily traceable from evidence in past changes than from evidence of contemporary change. It follows, then, that more research is available on the analysis of historical change even though, as Samuels (1972: 4) notes: "for diachronic purposes our choice of evidence is far more limited: we must for the most part reconstruct from written records, bearing in mind continually the differences that exist today between spoken and written media." Moreover, evidence from historical times may contain errors which can hardly be considered with certainty as being falsifications. Swadesh (1971: 117) advises that

> No one can deny that it is best to have accurate information, but since the best of manmade reports may contain some errors, and since the data we have for languages are not always the best, the most that we can require is that scholars use the most reliable evidence available, interpreting it as they proceed. The only legitimate objection that can be made to a body of evidence is that the amount and type of incorrect information is so great or so serious as to invalidate it. Properly, we should distinguish among degrees of reliability: (a) occasional errors, (b) frequent errors, (c) mostly wrong information, (d) invented data. Situations (a) and (b) do not invalidate a case, especially if the scholar makes allowance for the possibility of errors. Even situation (c), if handled with sufficient care, may permit effective analysis; if the correct information is carefully separated from the incorrect information, it cannot be seriously contaminated. In fact, some of our most important linguistic information from remote times and places comes to us mixed with errors, and scholars recognize that it can be used.

Finally, our respect for the inviolability of linguistic evidence turns to the fact that such evidence has a life outside of linguistic method. Kahane, Kahane, and Ash (1979: 67—68) observe that

> Linguistic evidence is, in principle, threefold: (a) When other forms of evidence are scarce, particularly in prehistoric times, language (plus or minus archeological support) is often decisive for hypotheses of reconstruction ... (b) Frequently historical events and linguistic data supplement and confirm each other ... While the documentary sources reveal the circumstances of their transmission, an etymological, semantic, distributional analysis of the words provides many details which round out the chronicler's reports ... (c) Often linguistic data function as an analogue for historical events, stimulating a hypothesis even though absolute proof is unavailable ... Recorded linguistic behavior becomes, in short, a model for unrecorded social behavior.

Linguistic evidence of language change thus ultimately evidences history, society, and above all, man, in spite of himself.

2.1. Datable documents

"Indo-European is the family best known to us at present, because texts in the various languages have been most thoroughly studied, and because these texts extend through a period of almost four thousand years ..." (Lehmann 1978: 405). How well the Indo-European languages are documented can be understood by reading the Introduction "Der indogermanische Sprachstamm im Allgemeinen und seine Verzweigung" in Volume 1, Part 1 of Brugmann's *Grundriss der vergleichenden Grammatik der indogermanischen Sprachen*. Merritt Ruhlen has compiled information on documentation of the languages of world in his resourceful *Guide to the languages of the world*, which also provides information on genetic and typological structures of many languages. Valdis Zeps, building on Chapter IV of Bloomfield's *Language* (1933), "The languages of the world", has provided us with an updated *Languages of the world: A selective survey* (1966). From it we learn that the oldest documentation for Insular and Continental West Germanic dates from the eighth and ninth centuries, North Germanic inscriptions from the fourth century, and the East Germanic Bible translation of Wulfila from sixth century manuscripts (1966: 1—2). Celtic is documented in manuscripts from the eighth century, but the Ogam inscriptions date from much earlier (1966: 3). The Baltic family has late documentation, i. e., sixteenth century, while Slavic is recorded from the ninth century Old Church Slavonic (1966: 3—4). The Italic group is documented in the ancient Oscan-Umbrian inscriptions and Latin documentation predates 300 B. C. (Devoto 1968: 71). Albanian has late documentation, i. e., fifteenth century (Baldi 1983: 87). Hellenic documentation is found in inscriptions from the seventh century B. C., the Homeric poems 800 B. C., and Mycenean Linear B of 1450 B. C. (Zeps 1966: 4—5). Armenian documents date from the fifth century A. D., Iranian Old Persian rock inscriptions from the sixth to the fourth centuries, and Avestan sacred texts from 600 B. C. (1966: 5). In the Indic family the *Rig-Veda* dates from 1200—1000 B. C. Tocharian is documented in the seventh century A. D., while Anatolian yields cuneiform tablets from the eighteenth century B. C. (Neu 1974: 2). Gray (1939: 297) identifies as the oldest documented languages Sumerian 4000 B. C., Egyptian 3500 B. C., Semitic 2800 B. C., and Chinese 1500 B. C. On the other hand he notes that documentation for Australian, Malayo-Polynesian, African, and American Indian is found only in the past few centuries (1939: 298).

A study such as Cowgill's "A search for universals in Indo-European diachronic morphology" (1963) is based on datable documents: Greenberg's (1960) evidence from Classical Sanskrit, pre-fourteenth century A. D. *Hitopadeśa*; Alfred's Old English *Boethius*, c. 900 A. D.; Modern English *New Yorker*, 1952; Modern Persian, 1889. To these four Cowgill adds evidence from the *Rig-Veda*, 1200 – 1000 B. C.; Early Middle Indic Aśoka's "Rock Edicts", third century B. C.; Bengali of 1862, Kaliprasanna Sinha's *Hutom penchār nakshā*; Old Persian of Darius I's Bisotun inscription, the last two decades of the sixth century B. C.; cuneiform Hittite of the thirteenth century B. C. in *The apology of Hattusilis*; Homeric Greek in the eighth century B. C. *Iliad*; New Testament Greek of the first century A. D. Luke; Modern Greek of the 1888 *Khamena logia*; the Gothic Bible of the fourth century Visigothic Bishop Wulfila, transmitted in the sixth-century Ostrogothic *Codex Argenteus*; Old Church Slavonic of the ninth century A. D. Penzl (1970) investigates the phonology of Old High German via the datable Old High German *Isidor* of 800, *Otfrid* of 860, *Tatian* of 830, *Notker* of 1000, *Exhortatio ad plebem christianam* of 850, and the *Otloh's Gebet* of 1070. Dietrich (1973) traces periphrastic verb formations in datable documents ranging from Ancient Greek, through Vulgar Latin, and up to Modern Romance (list of documents used: 1973: 329 – 336). Datable documents for linguistic change within a particular language are frequently inventoried as part of the grammar of a given language; see, for example, Gray's (1971: 4 – 6) discussion of Semitic documentation.

The Bible serves as one of the most common documents of comparison over time and known geographical distribution. There are numerous bilingual presentations such as that of Streitberg's (1908) Greek and Gothic Bible. From the Greek Old and New Testament also came the Armenian version (fifth century). In the ninth century we have the Slavic translation of the Bible by Cyril and Methodius (Gray 1939: 427). Tschirch (1969) offers eight versions side by side: New Testament Greek, the Vulgate, the Old High German *Tatian* 830, the Mentel Bible of 1466, the *Evangelienbuch* of 1343, Luther's Bible of 1522, the Zinzendorf Bible of 1739, and the Menge Bible of 1926.

Another source of evidence across time and space is provided by etymologies and glossaries: Isidore of Seville's (636) Latin *Origines sive Etymologiae* (cf. 4.2. below), which parallels the Greek *Etymologicum Magnum* and the *Etymologicum Gudianum*; the glossaries of Hesychios (fifth century), which includes not only Greek and Latin,

but also Egyptian, Akkadian, Galatian, Indian, Lydian, Persian, Phrygian, Phoenician, Scythian, Parthian; the *Glossae Latinograecae et Graecolatine* (second — sixth centuries); the Gaulish Endlicher Glossary (fifth century); the Anglo-Saxon Epinal, Erfurt, Corpus and Leyden glossaries (700); the *Althochdeutschen Glossen* (850); the Romance Reichenau and Cassel glosses (eighth — ninth centuries) (Gray 1939: 425 — 426).

2.2. Documents as sources of evidence

Phonological evidence is gleaned from orthography with a search for misspellings, overcorrections, inverse spellings, occasional spellings, archaic and obsolete spellings, and very importantly rhyme and metrical evidence. Repeatedly in the literature the English Great Vowel Shift is used to exemplify evidence for phonological change. So, for example, there is ample rhyme evidence for the late coalescence of ME *ē* and *ę́* into NE [ī]. Chaucer (1400) rhymed *mean* with *clean* but not with *keen, queen,* or *green* (Bloomfield 1933: 295). Dryden (1700) rhymed *dream* and *sea* with *shame* and *obey*, respectively. Pope (1744) rhymed *weak* and *eat* with *take* and *state*, respectively. And Swift (1745) rhymed *seat* and *meat* with *weight* and *say't* respectively (Lehmann 1973 b: 162). Scott (1967: 77 — 78) is interested in establishing the consistent spelling distinction between ⟨ee⟩ <ę̄ and ⟨ea⟩ <ę̄. The source of his evidence is the *Oxford English Dictionary*, which yields *beetle* 1589, *deem* 1581, *deep* 1560, *flee* 1600, *free* 1580, *geese* 1577, *green* 1578, *seek* 1590 beside *cheap* 1567, *clean* 1568, *deal* 1534, *lead* 1569, *leap* 1580, *sea* 1555, *speak* 1535. Although phonetically distinct at this period, the sounds remain orthographically the same into New English. Accordingly, only "rhymes and occasional spellings" can attest to their later coalescence, "since the value of using regularly maintained orthographic contrast has now been exhausted".

Penzl (1957: 204) cites rhymes from the Old High German Otfrid *quad* : *sprah* 'speak' (third person singular past), and *ward* 'become' (third person singular past): *tharf* 'need' (third person singular present), as evidence for the spirantal pronunciation of ⟨d⟩ from WGmc. *þ*. Suprasegmental evidence is also appealed to by Lehmann (1955: 107) in determining continuant articulation (cf. 5.2. below) for the Indo-Hittite laryngeals. He observes that in Homeric meter the laryngeals make position just as *σ* and *Ϝ* before *ρ* and *λ*. A line of alliterative verse can reveal information about both vowels and consonants. Leh-

mann (1973 b: 67) cites from the Old English *Beowulf: Da com of more under misthleopum/Grendel gongan, Godes yrre baer* 'Then came from the moor under cover of darkness/Grendel walking, God's anger he bore'. One can extrapolate the length of the *o* of *more* since it is the word which alliterates, while the *o* of *of* is not in an alliterating word, metrically non-prominent and therefore short. Further one can assume that the alliterating *g*'s of the second line must have been similarly pronounced.

Occasional spellings such as Lat. *Caesar* written in Greek texts as *kaisar* point to the retained stop pronunciation of Lat. *k* (Bloomfield 1933: 296; cf. 3.2. below). Early Modern English and American English occasional spellings like *nex* 'next', *husbon* 'husband', *myssomer* 'midsummer', *wrytyn* 'writing', *orphants* 'orphans', *meten* 'meeting', and *of* 'have' show "loss (or addition) of phonemes in fast, unstressed forms" (Penzl 1957: 202). Inverse spellings such as Spenser's *whight* for *white* tell us that *gh* after vowel is no longer pronounced (Lehmann 1973 b: 67; cf. 3.2. below). The overcorrection of Lat. *thesaurus* 'treasure' in Late Latin documents to *thensaurus* with unhistorical intrusive *n* lets us infer that the Late Latin scribe was aware that the Latin cluster *-ns-* of, for example, *mensa* 'table' or *sponsa* 'bride' was reduced to *s* in Proto Romance (Hall 1964: 292). Archaic spellings point to language change. Jeffers — Lehiste observe the high functional load of *i* in Modern Greek. They write:

> Because we are fortunate to have a long history of written records for Greek, we know that Modern Greek *i* merged unconditionally ... if Modern Greek did not use archaic spellings, the extraordinary statistical preponderance of *i* within Greek would be the only hint that the original state of affairs might have been different (1979: 47).

Essentially, documentary evidence for the morphological, syntactic, and semantic levels of language is pragmatically based as well. The appearance of morphemes, phrases, and sentences in context and discourse, and their alternation in form or syntagm serve as evidence (cf. 3.1. and 5.1. below). Polomé (1966: 60−61) uses both age of document and ability to rhyme as support for the suggestion that Skt. *umá* 'flax', which occurs in the Sátapatha Bráhmana and rhymes with younger *ksumá* 'flax', may be derived from Chinese rather than being of Indo-European origin. Li — Thompson (1974) speak of the emergence of compounds in early Archaic Chinese (tenth — eleventh centuries B. C.), where they were rare, in late Archaic Chinese (third — fourth centuries), where they increased, and during the Han dynasty,

where they became yet more frequent. In Modern Chinese, compounding is a productive process leading toward agglutination and the tendency of SVO to SOV in Mandarin Chinese. Traugott (1972: 93) observes the alternation between inflected and non-inflected past participle in Old English periphrasis with 'be', thus, *we wæron cumene* 'we were come' and *we wæron cumen* 'we had come' as indicative of the change from aspect to tense. In interpreting the documentary material of whatever linguistic level, the time-honored principle called variously that of biuniqueness, isomorphism, or iconicity, namely, that of one form: one meaning, is a fundamental working principle (Anttila 1972: 17), as can be observed in the above discussions of evidence.

3.1. Loans and grammatical reflexes of contact

The interpretation of loan material is a manifold operation. Penzl (1972) distinguishes between loans transmitted orally, for example quotidian terms, and those transmitted in documents. A transliteration or transference of a word from language *X* into language *Y* can be indicative of the pronunciation of language *Y*. This assumes thorough knowledge of the graphemics and phonology of the donor language (1972: 6.5 a). If the document can be dated and identified for dialect, accuracy of the analysis is heightened and the information can also be used to shed light on the phonology of the source language. Contact data are occasionally attributed to causation of linguistic change (1972: § 9.4), whether through adstratum, superstratum, or substratum.

Penzl further distinguishes between imitation and substitution of the loan material. With reference to phonology he writes:

> Lautnachahmung ist die Wiedergabe des Lautes der Ursprungssprache durch den nächststehenden Laut in der entlehnenden Sprache. 'Nächststehend' kann auch hier nicht rein phonetisch bestimmbar sein, sondern wird schon phonologisch-strukturelle Faktoren einschließen müssen. Lautersatz kann darüber hinaus oft unter Verzicht auf phonetisch möglichst nahe Wiedergabe verschiedene Grade der 'Nostrifizierung' mit phonotaktischen Umstellungen, Vereinfachung von Lautgruppen, Morphemersatz durch 'Volksetymologie', d. h. Angleichung an vorhandene Morpheme der Entlehnsprache enthalten (1972: § 6.5 a).

Penzl's imitation and substitution parallel somewhat Haugen's substitution and importation, respectively (Haugen 1969: 60–61). Haugen correlates these two types of borrowing to a "scale of adoptability" whereby, following Whitney's insight that "whatever is more formal or structural in character remains in that degree free from the intrusion

of foreign material" (1969: 72), certain elements of language are more likely to be borrowed than others. Borrowing can lead to language change in any component of the grammar, but it is generally agreed that phonological change is least affected by contact evidence, while lexical change is most affected.

The interpretation of contact evidence in the phonic, grammatical, and lexical components is systematized by Weinreich (1964) in identifying the type of interference exhibited by the loan data, and also explaining the structural and non-structural stimuli and resistance factors involved. On the phonic level, (a) underdifferentiation occurs when two sounds of the source language are not distinguished and therefore confused in the target language. The opposite occurs in (b) overdifferentiation. Reinterpretation of distinctions (c) occurs when a relevant feature in the donor language is not relevant in the borrowing language, but relevance is signaled by another, possibly concomitant, feature in structural conformity with the borrowing system. Phone substitution (d) refers to the borrowing of a structurally equivalent phoneme adapted to the borrower's pronunciation (1964: 18−19).

On the grammatical level Weinreich considers (a) the transfer of both bound and free morphemes. Interference from the recipient's language can reinforce a morpheme by adding the functionally corresponding native morpheme to the borrowed morpheme (1964: 33). As an example of (b) transfer of grammatical relations, Weinreich cites interference by word order, sentence intonation, and congruence rules from the recipient to the donor language (1964: 37−39). In a third case (c), change takes place in the function of a borrowed morpheme or grammatical category due to the borrower's misidentification (1964: 39−40). By (d), abandonment of obligatory categories, Weinreich refers to loss of grammatical markers of the source language by the target language (1964: 42−43).

Lexical evidence is categorized by Weinreich as 1) outright transference of words, 2) semantic extensions, and 3) phonic adjustment without effect on the content (1964: 47−50). Weinreich sums up the effects of lexical evidence on the recipient language thus:

> Except for loanwords with entirely new content, the transfer or reproduction of a foreign word must affect the existing vocabulary in one of three ways: 1) confusion between the content of the old and the new word; 2) disappearance of the old word; 3) survival of both the new and the old word, with a specialization in content (1964: 54).

3.2. Loan and contact evidence

Graphic interference is rarely considered with regard for its own system, i. e., as orthographic change or development. Traditionally the orthographic emergence of writing systems in Europe is traced from Western Semitic (Phoenician), to Greek, into such European languages as Germanic, Slavic, and Romance. *A study of writing* (Gelb 1952) is a thorough presentation of the evolution of writing and of comparative writing systems. Johannes Friedrich's *Extinct languages* (1957) provides the history and methods of the deciphering of ancient scripts such as the cuneiform Hittite of Boğazköy, one of the spectacular finds of the twentieth century. See also George Trager's convenient overview in his "Writing and writing systems" (1974).

Similarly, the adoption of, for example, German ⟨ä⟩ and ⟨ö⟩ into Finnish, or that of the Runic *thorn* into Old English, are hardly interesting for language change unless they have phonological consequences, such as serving as evidence of their pronunciation in German and Runic, respectively. Lehmann (1973 b: 226) represents another case in point in the replacement of Runic/Old English *thorn* ⟨þ⟩ by English *upsilon* ⟨y⟩ for the definite article *the* in fifteenth-century printing: thus *ye* was used in, for example, *ye olde gifte shoppe* entailing a change in pronunciation. Similarly, the introduction of unhistorical ⟨h⟩ early into Eng. *autor* borrowed from Fr. *autor* yields the spelling-pronunciation /ɔ́θər/ (Hall 1964: 272). The vast amount of spelling evidence of borrowed words, then, properly belongs to phonological evidence.

Phonological evidence consists principally of two types: that which introduces a new feature into the borrowing language and that which serves as corroboration and/or confirmation of the persistence of a feature already in the borrowing language. Weinreich (1964) gives as new phonological sequences produced through borrowing, word-initial *v-* and *z-* in English, final *-ng* in French, and word-initial *dl-* in Yiddish. Entirely new phonemes through contact "may be created: /g/ as distinct from /k/ in Czech, /ʎ, ɲ/ as distinct from /l, n/ in Yiddish, /o/ as distinct from /uo/ in Lettish, /f/ as distinct from /xv/ in dialectal Russian, /t/ as distinct from /d/ in Mazateco ..." (1964: 27). The reintroduction of the initial *sk*-cluster into English came via Scandinavian borrowings such as *sky, skin, skirt* after English had shifted [sk] to [š] as in OE *scōh* which is NE [šu] 'shoe' (Jeffers—Lehiste 1979: 149).

Other phonological loan material can be applied to yield evidence either of the source or the target language. Most commonly borrowed

matter accommodates the structural tendencies of the target language. Thus Chinese words such as *chow mein* or *kowtow* lose their tones in English. This tells us only that English is a non-tone language and nothing about sound change per se. Quite otherwise is the case of Swedish spoken in Finland, where loss of Swedish tone is due to the Finnish adstratum. On the other hand, in NE *Don Quixote*, [h] resembles Spanish pronunciation, while its derivative adjective *quixotic* [ks] is integrated into English structure. The non-integrated *Quixote* is of interest on the sociolinguistic level, displaying retention of a foreignism as a possible learned or prestige pronunciation. The [h] changes nothing in the structure of English phonology, but the ⟨x⟩ points to the fact that when first borrowed *Quixote* was probably pronounced according to English habit [ks] (Arlotto 1972: 186). Penzl (1957: 207) points to the Old High German rendering of Old Slovenian *s, z* by OHG ⟨z⟩ and of Old Slovenian *š, ž* by OHG ⟨s⟩ as indicative of a sibilant pronunciation for OHG *z* but a shibilant pronunciation for OHG *s*. Interestingly, a change in spelling took place from Middle English *deleite*, a loan from Old French, to later *delight*. The pronunciation remained [ai] and the newer spelling serves as evidence for the loss of the velar spirant in such words as *light* and *eight* (Bloomfield 1933: 294). Phonological change in the source language is revealed by the German borrowing of Lat. *cellarium* as Ger. *Keller* 'cellar' but Lat. *cella* as Ger. *Zelle* 'cell', showing the Latin change of stop to affricate about the fifth to the seventh centuries.

Consequences of morphological borrowing are also twofold. Lehmann writes (1973 b: 218) "borrowings generally take on the patterns of native elements". He cites the borrowing of the Old Norse reflexive *báda sik* 'bathe oneself' into Old English as *bask*, wherein the reflexive pronoun is not perceived by the English speaker. Morphological blends occur in the case of the borrowing of Chinese *keňkyuu* 'study' or English *taipu* 'type' into Japanese, which supplies its native forms *suru* 'do' or *shita* 'did' for conjugation.

Arlotto (1972: 187) describes the borrowing of Arabic *qadi* 'mayor' into Spanish as *el alcade*, in which *al-*, the Arabic definite article, is not perceived by the Spanish and accordingly Spanish supplies its native morpheme. The opposite occurs in the case of English *apron*, borrowed from Old French *naperon*, by reinterpreted juncture after the initial nasal, giving the semblance of the English indefinite article preceding the noun. Similarly, Whiteley (1967: 138) describes the integration of English loans with initial *ma-* into Swahili, for example,

maching'óda 'marching order', as Swahili plurals due to the *ma-* plural marker; a singular *ching'óda* is then derived.

Weinreich's (1964: 32) discussion of the transfer of the Bulgarian first and second persons singular verb desinences into Meglenite Rumanian is an instance of outright transfer, i. e., transfer without modification to the native system. Thus, Bulgarian *-um* (*-ăm*) and *-iš* replace older *-u, -i*; for example, *aflum, afliš* 'I, you find'. He explains the success of this change in the Rumanian dialect as dependent upon "congruent grammatical structures and *a priori* similar vocabularies" (cf. Penzl's discussion of loan imitation and substitution, 3.1. above). On the other hand, Jeffers and Lehiste (1979: 149 — 150) cite an instance in which the morphological acceptance by the target language is only partially completed. Russian *pal'tó* 'overcoat' is borrowed from French *paletot*. The French masculine noun assumes neuter gender in Russian under the influence of neuter nouns ending in *-o*. However, *pal'tó* resists inflection in all cases and numbers and accordingly is still perceived as a loan.

Syntactic interference on the word level is noted by Lehmann (1973 b: 221) in the case of English compounds such as *attorney general* and *malice aforethought*, which reflect the noun adjective structure of a VO language such as French, from which English borrowed these collocations. The Russian habit of signalling possession by the preposition *u* 'at, by', rather than through a verb of possession, for example, Rus. *u menja dengi* 'at me money' = 'I have money', reflects transfer of Finno-Ugric syntax, thus Hungarian *van nekem egy könyr* 'there is to me one book' = 'I have a book', according to Arlotto (1972: 194). Arlotto further cites the borrowing of the Persian *izafet*, the morpheme linking adjective and noun, together with Persian NA order in Turkish, for example, Per. *mardan -e xub* 'men -izafet good' = "good men' beside Tur. *donanma -i hümayun* 'fleet -izafet imperial' = 'the imperial fleet' (1972: 195).

Topicalization in the English spoken in Ireland is affected by the Celtic substratum. Jeffers and Lehiste (1979: 155) observe the possible variations of the sentence *I'm going to Dublin tomorrow* in Hiberno-English as "a) It's me that's going to Dublin tomorrow. b) It's going that I am to Dublin tomorrow. c) It's to Dublin that I'm going tomorrow. d) It's Dublin that I'm going to tomorrow. e) It's tomorrow that I'm going to Dublin". Another syntactic example is the basic word order of Amharic, an Ethiopic Semitic language, which is attributable to enduring contact with Cushitic (Givón in Hyman 1975: 115).