LANGUAGE CHANGE—
A LEXICAL PERSPECTIVE

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Within the Western tradition, the study of language change began with the famous observation of William Jones in a lecture delivered on February 2, 1786. After some detailed comparisons of the classic languages of Europe, Greek and Latin, with far-flung languages like Persian and Sanskrit, he suggested that (a) the similarities among them were too strong to be attributed to chance—consequently they must be due to "some common source"; and (b) this parent language, "perhaps, no longer exists." Considering the year, which was decades before biologists came to parallel observations concerning speciation of organisms, this was a remarkable pair of insights, which centuries of historical studies on language were to build upon. Although this observation has been called the "Indo-European hypothesis" because of the languages referred to, its implications were, of course, equally applicable to all languages.

Before Jones, discussions on language history were mostly mired in philosophical and theological speculations. The small amount of empirical work that had been done was largely unfruitful. It was vitiated by the misconception that one language could have evolved from another language contem-

1On leave from the University of California at Berkeley, 1978–1979. I wish to thank the Guggenheim Foundation for their support, Osmania University for their hospitality, and, in particular, Bh. Krishnamurti and M. Hashimoto for much stimulation and helpful comments.

2In footnote 7 of their important paper, Weinreich, Labov & Herzog (51) trace discussions of language change to Dante and Tolomei in the early sixteenth century. However, we should surely learn more about non-Western traditions (e.g. Chinese, Indian, Semitic) before accepting their claim that "For obvious reasons, awareness and discussion of language change developed first in the Romance world."
poraneous with it. (A very similar mistake was made in the middle of the
nineteenth century in evolutionary discussions, when many misunderstood
Darwin to claim that man evolved from the living apes.) The Indo-
European hypothesis turned the study of language change into a serious
field, where pieces of objective data could be cumulatively gathered and
finer and finer theories could be constructed on the basis of these data.

This is indeed what took place. In the ensuing decades, a tremendous
amount of empirical work was done on the Indo-European languages by a
series of outstanding scholars, starting with names like Rask, Bopp, and
Grimm. A very readable account of their accomplishments is available in
Pedersen’s brief survey (37). In Lehmann’s anthology (28), one can read the
English translations of some of the original contributions.

After almost a century of gathering data and reconstructing the linguistic
past, a movement took place that continues to influence diachronic thinking
to this day. This movement has been vividly recounted in Hockett’s presi-
dential address to the Linguistic Society of America. The major figures of
this movement called themselves “neogrammarians”, and Hockett (17)
labeled the central product of this movement “the regularity hypothesis.”

A classic piece of scholarship from this movement is Karl Verner’s study
of the so-called Grimm’s Law (45)—an exemplary work for its lucidity and
boldness. The data here have to do with the “exceptional” voicing of certain
Indo-European plosives as these are found in Germanic. In successfully
explaining away these exceptions, Verner showed more clearly than ever
before (a) the distinction between a conditioned change and an uncondi-
tioned change, and (b) the critical importance of taking in account as
possible conditioning factors not only the segmental context, but the sup-
rasegmental context as well. Although point (a) was not new with Verner
(Grassman had used this distinction in an earlier investigation of the same
data), point (b), with its innovative conception of two types of accent, was
definitely an original contribution. But even more significant than these two
points was the boldness of his victory proclamation: “There must . . . exist
a rule for the irregularities; the task is to find this rule” (45, p. 36).

The use of the word “law” has not been consistent. Discoveries of the sort
Grimm made among a specified set of languages, e.g. Indo-European and
Germanic, are better termed “correspondences” since they refer to particu-
lar events. Phonetic formulas which recur in a variety of correspondences
across time, space, and genetic groups can rightly be called “laws,” with the
implication that their recurrence has a basis in the mechanisms of speech
production and perception. Linguistic laws, unlike physical laws, are not
predictive, though they are explanatory to varying degrees. In this respect,
they are like evolutionary laws in biology, as explained, for example, by
Luria:
The modern theory of evolution, like all historical theories, is explanatory rather than predictive. To miss this point is a mistake that theoreticians of history have often made. Prediction would require not only a knowledge of the main force—natural selection—but also a prescience of all future environmental conditions, as well as of future balances between the quasi-deterministic effects of the law of great numbers and the purely probabilistic role of genetic drift (31, p. 23).

With a few obvious substitutions of terms, this statement has a perfect fit for linguistic laws.

Encouraged by a series of successes like that of Verner, the neogrammarians stated their doctrine, its clearest wording offered by two leaders of the neogrammian movement, Osthoff and Brugmann (35):

... every sound change, inasmuch as it occurs mechanically, takes place according to laws that admit no exception. That is, the direction of the sound shift is always the same for all the members of a linguistic community except where a split into dialects occur, and all the words in which the sound subjected to the change appears in the same relationship are affected by the change without exception.

This neogrammian doctrine, formulated most forcefully in the 1870s, has been accepted in various forms by virtually all major groups of historical linguists since that time. There were critics, of course, most notably Hugo Schuchardt (41), who pointed out internal paradoxes in the doctrine itself and also referred to large bodies of exceptions from dialect studies. As a more recent example, Malkiel (33) pointed out that in Bloomfield's influential book, Language, the chapters on the comparative method and on dialect geography depicted views of sound change that are mutually contradictory. Elsewhere he remarks, as do many other scholars of change, that exceptions are "embarrassingly numerous" (34). But by and large those challenges were not heeded, and the doctrine passed down across the generations through Saussure, the structuralists, and the generativists, in one form or another. Lucid analyses of this historical development have been provided by Weinreich, Labov & Herzog (51), and by Chen (5).

Basically there are two related sources of difficulty with the neogrammian conception of language change: the isolation of language from its social context, and the exclusive assumption of phonetic gradualness. The first difficulty is reflected in Hermann Paul's influential neogrammian text, Prinzipien der Sprachgeschichte of 1880 (36). As has been pointed out by Weinreich et al (51), "the price of such isolation was the creation of an irreconcilable opposition between the individual and society." The difficulty became greater in Saussure's writings, where there is an increased tendency to retreat to the language of the isolated individual, and where furthermore, such language is supposed to be "homogeneous." The ultimate retreat along this line of reasoning is the "monastic" model advocated by Chomsky, "the ideal speaker-listener, in a completely homogeneous speech-
community...”, which is to my mind the *reductio ad absurdum* of such isolationism.

This characterization of “monastic,” which comes from Labov, is particularly apt for describing the withdrawal symptoms of the kind of puristic linguistics that weaves grand designs of “logical structure” that somehow manages to remain untainted by work either in the field or in the laboratory. Little wonder that this sort of pursuit has drawn embarrassing criticisms from other fields, such as that from Wilson (53), that linguistics appears to be more concerned with “celebrating personal visions” than serious empirical work. These criticisms, however, are unfair to linguistics-at-large. Responsible, cumulative, empirical work has been and continues to be carried out in many sectors, adding to the long-term heritage of the field. Such work typically has less popular appeal and is less likely to be noticed by people from other fields.

Time and again it has been shown that completely homogeneous speech communities have no more reality than the ideal speaker-listener. [An early demonstration of this was the study of Gauchat in 1905 (13).] To advocate that the primary concern of linguistic theory is with those two illusions is to seduce linguistics onto the path of sterility. It is like trying to understand chess by watching the movements of some single piece in isolation, without regard to its relation and interaction with all the other pieces. More than any others, Weinreich et al (51) exposed the difficulty of the neogrammarians position and at the same time provided a strong foundation for cumulative empirical research. This foundation has been built upon by a host of scholars who show that orderly heterogeneity not only exists in every language, but that it can be elegantly treated by techniques developed for describing variation. Although the bulk of this research grew out of investigations on English, the techniques should prove even more fruitful when applied to settings which are typically much more multilingual, which is the case with the majority of speech communities in the world today.

In this chapter, however, I will be concerned more with the other major source of difficulty that has been passed down from the neogrammarians, the exclusive assumption of phonetic gradualness. Osthoff & Brugmann (35) conceive of the manner of the change as being “mechanical,” and it is by virtue of this fact that “all the words... are affected by the change

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3The orderly heterogeneity that must be there by the very nature of linguistic development has not escaped the sharp observations of scholars looking at language from the outside, as it were. Consider this suggestive metaphor by L. Wittgenstein, brought to my attention by B. Abbot: “Our language can be seen as an ancient city: a maze of little streets and squares, of old and new houses, and of houses with additions from various periods; and this surrounded by a multitude of new boroughs with straight regular streets and uniform houses” (*Philosophical Investigations* I:18).
without exception." In other words, the regularity is guaranteed by the mechanical nature of sound change—it is not an independent fact to be discovered and verified, but a consequence of the nature of the process itself. The faith in this mechanical process is what prompts Bloomfield, for instance, to put the matter in two crisp words: "phonemes change," i.e. not morphemes or words or other possible units.

But what is the nature of this mechanical process? This was not elucidated by the neogrammarians themselves. Later scholars proposed various metaphors toward an explanation. Jespersen, for one, compares shifts in phonetic values with random errors that one observes when sawing logs, estimating the length of the next log on the basis of the one just sawed off. The picture that Hockett offers is one where numerous phonetic events scatter into a statistical distribution, much as the hits and misses are scattered on a target board; the statistical modes of these distributions gradually shift in time by small, imperceptible increments. Although such metaphors of random events do not entail a directionality per se, presumably these authors would accept some hypothesis of least effort, such as that proposed by Paul as well as many others, so that the fluctuations are cumulative rather than mutually cancelling.

In the Osthoff-Brugmann passage quoted earlier, we can discern two parameters of a sound change—lexical and phonetic. To build regularity into the process, the neogrammarian assumption is that all relevant words change at once—that is, in a lexically abrupt fashion. Yet we all know that sound changes do not take place overnight. So graduality along the phonetic parameter is an inevitable consequence of assuming lexical abruptness. Furthermore, many writers of the neogrammarian persuasion appear to believe that this combination of lexical abruptness and phonetic gradualness is the process of sound change. Within the perspective advanced in this paper, however, I will argue that there must be other processes as well for implementing sound change.

I first encountered difficulty with such a conception of sound change when I was trying to understand a peculiar development in Chaozhou Chinese, where the short high tone became the short low tone during the same period that the short low tone became the short high tone. Since such a phenomenon is of great theoretical interest, though they are rarely reported in the literature, I suggested the term "flip-flop" to call attention to them (47). The logical difficulty with the situation is that if X is gradually shifting toward Y during the same period of time that Y is gradually shifting toward X, then surely X and Y would merge phonetically. To get around

4This is by no means an aberration of some isolated community; similar phenomena can be observed in numerous dialects of the South Min group.
this difficulty, I suggested that a sound change of this type must be implemented in a lexically gradual fashion, diffusing across the lexicon by several words at a time (47). In a later paper, I called this process “lexical diffusion” (48).

The logic is the same in metathesis and in flip-flops, which can be viewed as syntagmatic and paradigmatic versions of the same type of development. It simply makes no phonetic sense to say that a cluster like /ks/ gradually shifted to /sk/ through a series of imperceptible increments. Neither is it plausible to believe in a gradual continuum between members of well-attested changes, such as m > n or t > k, since altogether different articulators are involved (48).

A more general way of seeing the difficulty is the absence of intermediate forms, a situation not unlike that discussed by evolutionary biologists (e.g. 15). If there is indeed the kind of constant massive shifting along continua that phonetic gradualness assumes, then there should be evidence for this everywhere in the living language of the world. Since all living languages are always undergoing change, we should be able to observe many intermediate forms as the various sounds shift from one prototype to another. For an ubiquitous change like t > ç, for example, phonetic gradualness would predict that one can easily gather data from the living languages to document the infinite continuum between the t and the ç. Yet such data are strikingly rare in the literature; for many types of change, such as those involving articulatory discontinuities exemplified in the preceding paragraph, they may not be obtainable at all.

To get around the problem of articulatory discontinuities, one may entertain the hope that perhaps the gradualness can be found in the acoustic signal instead. But this hope would be short-lived when we consider in greater detail the complex relationship between articulation and acoustics, along the line suggested by Stevens (43, 43a). By modeling the vocal tract electronically and mathematically, Stevens has found that the articulatory-acoustic relations can best be understood as relatively discrete “quanta.” These quanta are schematized in Figure 1.

In the diagram, regions I and III show areas where relatively large differences in articulatory configuration have negligible acoustic consequences, that is, they correspond to minor differences in the speech signal. We would hear the same sound in I and III, even though they involve a large range of different articulations. On the other hand, region II shows areas where relatively small differences in articulation have major acoustic consequences corresponding to large changes in the acoustic parameter. The inference to be made from this quantal theory is that the prototypical speech sounds are constructed in regions like I and III, the quanta, which would permit the speaker a larger range of imprecision in his articulation with
Figure 1  Schematized relation between an acoustic parameter of speech and a parameter that describes some aspect of articulation. Regions I and III show phonetic areas where relatively large articulatory movements have negligible acoustic consequences, whereas region II shows the converse. Taken from Stevens (43).

a minimum distortion on the intended acoustics. Such a theory would predict, then, that the actual acoustic space populated by the sounds of human language does not have uniform density, but is made of clusters of stable regions. Although it would be a tremendous task to verify this theory in detail, the evidence that Stevens has presented on selected examples is quite persuasive. [However, see the criticisms of Ladefoged (27a).] If we accept these quanta, the hope that phonetic gradualness is to be found in acoustics rather than articulation would be considerably dimmed.

Having considered the discontinuities in the articulatory and the acoustic, one might still wish to pursue phonetic gradualness into the one remaining domain, the perceptual, since again the acoustic-perceptual relations are anything but straightforward. Is the perceptual space made up of multidimensionally gradual continua? Again, recent results on categorical perception suggest that the perceptual space is like that which Stevens proposes for acoustic space, quantal rather than smooth (30). Furthermore, the perceptual boundaries for these quanta, for some linguistic features at least, appear to be located at consistent places and detectable even in very young infants, suggesting such boundaries may have a genetic basis (10). Establishing discontinuities in all three phonetic domains—articulatory, acoustic, and perceptual—does not conclusively rule out the possibility that there may be a very few types of change that are phonetically gradual, namely
those for which the space is smooth in all three domains. But we are justified in concluding that phonetic gradualness cannot be the exclusive process for sound change.

In the above paragraphs I have only given a brief outline of the arguments surrounding phonetic gradualness; more detailed reviews of this problem, including the opinions of a variety of scholars (e.g. Wheeler, Sturtevant, Sommerfelt, Sapir, Martinet, Greenberg, Halle, etc), have been presented by Chen (5) and Wang (48). To take just one example, we find Sweet (44), under the influence of Paul and Sievers (p. xii), writing confidently that “... all sound change is gradual; there are no sudden leaps in the phonetic history of a language” (44, p. 15).

It appears that an analogous problem was discussed much earlier in biology, as seen in this prescient remark from Thomas Huxley to Charles Darwin, made on the day before publication of the Origin of Species “You have loaded yourself with an unnecessary difficulty in adopting Natura non facit saltum so unreservedly” (15, p. 115). With the benefit of hindsight, the same remark might very well be made to the neogrammarians.

These two sources of difficulty in the dominant doctrine, namely the isolation of the individual from his social context, and the assumption of exclusive phonetic gradualness, are of course importantly related. If indeed the sounds undergoing change shift by minute imperceptible increments, it is difficult to see how the various members can calibrate against each other and keep the speech community “homogeneous,” since no one presumably can hear these increments. If, on the other hand, the change were to be lexically gradual, an inevitable consequence of admitting phonetic abruptness, then the changed words are clearly observable from speaker to speaker in a community characterized by “orderly heterogeneity.” Those words are part of each speaker’s linguistic personality, which can be imitated or avoided, much like many other cultural traits, depending on a host of other linguistic and extralinguistic factors. Alexander Pope must have had some such scenario in mind when he wrote:

In words, as fashions, the same rule will hold
Alike fantastic, if too new or old.
Be not the first by whom the new are tried
Nor yet the last to lay the old aside.

In the remainder of this paper, my concern will be with two related tasks. First, I will present some detailed results which show the reality of lexical diffusion as it is observed in a variety of languages. In doing this, it will be seen that certain methods of working with large quantities of data are more likely to lead to fruitful results.
With the proof of lexical diffusion and exchanging of the neogrammarian monolithic path of phonetic gradualness for one that has multiple paths, new areas of research become available that prompt even deeper questions on the nature of language change. What is the relation between a particular type of sound change (say, final obstruent devoicing vs vowel raising) on the one hand and the process of change on the other, i.e. phonetic gradualness, lexical diffusion, and statistical variation in the sense of Labov? (These paths, it should be emphasized, are not mutually exclusive.) How are these different processes related to central linguistic issues like language acquisition, dialect borrowing, the comparative method, and techniques of genetic subgrouping? My second task here will be to pursue these new questions for diachronic linguistics in a preliminary way in the hope of eventually sharpening them enough so that useful hypotheses can be formulated on them.

In sharp contrast with the case of phonetic gradualness, where intermediate forms are all but easy to come by, intermediate forms for lexical diffusion are everywhere, provided we recognize them for what they are. One of the clearest statements of this issue is that by Vogt (46, p. 367):

At any moment, between the initiation and conclusion of these changes, we have a state characterized by the presence of more or less free variants, so that the speakers have the choice between alternative expressions. In each case the choice will be determined by an interplay of factors, some linguistic, some esthetic and social, an interplay so complex that most often the choice will appear as being due to pure chance. ... What therefore in a history of linguistic system appears as a change will in a synchronic description appear as a more or less free variation between forms of expression, equally admissible within the system.

Although such synchronic variation has been widely observed in literature, discussions of it have been largely within the context of dialect borrowing. Scholars like B. Wheeler, for example, specifically rejected synchronic variation as evidence for system-internal change because of their acceptance of phonetic gradualness. The following quote is a good example of how expectation determines perception: "It is to be noticed that the operation of the laws of sound is unconscious and gradual, so that the old form cannot, except through mixing of dialects, survive alongside the new (52)."

Freed from the illusions of homogeneous speech communities with ready-made geographical and social boundaries, the concept of dialect turns out to be both subtle and complex. Many linguists have too easily resorted to the facile escape clause of "dialect mixture" without adequate verification. For the present purposes, I see no reason why synchronic variation cannot be a mechanism in the implementation of sound change, whether the actuation was from inside or outside the linguistic system undergoing change.
In Table 1, a schematized version of lexical diffusion via synchronic variation is given. For any \( W_i \), the notation \( \overline{W}_i \) represents the changed counterpart of that particular word. So a typical change involves three stages: u (unchanged), v (synchronic variation) and c (changed). In the table, \( W_1 \) is the most advanced word, having already reached the last stage, i.e. the c stage. \( W_2 \) and \( W_3 \) are in the v stage, while \( W_4 \) and \( W_5 \) are still in the u stage. Although it is obviously important to know whether a change is actuated internally or externally, phonetically or conceptually, the implementation by such a process of lexical diffusion should be the same.

Once we recognize the critical role that synchronic variation plays in lexical diffusion, then such evidence is indeed easily found in every living language. Here is an example from American speech.

There is an ongoing change of vowel laxing that is diffusing across the lexicon, where /u̯w/ in monosyllables is becoming /i/. Usually in such cases, one can detect subregularities. So the words which end in /k/ have mostly reached the c-stage: book, nook, rook, hook, shook, cook, brook, took, look, etc. On the other hand, those in which the vowel is morpheme-final remain in the u-stage, (since English phonotactics does not permit final /i/), even when it is not word final: whose, zoos, twos, shoes, moored, wooned, coed, etc. Perhaps it is the model of words like "zoos" that keeps another group in the u-class: ooze, choose, lose, lose, booze, goose, noose, etc. Similarly, the words which end in /I/ have conservatively remained in the u-stage: tool, pool, spool, drool, etc.

If the above were the sum total of the relevant data, then lexical diffusion is no more than the specification of regular sound change in terms of very fine classes, where each class is uniquely defined by a condition, either phonological or morphosyntactic. However, the point is that typically the data are more complex, and that words leak through the boundaries of these definable classes, forming groups which are too idiosyncratic to justify setting up new classes, and stretch across the stages of change. So for the words ending in /t/, we have all three stages of the change in the words.

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<th>Words</th>
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u-stage: boot, loot; v-stage: soot, root; c-stage: foot. Words ending in labials largely exhibit the first two stages of change—u-stage: boom, loom, gloom, groom, spoof; v-stage: roof, room, broom, coop; though for many speakers there is the c-stage: hoof. Words ending in /d/ show the u-stage: food, mood, versus the c-stage: hood, wood, stood, good. (The word “good,” incidentally, appears to be involved in a later stage change from /ʊ/ to /ʌ/, where it is in the v-stage, while words like “blood” and “flood” have already reached the c-stage).

The above data are given to illustrate the complex dynamics of the diffusion process; they are far from complete. (One should also consider, for example, the large class of words spelled with “u” or “w,” e.g. flute, dune, rouge, newt, etc. Is it the spelling or the palatal onglide that is keeping them from joining this laxing?). Although not every American speaker will agree with each pronunciation reported here, the data do give a time-slice of the change in progress. The variation across speakers is something we should expect within an “orderly heterogeneity.”

A more fine-grained study of lexical diffusion has been reported by Bauer (4) for the Cantonese spoken in Hongkong. In this language there is a change that has been going on over several decades in which the final velar nasal is becoming alveolar, a trend that goes against the general development of final nasals as observed by Chen and Wang (7, p. 267). Again there are subregularities. For Bauer’s informant, the words which have low vowels are the most advanced; they have reached the c-stage. At the other end, the words with high vowels are all in the u-stage. So the process of alveolarization is obviously operating on the velar nasal in a way that is conditioned by the height of the preceding vowel. At present it is the mid-vowel words that exhibit the greatest synchronic variation. Whereas words with the vowel /ɛ/ have all changed, many words with the vowels /œ/ and /ɔ/ are pronounced with either nasal, with the conservative velar occurring in more formal contexts.

In another study of lexical diffusion, Janson (21) noted that Swedish words were losing word-final /d/ as early as the fourteenth century in their pronunciation, though not in their spelling. The point of special interest here is that the words that can undergo optional d-deletion are now much fewer than a half-century ago, as determined by earlier descriptions. In Stockholm speech the deletion used to be possible for more words across more grammatical categories. Since the “d” has been kept in the orthography, the reversal of this change probably came as a result of the rapid rise in literacy in Sweden in recent decades. In a few cases, furthermore, single words have bifurcated and the bifurcation is probably made permanent by the new spellings. “Träd” now means “tree”, with the “d” spelled, while “trä” means “wood”; “skuld” means “dept”, while “skull” means “sake.”
Typically in historical work, linguists have not paid much attention to the influence that orthography may have on changes in the spoken language. That this has not been a source of difficulty is probably due to the fact that although writing has existed for many thousands of years, the frequent use of written language has been restricted to a very small percentage of people during almost all of these millenia. With the dramatic rise in literacy in the world, however, over recent decades, it is likely that orthography will figure more and more prominently in language change, as will some other cultural innovations (e.g. mass communication and transportation devices) as these become increasingly available and widespread.

In all cases of lexical diffusion, we find leaders and laggars among the words, which raises the issue of what determines these schedules. As Vogt (46) remarked, they appear to result from an interplay of factors so complex that "most often the choice will appear as being due to pure chance." Yet it seems that slowly some of these factors are being sorted out. One such factor is frequency.

The notion that more frequent words will change earlier has been around for a while, though the tools for its verification were not available. At times the notion has been misused. A century ago, Scherer tried to apply this notion to certain apparent exceptions in Germanic, but as Verner correctly criticized (45, p. 37), his interpretation of the frequencies of the exceptional words was very vulnerable. Indeed tools like frequency dictionaries for even living languages need to be used with care in the light of various problems of statistical sampling; a fortiori intuitive guesses on old languages of the kind Scherer made cannot be convincing.

With judicious care, however, frequency data like those for English provided by Kucera & Francis (25) can be used to good advantage. Such is the case of Hooper’s (18) study of schwa deletion in sequences like -ary, -ery, -ory, and -ury. As Janson (21) did with the d-deletion in Swedish, she relied upon introspective judgments of subjects for phonetic data. She presented them with a list of 112 “schwa” words and asked them to classify each word into one of three categories.

There are subregularities here that Hooper duly factored out before correlating the phonetic data with the frequency data. The deletion is blocked, as one might expect, if a sequence results that is not permitted by English phonotactics, as with the vowel laxing discussed earlier. The three varieties of this subregularity, Hooper observes, are when the schwa is preceded by (a) a flap (e.g. watery), (b) certain consonant clusters (e.g. burglary), or (c) affricates (e.g. forgery).

The result of this study is a very good correlation between the phonetic data and the frequency data. The higher the frequency of the word, according to the count by Kucera and Francis, the greater its likelihood of losing
its schwa, according to the tabulation of subjects’ responses. From this result it would seem that when everything else is equal (which unfortunately is rarely the case), the leading words in a sound change are the most frequent ones.

An important aspect of Hooper’s study is that she extended her study to cases of analogical change. In these cases, there has never been any doubt that the process of implementation is via lexical diffusion. In the cases of phonological change that we have recounted, the words undergoing the stages of change are scattered irregularly over the various grammatical categories, making it more difficult for the analyst to observe the fact that the c-stage words are the models and the v-stage words provide the bridge for the change. In contrast, since analogical change typically takes place within paradigms of specific morphological classes, it has always been easy to observe lexical diffusion at work here with its stage by stage progression. Some strong verbs to illustrate the progression are given in Table 2.

A striking difference emerges when we compare these analogical data with the earlier cases in terms of frequency. In Table 2, the leading words are the least frequent ones, whereas the most frequent words are the lagger, a fact that Hooper was able to verify against the Kucera and Francis count. An early prediction of this relationship, which Hooper attributes to Paul around 1886, is thus now confirmed through the availability of the necessary tool.

In the context of the contrasting tendencies between these two types of change, the question naturally arises whether these tendencies can be traced to differences in how the changes are actuated. In discussing this question, Hooper suggests that analogy is actuated in the learning process, whether by child or adult. The less frequent words would be changed earlier (i.e. more vulnerable to overgeneralization by the learner), because it is less likely that the learner has encountered the unchanged form. In phonological changes, on the other hand, which are frequently phonetic reductions, the actuation is probably to be sought in the two-step process of reduced pronunciations in casual or rapid speech by the user and of acquiring the

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reduced form by the learner. Even though the articulatory gestures in the frequent and infrequent words may be quite comparable in their full forms, the speaker is more likely to reduce the more frequent forms because of their familiarity to the hearer. These are promising ideas that merit further investigations.

Among the languages of the world, only relatively few are associated with sizeable historical documentation. In cases where this exists, we have the opportunity of tracing an individual change through the centuries and arrive at an approximate “chronological profile” for that change. The study by Sherman (42) is aimed at providing such a profile for the development of diatones in English.

Diatones are noun-verb pairs where the stress falls on the first syllable for the noun but the second (or third) syllable for the verb, e.g. address, permit, subject, contract, etc. Evidence from early dictionaries show that such words used to have the stress on the noninitial syllable for both nouns and verbs. For words longer than two syllables, the picture is somewhat more complicated. Note, however, that in words like “delegate,” the last syllable is different both in stress and vowel quality depending on whether it is used as a noun or verb, showing an aftereffect of the stress shift.

The stress shift onto the first syllable to mark nouns is a change that started 400 years ago. For example, a word like “affix” was stressed on the second syllable for both noun and verb in 1612, as well as in Samuel Johnson’s dictionary of 1755. In 1775, however, it was first entered in John Ash’s *New Dictionary of the English Language* as having initial stress when used as a noun. This was, in short, how “affix” became diatonic and how a new diatone entered the lexicon. By referring to some 30 dictionaries and grammars published over these four centuries, Sherman was able to construct a chronological profile of this change. The change apparently is still in progress: of the 1315 disyllabic candidates for this change, where the noun and the verb are homographs, only some 150 have become diatones. See Figure 2.

Many other examples for lexical diffusion can now be given for a variety of languages. When the first cases of lexical diffusion were presented from Chinese data, there was the suspicion that this was an atypical phenomenon that was restricted in its occurrence to languages of the Chinese type, either due to the nonalphabetic orthography or to the noninflectional morphology. This suspicion can no longer be maintained, of course, now that lexical diffusion has been reported for a wide range of languages besides Chinese and English, including Canadian French (9), Dravidian (23), Dutch (14), German (3, 40), Nitinat (12), Swedish (21, 38), and Tibetan (32). It appears to be a basic process of change for all languages that exhibit “orderly heterogeneity,” which means, more simply, all human languages.
Figure 2  Increase in number of disyllabic diatonic N-V homographs in a chronological profile [based on Sherman (42)].

Several other significant issues arise within the perspective of lexical diffusion, some of which are discussed by Chen & Wang (7). Due to page limitations, I will allude to them only briefly here.

Given the gradualness of the diffusion process across the lexicon, there is no a priori reason to expect the process to complete itself. In particular, the process will be prevented from completion when during the course of the diffusion one or more changes compete against it for some of the same words. Some instances of such competing changes have been observed (6, 39) and provide interesting data for examining the dynamic relations of interacting processes along the time dimension.

Another basic issue is the relation of lexical diffusion with processes of language acquisition. Such a relation has been hinted at in my earlier recapitulation of Hooper’s results on word frequency and schedule of change. How a child learns a behavior as intricate as language must of course be an extremely complex process, composed of numerous different strategies at various stages. Furthermore, we should not underestimate the considerable differences that must exist from child to child, much as there are such differences across mature individuals. Nonetheless, some very clear data are available from Chinese (19) and from English (11) which show that
the acquisition is word-by-word rather than phoneme-by-phoneme, and that there are ontogenetic analogs in the 3-stage progression, i.e. from wrong pronunciations through variable pronunciations to right pronunciations.

Yet another issue of fundamental importance to our understanding of language change is the following. Can we take advantage of the diversity of the lexicon as it undergoes diffusion to arrive at finer genetic subgroupings of the languages or dialects undergoing change? Traditionally such groupings were primarily based on categorical changes—two dialects either share a change or they do not, one single bit of information. But if we take words as units, in cases of lexical diffusion, where for each dialect some words have changed and others have not, then clearly we have access to many more bits of information on which to base the subgrouping.

An early attempt to formulate this idea was made by Hsieh (20), in which a set of Wu dialects were grouped by criteria made available by lexical diffusion on one tone change. The results were encouraging in that they corresponded well with the subgroupings that scholars have made on the basis of other linguistic criteria. More powerful methods have now been developed by Bh. Krishnamurti (23), working with Dravidian languages. By supporting his methods with extensive computer programs and using the valuable Dravidian Etymological Dictionary as a point of departure, Krishnamurti has been able to make much more fine-grained comparisons than Hsieh did. Even though the comparisons were made on just a handful of changes, these methods have proved to be successful. With these powerful methods developed and available, the relations within and among the languages of the world can now be investigated with much greater objectivity and precision.

However, in studying these relations, it is critical to bear in mind the intrinsic limitation that tree diagrams impose. The use of trees is based on analogy with biological speciation [see (42a) for a historical sketch of Darwin’s influence on Schleicher]. The vital difference is that whereas species by definition do not interbreed and can transmit features only vertically, languages in contrast typically transmit horizontally as well. Furthermore, the manner of this transmission probably varies with different patterns of population movement, with very interesting consequences on the language histories, as seen in the recent comparison of Indo-European with Sino-Tibetan by Hashimoto (16a).

To conclude, from the studies discussed cursorily in this chapter, several trends in research method can be discerned which depart from the style of earlier scholarship. One has to do with the much larger empirical data base that we can now draw upon, which was not available even a few decades ago. This data base has been enriched both horizontally, in terms of the range of languages and dialects, and also vertically, by the recent availabil-
ity of more etymological dictionaries and more reliable reconstruction (43b). This method has been especially valuable in the work on Chinese and Dravidian.

Another trend is working in teams. The larger problems in linguistics simply are beyond the scope of single scholars working in isolation—not unlike the situation in other "harder" sciences. Their solution requires the collaboration of a coordinated team of investigators working in a symbiotic environment, each member contributing his own piece of expertise. In the cases of the quantitative work with the Chinese dialects and with the Dravidian languages, some of the expertise required came from outside linguistics proper, namely from statistics and data-processing by computer. Interdisciplinary work is no longer an accidental luxury; for many problems in linguistics such a mode of research is indispensable and must be consciously cultivated.

Lastly, note that current technology has been an important part of several of the studies I discussed. Without the computer to process immense bodies of linguistic data, or without the instruments of experimental phonetics to quantify speech sounds, many of the arguments recapitulated here would lose much of their force.

In providing this discussion of language change with a historical perspective, and in putting some earlier problems in relief, my intention has been to trace the stimuli which led to the present lexical perspective, not to detract from the solid contributions that the neogrammarians have made. Current researches on language change, in many ways, are building on the achievements of our predecessors. Indeed, reading Verner's paper must rank among one of the most exciting experiences for many a budding linguist, as it surely did for me.

Occasionally, one hears the lament from younger scholars who despair that "the truth of today inevitably turns into the mistake of tomorrow."\(^6\) Not so! A more accurate account is rather "the truth of today will be the 'special case' of tomorrow," when, hopefully, our perspective will be widened still more. The neogrammarian conception of language change will probably continue to be part of the truth. With the benefit of richer data and more powerful methods, our perspective on language change has been enlarged. We see that, given the remarkable complexity of language, which we are always too prone to underestimate, changes occur along other paths as well. Hopefully, this wider perspective will provide a more realistic foundation upon which deeper questions concerning language change and language relations can be raised and explored.

\(^6\)Such a distinction was made in a discussion between two biologists, as recounted by Konrad Lorenz in his preface to the reprinting of Darwin's *The Expression of Emotion in Man and Animals.*
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