RESOLVING THE NEOGRAMMARIAN CONTROVERSY

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Recent investigations of the history of Chinese have given new support to the view that sound change diffuses gradually across the lexicon. Yet instrumental studies of sound change in progress support the Neogrammarian position that change affects all words that include the sound according to their phonetic environment. The paradox can be resolved by distinguishing abstract phonological change from change in low-level output rules. Both types of rules can be observed in recent studies of sound change in progress in Philadelphia: the lexical split of short *a* shows lexical diffusion in progress, while raising, lowering, fronting, and backing rules show Neogrammarian regularity. A review of the literature on completed changes and other changes in progress tends to support the relevance of a hierarchy of abstractness in determining the nature of the transition from one stage to the other.*

1. INTRODUCTION. If such a thing as an advanced study of human behavior exists, linguistics may claim to hold that position—perhaps for no other reason than its ability to state precisely the issues being argued.[1] That such an ability represents a real advance is clear by comparison with the situation in closely related fields. The question of 'One phoneme vs. two' has a more precise meaning than similar questions that might be raised concerning, e.g., two statuses or roles, two memories, or two personalities. The precision of linguistic statement has gone further than this. More than once, we have been able to bring into confrontation two well-developed viewpoints, each firmly supported by the evidence.[2]

* This paper represents the presidential address given at the Annual Meeting of the Linguistic Society of America, Los Angeles, December 28, 1979.

[1] The research reported here was supported by the National Science Foundation under contracts to Columbia University from 1968 to 1970, and to the University of Pennsylvania from 1971 to 1977. I am greatly indebted to the members of the research groups involved for their contributions—some noted in the discussion, others included in the research reports that are the source materials: Anne Bower, Paul Cohen, Elizabeth Dayton, Donald Hindle, Anthony Kroch, Matthew Lennig, Arvilla Payne, Deborah Schiffrin, Richard Steiner, and Benji Wald. I am grateful to a number of other colleagues for personal contributions that appear throughout the discussion: principally Henry Hoenigswald, Robert Stockwell, and William Wang. It will also be evident that I have drawn heavily upon the exemplary scholarship of Yakov Malkiel and Iván Fónagy to an extent that demands special recognition. It is not usual to register our debts to linguists who are long removed from the present scene: but all those who work on linguistic change in progress must feel, as I do, a personal relationship with Louis Gauchat, and look for any opportunity to acknowledge a direct descent from the work of that extraordinary linguist and student of the human race.

[2] Among the many confrontations of this sort, several which I have more or less witnessed at first hand can serve as recent illustrations. The controversy over the phonemic status of the Old English short diphthongs, initiated by Stockwell & Barritt 1951 and Kuhn & Quirk 1953, continued over more than a decade but was never really resolved. There is no indication of a meeting of minds on the problem of the origin of creoles, where diffusion from a Portuguese-based Sabir (Whinnom 1965) is opposed to independent origins on the basis of universal psychological features (Bickerton 1974). The opposition between generative semantics vs. interpretive syntax may no longer be a current issue, but it would not be accurate to say that it was resolved.

Unfortunately, progress often stops at the point of this confrontation. Linguistic partisans are not unique in their tendency to discount rather than consider the evidence advanced by the other side. But it is fair to say that we are more noted for the clarity of our pronouncements than for the convergence of our views. We often find ourselves debating the same issues for decades without resolving them, until people lose interest and go off to consider other questions.

The orientation toward linguistic research that I would like to demonstrate here begins with a somewhat different perspective. It is motivated by a considerable respect for the intelligence of our predecessors, and for the evidence that led them to their conclusions. A careful consideration of competing bodies of evidence leads us to recognize the need for a higher-level theory that will take into account, as well as account for, the findings of both sides of the controversy. Such a synthesis can be achieved only if we ascertain the conditions under which each of the opposed viewpoints is valid. I don't think this can be done by simply re-shuffling the data already accumulated, or by manipulating and re-organizing a set of known data points—in a word, by trying to be more intelligent than our predecessors. The sort of synthesis I have in mind requires broader and richer data, drawn from a wider variety of sources and measured by more precise techniques.

This report is devoted to the resolution, in this spirit, of one such opposition—the Neogrammarian controversy. It is now formally 103 years old, and perhaps the most clearly stated issue in our history: In the evolution of sound systems, is the basic unit of change the word or the sound? At the outset, I will be concerned only with the question of the regularity of change, as it was stated in categorical form by Osthoff & Brugmann 1878:

> ... every sound change, inasmuch as it occurs mechanically, takes place according to laws that admit no exception.' (translated in Lehmann 1967:204)

The other side of the Neogrammarian position will ultimately be involved as well: that sound change is conditioned only by phonetic environments, and not by grammatical or semantic factors.

If we consider first this main issue of the regularity of sound change, it seems clear that, until recently, the Neogrammarians had won the day. While dialectologists, philologists, and scholars generally were still impressed with the facts that pointed to the slogan 'Each word has its own history', the mainstream of linguistic theory seems to have been Neogrammarian throughout this first century of the controversy. This holds not only for the American structuralists, and the absolute stance taken by Bloomfield 1933 and Hockett 1958, but for the most recent mainstream as well. In the various attempts to apply generative phonology to historical linguistics, the dispute with the Neogrammarians has been over the question of grammatical conditioning, not over the regularity of sound change (Postal 1968, King 1969, Kiparsky 1971).[3] Thus it was possible

[3] Thus, in the discussion which follows of the splitting of short *a* in Philadelphia, a generative treatment would have no difficulty with a rule operating on affective adjectives ending in /d/—*mad, bad, glad,* and *sad*; but a distribution that included only the first three could not be handled by any rule that was significantly different from a lexical inventory (see Kiparsky).

for Hockett 1965 to place the Neogrammarian hypothesis among the four great breakthroughs of linguistics, on a par perhaps with the theory of evolution in biology. Recent years have seen a series of re-publications, translations, and re-assessment of the Neogrammarians; so I need not review their achievements, their linguistic arguments, or their ideology.[4]

However, the evidence in favor of the belief that sound change proceeds word by word has not disappeared. Interest has persisted in the work of classical opponents of the Neogrammarians, beginning with Schuchardt.[5] Moreover, new (and much more decisive) evidence for lexical diffusion has been accumulated in a series of imaginative research efforts spearheaded by students of Chinese. Since that work is not so well-known as that of the Neogrammarians, I will begin here with a review of the recent research on lexical diffusion by W. S-Y. Wang, C-C. Cheng, M. Chen, H-I. Hsieh, Bh. Krishnamurti, and others, and restate their confrontation with the Neogrammarian position. This review will indicate that the traditional calm acceptance of the regularity of sound change can be maintained only by ignoring these linguists, together with their results. If we are not willing to do that, then—as I will show—we will be faced with the massive opposition of two bodies of evidence: both are right, but both cannot be right.

2. LEXICAL DIFFUSION. Wang 1969, on 'Competing sound changes as a cause of residue', suggested that exceptions to regular sound change might be caused by the overlapping operation of two rules in a bleeding relationship. From the standpoint of the Neogrammarian hypothesis, this was a new idea: irregularities might be the result of two regular sound changes, rather than the competition of sound change and analogy. This was a fairly abstract paper, based on several examples of how such intersection might have operated in the past. As Wang began to gather empirical evidence for this idea, he discovered data that had more serious consequences for the Neogrammarian position—findings of considerable scope that threw doubt on the whole idea of change by regular phonological rule.

It was only natural that Wang would turn to his native language for an empirical base. In 1962, Peking University published the *Hanyu Fangyin Zihui*, the results of a massive research project of the 1950's, with phonetic transcriptions of 2,444 morphemes in seventeen modern Chinese dialects. With data from the Middle Chinese Dictionary and Sino-Japanese sources, these materials formed the base of the Dictionary on Computer, or DOC (cf. Streeter 1977). Wang, together with Hsieh, Cheng, Chen, and others, used this data set

[4] Most American linguists have been introduced to the Neogrammarians through the account in Pedersen 1962. Lehmann includes translations of a number of important Neogrammarian documents. A set of the most important papers debating the Neogrammarian position is reproduced in Wilbur 1977; and the introduction to that volume gives a detailed account of the academic setting of the controversy. Jankowsky 1972 deals with the achievements of the Neogrammarians in a larger sense. For recent scholarly reviews of the controversy, I draw on Hoenigswald 1978, Malkiel 1967, and Fonágy 1956.

[5] Schuchardt's statement 'Gegen die Junggrammatiker' is reproduced by Wilbur. A more complete presentation of his point of view is available in Schuchardt 1980.

to trace the paths followed by Chinese sound changes. As Wang has pointed out, Chinese data are particularly useful for testing the Neogrammarian hypothesis because the morphological analogies that can interfere with the regularity of sound change in inflectional paradigms are practically non-existent.

It quickly became evident that the exceptionless character of sound change received very little support from Chinese data. One of the most concise statements of the position that emerged is given in Wang & Cheng 1977. They analyse the Neogrammarian position, summarized in the Bloomfieldian dictum that 'Phonemes change', into two components: sound change is PHONETICALLY GRADUAL, proceeding by imperceptible increments, but LEXICALLY ABRUPT, affecting all relevant words simultaneously. They point first to the unsuitability of this model for a wide range of discrete phonetic changes: flip-flops, metatheses, epentheses, deletions, and changes in point of articulation. Given this limitation, plus the existence of many competing forms and exceptions and the artificiality of many explanations of dialect borrowing, they propose an alternate model:

> 'we hold that words change their pronunciations by discrete, perceptible increments (i.e. phonetically abrupt), but severally at a time (i.e. lexically gradual) ...' (150)

They call this conception LEXICAL DIFFUSION. They do not deny that sound change may be regular: in this respect, lexical diffusion may predict no less ultimate regularity than the Neogrammarian principle. However,

> 'The difference lies rather in the description (and ultimately, the explanation) of the change mechanism, i.e. how the change is actually implemented.' (151)

For Wang & Cheng, lexical diffusion is plainly more than a working principle: it is a substantive solution to the transition problem. They support their position with an impressive demonstration (Cheng & Wang 1977) of lexical split in the reflexes of Middle Chinese tone III in the dialect of Chao-Zhou. No matter how narrowly the phonetic environments are analysed, the split into modern tones 2b and 3b persists. Neither the Middle Chinese initial consonants or final vowels nor the modern initials or finals explain the massive splitting of word classes. Table 1 shows a typical distribution of Chao-Zhou tones after modern

	CHAO-ZHOU TONE	
	2b	3b
MC initial b	6	7
v	1	3
d	11	14
dz	6	2
z	3	3
ḍ	3	4
dẓ	1	3
ẓ	3	5
dj	2	1
g	6	4
γ	14	15
Total	56	61

TABLE 1 (from Cheng & Wang, 94).

initials. Furthermore, Cheng & Wang locate twelve pairs that were homonymous in Middle Chinese but are now split in this way.

The Chao-Zhou case is a dramatic example of an even split without phonetic motivation—and with no analogical or grammatical motivation. Many other cases are documented in the various articles that flowed from the DOC research, now collected in Wang 1977. It is interesting to consider what kind of response might be made to these results from a traditional Neogrammarian viewpoint. At the 1969 meeting of the LSA in San Fransicso, Matthew Chen and Hsin-I Hsieh delivered a paper on 'The time variable in phonological change'. Several Indo-European scholars rose and pointed out that these data had no application to the regularity of sound change, since it was obvious from the materials presented that there must have been extensive dialect borrowing in 13th century Chao-Zhou, and so on. But this response has little explanatory value, since it is not true that the splits are randomly distributed throughout the lexicon. As the Chao-Zhou case indicates, they are concentrated in certain etymological classes. If the over-all rate of borrowing in the Chao-Zhou lexicon matched the 50% of Table 1, then the argument from dialect borrowing might be considered; but this is not the case. No process of dialect borrowing is known that is sensitive to etymological classes to the point that borrowings are specialized to reflexes of Middle Chinese tone III.

Evidence for lexical diffusion is not confined to Chinese: Wang 1977 presents arguments based on Swiss German, Classical Tibetan, Old Welsh, and Swedish, as well as discussions of acquisition in English and Chinese. In another statement, Chen & Wang 1975 have also drawn on Sherman's 1973 study of the historical development of English fore-stressing of nouns derived from verbs.

Perhaps the most impressive demonstration of lexical diffusion since the DOC work has come from a new direction: Krishnamurti's 1978 analysis of the development of Dravidian consonant clusters, using computational methods on the data from Burrow & Emeneau 1961. Krishnamurti must be considered an impartial newcomer to the discussion; and his results leave no doubt that these sound changes proceeded with the word, not the phoneme, as the basic unit.

Thus we have arrived at a situation where no reasonable person can maintain what might be called the Neogrammarian dogma: that sound change is always gradual, always regular, affecting all words at the same time. The question is now whether the Neogrammarian position retains any substantive value. Are SOME sound changes regular and lexically abrupt? Wang comes close to saying no. Chen & Wang (257) state:

> 'This lexically gradual view of sound change is incompatible, in principle, with the structuralist way of looking at sound change.'

Here they are looking at the rigidity of the doctrine that 'Phonemes change.' They then add:

> 'No one has seriously contended that phonological processes operate abruptly and transform the entire vocabulary overnight.'

This statement reflects the other half of the lexical diffusion view, that change

is phonetically abrupt. Chen & Wang seem to be ruling out the major possibility advanced by the Neogrammarians, that the entire relevant vocabulary is affected by gradual, even imperceptible shifts in the realization of a given phoneme. Nowhere in the writings of the lexical diffusionists do we find evidence for such a change, or serious consideration that such changes might occur. Yet Wang himself has been increasingly careful to avoid the dogmatic style of the Neogrammarians. In his most recent statement (1979:69), he argues that the next step is not to continue piling up evidence for lexical diffusion, but rather to begin a more general program of research on the transition problem: 'Our next challenge, it seems to me, is to solve the puzzle of what kind of sound change would travel along which path for its implementation.' It is in this judicious spirit that we turn to reconsider the nature of the evidence for regular sound change.

3. THE CURRENT STATUS OF THE NEOGRAMMARIAN POSITION. At this point, I must raise the question whether the title of this communication is not out of date, for the reason that Hoenigswald 1978 may in fact have already resolved the Neogrammarian position. He has brought to a precise statement the position he has presented for a number of years: that the Neogrammarian hypothesis was not a substantive statement about sound change, but a working principle that DEFINED sound change.

Hoenigswald points out that, if one considers the practice of the Neogrammarians, rather than their ideological statements, it appears that what they were doing was motivating the choice between two competing regularities found in the course of comparative reconstruction. One was to be called sound change; the other, analogy or dialect borrowing. Thus sound change is defined as a certain kind of object. The argument may seem circular; but, as Hoenigswald suggests, it is a useful kind of circularity that may clarify our understanding of what we are doing in historical reconstruction.

This analysis may seem especially congenial in the light of the evidence reviewed above for lexical diffusion as the primary mechanism of sound change. The Neogrammarian hypothesis can remain as a way of identifying the regular correspondences that result when all the words that have followed a change are finally gathered into a single class, and then as a way of using that regularity as a base for constructing language relations.

What then remains of the Neogrammarian controversy? Are we to put aside the long list of articles that vigorously argued for one side or the other? On the one side are Curtius, Delbrück, Osthoff, Paul, Saussure, Bloomfield, Hockett; on the other, Schuchardt, Gilliéron, Jespersen, Sturtevant; and on all sides, commentators too numerous to mention. If Hoenigswald is right, the time and ink devoted to the Lautgesetzfrage has been spilt on a misunderstanding.

Perhaps, but it seems to me that a substantial question remains. The ideological problem may not have determined real practice, or even reflected that practice; but it was actively used to encourage some kinds of research and discourage others. One may argue that regular sound change and analogy are always present, even co-present; but, as Malkiel 1967 points out in his insightful

review of the competing principle that 'Each word has its own history', it makes quite a difference if we have 90% of one type or 90% of the other:

> 'In such a climate, the classic "sound laws" would not necessarily be abolished (in fact, their residue might be doubly important for genetic reconstruction), but they would, psychologically, lose a great deal of their immediate appeal, and certainly would no longer, without grave damage, dominate the scene of linguistic research.' (140)

I met the ideological use of the Neogrammarian principle early in my own work, in Goidanich's 1926 reaction to Gauchat's 1905 description of sound change in progress in Charmey. Gauchat found variation that did not fit in with the Neogrammarian conception of sound change; e.g., in the lenition of /l'/, the oldest generation used [l'] and the youngest used [j], but the middle generation used both. Goidanich argued that Gauchat could not have observed the true sound changes that lenited [l'] to a glide, but only borrowings from the older and younger generations. This negative reaction to the findings of dialect geography echoes the earlier pessimism of Delbrück, who thought it might be very difficult to assess 'how great the uniformity will be within the sounds of a homogeneous language' (1885:117), and concluded that the exceptionless character of sound laws could only be derived deductively, never by induction. Delbrück defined the empirical problem as follows:

> 'From what an individual speaks or would speak at a definite moment in his life, if he allowed the whole mass of his vocabulary to pass through his vocal organs, we must subtract all that can be regarded as borrowed (in the broadest sense) and then all phonetic formations that depend on analogy.' (129)

The empirical task is defined as an impossible one. Following a parallel logic 70 years later, Hockett (1958:444) concluded that sound change was too slow to be observed, and phonological change was too fast to be observed. In 1968, Weinreich, Labov & Herzog reviewed this long decline from Osthoff & Brugmann's enthusiastic endorsement of the results of dialect geography. It appears that the pessimistic view of empirical possibilities set in quite early: it did not need the 20th century to discover the advantages of rejecting uncomfortable data.

It must be granted that this negative approach has successfully influenced the course of linguistic research. Dialectology has been isolated from general linguistics. Though Bloomfield himself engaged in the study of word history, his own chapter on dialectology was never reconciled with his chapters on phonetic change (as Malkiel pointed out in 1967). Scholars continue to search for universal principles by manipulating isolated examples—subtracting from the available data, rather than adding to them.

This was not the original idea of the Neogrammarians. Re-reading Osthoff & Brugmann, I realized that their enthusiasm for empirical research went far beyond their endorsement of Winteler 1876. There are remarkable parallels between the methodological issues that they confronted and those which we face today:

> '... only that comparative linguist who for once emerges from the hypotheses-beclouded atmosphere of the workshop in which the original Indo-European forms are forced, and steps

> into the clear air of tangible reality and of the present in order to get information about those things which gray theory can never reveal to him, and only he who renounces forever that ... method of investigation according to which people observe language only on paper and resolve everything into terminology, systems of rules, and grammatical formalism and believe that they have then fathomed the essence of the phenomena when they have devised a name for the thing—only he can arrive at a correct idea of the way in which linguistic forms live and change ...' (translated in Lehmann, 202)

It seems to me that Osthoff & Brugmann were on the right track—and that the discouragement of Delbrück, echoed by Hockett and many others, was premature. We can give new life to the Neogrammarians' ideas by opening up the field to the vast array of data provided by the study of sound change in progress.

4. THE STUDY OF SOUND CHANGE IN PROGRESS. The case for lexical diffusion was presented by Wang, Cheng, and Chen within a framework closely aligned with that of Weinreich, Labov & Herzog: the empirical foundations for a theory of language change must include the capacity to deal with the 'orderly heterogeneity' that is a fundamental characteristic of language. Diffusion across the lexicon is one example of that orderly variation. The case that Weinreich originally developed for this view of language also involved a detailed critique of the same Neogrammarians—with the difference, to be sure, that this critique was directed at the view that the individual was the sole repository of regularity in language, rather than at the regularity hypothesis. I was not only impressed with the proofs of lexical diffusion, but I also had every reason to be sympathetic to this theoretical orientation.

However, the critique of the Neogrammarian position presented by the proponents of lexical diffusion tended to focus on the slogans or ideological positions published in the course of the controversy, rather than to review the evidence on particular sound changes. The literature on past and present changes in IE languages is so large that one can hardly blame the specialists in Sino-Tibetan linguistics for not immersing themselves in it. But there have been many strenuous efforts by scholars to assemble this evidence and assess its impact on the issues. The most useful for my present purpose is the remarkable work of Fónagy 1956, 1967.

Fónagy assembles data from over 60 studies of sound change in progress—concentrated in French, English, German, and Hungarian dialects—together with his own instrumental measurements of vowel lengthenings and shortenings in French and Hungarian; he also reviews a wider range of completed sound changes. He gives full weight to the critique of the Neogrammarian position that emerges from the evidence of dialectology. Much of his criticism of the Neogrammarians enlarges the position taken by Gauchat: it is directed at their portrait of sound change as a uniform, gradual process in a homogeneous community, where the old forms give way to the new without oscillation or variation. He also presents many studies that show lexical diffusion and grammatical conditioning of sound change. At the same time, Fónagy warns that the opposing slogan—that sound change proceeds from word to word—receives even less support from the available evidence. He points out that lexical conditioning is comparatively rare, and that sound change begins in the majority

of cases with the entire relevant vocabulary (1956:218–20, 1967:109). In fact, he shows that the assumption of homogeneity can lead dialectologists to exaggerate the case for lexical conditioning; when a wider range of data from the speech community is taken into account, words that appeared to be exceptions may turn out to differ only in the frequency of the new variant (1956:219).[6]

In the course of quantitative studies of sound change in progress, beginning in 1968,[7] several characteristics of the data have emerged that have turned my own thinking away from lexical diffusion and toward the Neogrammarian position. One argument for lexical diffusion revolves around the social importance of words: that they are the basic carriers of social significance. It is certainly true that, when people talk about sound change, it is usually in terms of particular words: the social correction of sound changes in New York, Philadelphia, or Chicago is focused on individual words, not sounds. But these corrections occur only in the late stages of a change, when it is all but completed, and they are remarkably unsystematic or sporadic, with none of the predictable and regular behavior of the original sound changes.

The irregular character of lexical correction is not an argument for lexical diffusion, but rather a reason to doubt that words are the fundamental units of change. The earlier stages of change appear quite isolated from such irregular lexical reactions; and even in a late stage, the unreflecting use of the vernacular preserves that regularity. Figure 1 shows the distribution of words in two classes in the speech of Leon Alinsky, 30, from New York City.[8] The change involved here—the raising of 'short *a*' in closed syllables—appears to date back to the middle of the 19th century.[9] In New York City it affects a specific set of words in closed syllables that become long or tense, rising from low to mid and eventually to high position. The triangles, representing tense /æh/, are discretely separated from the squares and diamonds that represent the lax subset /æ/. Fig. 1a is Alinsky's pattern in reading a connected text; the two word classes are brought close together, but the distribution of spontaneous speech is preserved. Figure 1b shows the measurements of the nuclei pronounced by the same speaker in the reading of word lists. Three words remain in the raised position: *pass, bad, bag*, and perhaps *dance*. The laxing of *half,*

[6] The case he presents is the observation of an 11-year-old boy by Remacle in his 1944 study of the alternation of intervocalic /h/ with zero in the 'Ardenne Liègoise' of Regne-Bihain. The boy showed lexical variation at first; however, on repetition, it became evident that stylistic variation affected all words. The /h/ was used most often in careful speech, and some words were used more often in casual style. The case is thus most comparable to the study of postvocalic /r/ in New York City (Labov 1966).

[7] Research supported by the National Science Foundation under contract GS-3287, reported in Labov, Yaeger & Steiner 1972 [LYS], and under Contract SOC 75-00245 and BNC 768-0910, reported in Labov et al. 1980 and a number of other publications.

[8] From LYS, Fig. 3-3. This is a doubly linear display of F1 and F2 of the nucleus or steady state of the vowel as measured on spectrographs (LYS, Chap. 2).

[9] Real-time data on this change in progress date back to the end of the 19th century. Babbitt 1896 described the raising of the words in the 'broad *a*' class to mid front position, perhaps the same subset now characteristic of Philadelphia.

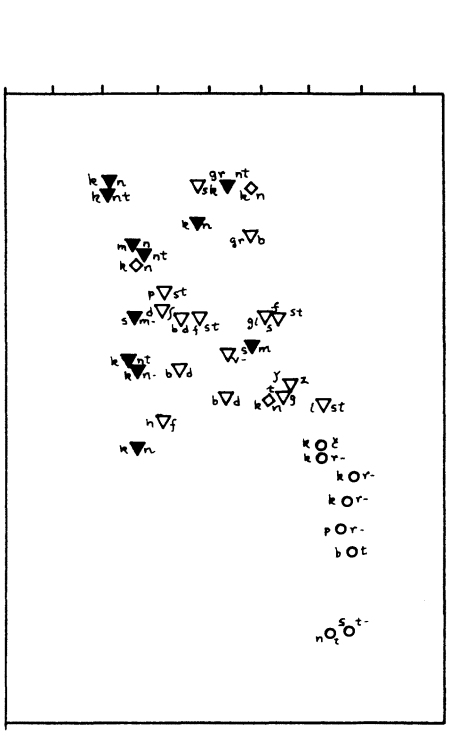

FIGURE 1a.

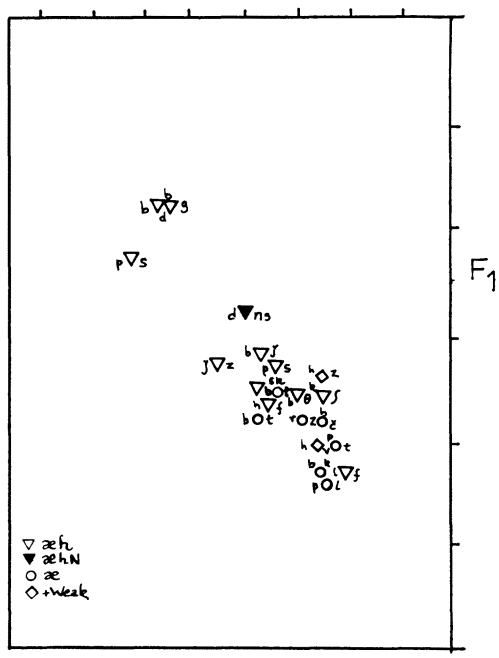

FIGURE 1b.

ask, laugh, and the other token of *pass* is just as inexplicable and unpredictable as the splitting of Chao-Zhou tone III.

Several cases of lexical diffusion cited in Wang 1977 involve such late stages in the correction of a socially stigmatized variable. Thus Janson gives clear evidence that the deletion of final /d/ in modern Stockholm Swedish is the crystallization of a stereotype of the 19th century:

> 'Out of the fifty-two words that were marked by at least two of my informants [as possible deletions of /d/] ... this [1889] dictionary reports forms without /d/ in forty-six cases, and invariably assigns them to one of the lower levels of style [solemn down to vulgar].' (257)

The other observation that turned my thinking toward the Neogrammarian point of view is more general. If we set aside corrections in the late stages of change, and consider the vowel shifts in spontaneous speech—fronting, backing, raising, lowering, rounding, unrounding, nasalization—in New York, Detroit, Rochester, Buffalo, Chicago, Atlanta, London, Birmingham, Norwich, Cardiff, Glasgow, Edinburgh, and Belfast, then we find regular, phonetically conditioned sound change. To quote LYS:

> 'The kinds of sound changes we have tracked through this phonetic space appear to be typical of the regular processes which formed the basis of the Neogrammarian view of sound change.' (260)

Three characteristics of Neogrammarian sound change are prominent and ob-

vious to our inspection. First, every word in a given historical class is affected; e.g., the raising of /ohr/ does not involve just the common *door, four, more*, but every lexical item in this class that we meet in spontaneous speech, no matter how common or uncommon, learned or vulgar: reading from the records of Leon Alinsky, I find *born, forth, fort, horns, source* ... None of these remain at the original cardinal [ɔ] or go off in another direction, but all move steadily upward toward [u:ə]. We have never found it necessary to mark certain members of a word class as lexical exceptions. This holds for several dozen sound changes in process studied over many speech communities.

Second, these changes appear to be gradual. It is difficult, of course, to prove the absence of discontinuities. To compare speakers across age levels, we must superpose one vowel system on another; and such superposition is hardly precise enough to establish continuity or discontinuity. Only recently have we made enough progress on the normalization problem to allow us to superimpose several hundred mean values, and to derive the age coefficients that show the relative rates of change (Labov 1978, Labov et al. 1980). Problems of incomplete or over-normalization could easily mask a discontinuous distribution. But conversely, every effort we have made to find discontinuities, where we most expected them, has failed.[10]

Third, the changes in progress show the most detailed kinds of phonetic conditioning, with no indication of grammatical constraints.[11] New and vigorous changes show extended, elliptical distributions in the direction of the change, as opposed to stable vowels which show more globular distributions. Within these elliptical distributions, we find a fine subdivision of phonetic classes, as almost every feature that might favor or disfavor the shift comes into play. LYS used Figure 2 to illustrate this point: the distribution of tense (æh) for Bea Black, 54, of Buffalo. As one of the Northern cities, Buffalo shows an unconditioned tensing of all short *a*'s; but the degree of raising is finely determined by phonetic environment. Following nasals (the solid triangles), they are always higher and more peripheral. Below these, the effect of initial /l/ lowers F1.[12] Next in peripherality is (æh) before voiceless fricatives, with the consonant cluster [skt] of *asked* producing further centralization. Words with following voiceless stops are less peripheral, with a finely-graded ordering by place of articulation: palatals are regularly highest (see *batch*); apicals are next with *sat*; then come labials and velars, as in *trap, back*. Initial clusters of obstruent plus liquid lower the F1 and F2 of the vowel nucleus,

[10] One of the most likely sites for such a discontinuity was in the tensing and raising of the nucleus of (aw), which was closely identified with [æ] for the oldest speakers in Philadelphia: we anticipated a discrete jump to an identification with the mid or lower high nucleus of tense (æh), but we found no evidence for this. The (aw) nucleus sometimes overlaps with (æh) but is usually lower, even when both vowels show parallel in-glides.

[11] This statement will be modified in respect to certain sound changes, such as that of short *a* to be discussed below in detail. LYS distinguished the raising of short *a*, which showed this regularity, from the tensing process, which did not.

[12] Initial /l/ shows a tendency to lower both F1 and F2, though not to the extent of final /l/, or of /l/ after an initial consonant. One of the tokens in Fig. 2 shows this effect; the other does not.

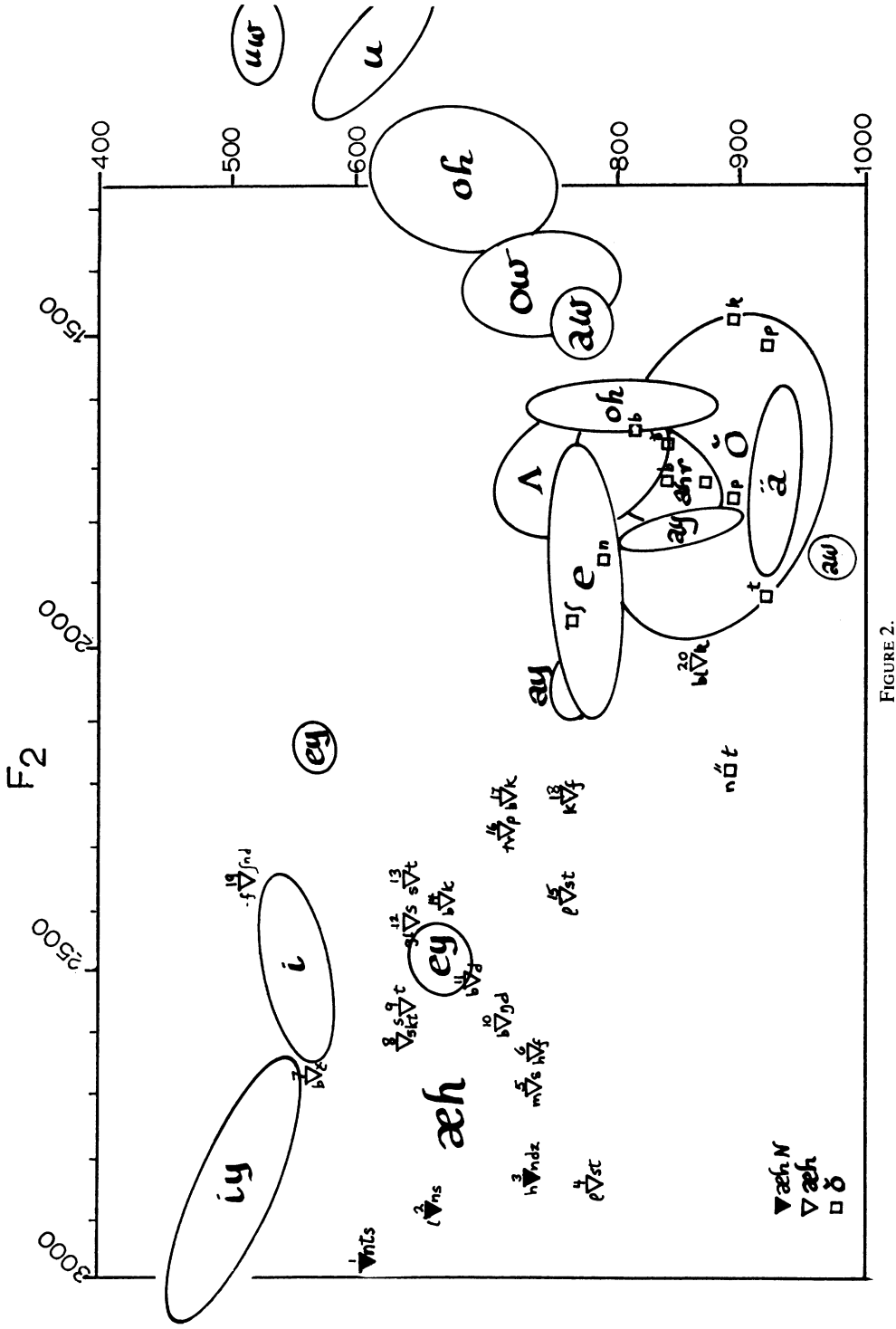

FIGURE 2.

particularly when the obstruent is voiced. Following velars also lower the F2 of the vowel nucleus, so that the least advanced form is predictably *black*. On Fig. 2, this word is found as usual in a low central position, considerably back of the most forward of the short *o* words, *not*. Buffalo is involved in the 'Northern Cities shift' /oh/ → /o/ → /æh/ → (LYS, Chap. 3); and *not* appears here as the most advanced member of the second unit in the chain shift: an initial nasal and following apical provide the most favoring environment for the raising of F2.

It might seem that this case approaches the endpoint where the Neogrammarian hypothesis merges with the view that every word has its own history. If every phonetic feature of the environment affects phonetic development, then homonyms would form the only word classes. This is not the case, since many initial consonants have such slight effects on the phonetic realization of the vowel that they are lost in the random noise of subtle differences of stress or duration and of errors in measurement. But even if every word had its own history, this would not necessarily be lexical diffusion. Lexical diffusion implies a rejection of the idea that phonetic conditioning fully accounts for sound change: that there are at least some words whose behavior is not predicted by their phonetic composition. If the word is a fundamental unit of change, it is because some words undergo the change for reasons that are not phonetic.[13]

5. ARE HOMONYMS SPLIT? We could hardly consider the illustration just given as a proof that lexical diffusion is entirely absent from the sound changes being examined. Lexical identity is certainly not a major factor. But it might still play a minor role—so that, over the course of a century, some words might gradually advance their position, or fall behind to be eventually filtered out and become members of another class. In the course of our study of linguistic change and variation in Philadelphia (hereafter LCV; see Labov et al. 1980), we searched for a precise way of testing this possibility.

The ideal test would be to measure a large number of words in the speech of one person, over a good stretch of time, and then carry out a regression analysis in which the identity of particular words would be entered into the equations—along with phonetic, prosodic, and social factors. This would require a fair number of items; otherwise, a non-significant result would be inevitable.

Interviews conducted in Philadelphia neighborhoods often involve two, three, or even four hours with a single speaker, and several series of recordings are made in most cases. But even then, the numbers of tokens of any one word are limited. If we were fortunate enough to find homonyms in spontaneous speech, we could hardly expect to find more than a few of each pair. The reading of word lists will give us all the lexical comparisons we need; but the absence of lexical diffusion in such lists would prove very little about the course

[13] Lexical diffusion does not imply that the selection of words is entirely random: e.g., frequency is often cited as an important factor. The crucial point is that no phonetic (or grammatical) explanation exists.

of change in progress.[14] We could, of course, carry out the analysis on the entire set of normalized tokens for the 176 Philadelphia speakers that we have analysed; but we have no independent way of assuring that any lack of significance in the effect of lexical items would not be caused by limitations of the normalization procedure.

The solution to our problem lies in a unique data set. In the exploratory stages of LCV, Arvilla Payne made a series of recordings of one speaker, Carol Meyers, 30, over the course of an entire working day in a travel agency, followed by dinner at home with her family and a bridge game at night with close friends. Payne was then living at the Meyers' home, and knew all the people involved quite well, so that the effect of observation was reduced to a minimum.[15] Hindle 1980 is a quantitative analysis of the effects of addressee, work situation, key, and many other independent variables on the formant positions of 3,600 vowels spoken by Carol Meyers.

To test the possibility of lexical diffusion in this system, Hindle and I located two word classes where the largest sets of homonyms exist along with the largest sets of near-homonyms. These are free (uwF) and free (owF): the subsets of /uw/ and /ow/ words with no final consonants. One of the special features of the Philadelphia Pattern 3 chain shift is the sharp differentiation of checked and free allophones in the upgliding high and mid vowels.[16] The combined data of earlier studies, the Philadelphia neighborhood studies, and a telephone survey show that (uwF) and (owF) are indeed involved in change in progress. The (uwF) class is older, from the stratum of nearly completed changes that show minimal age-grading today: the neighborhood studies show an age coefficient for F2 of -1.57 Hz per year. The (owF) class is younger, in a middle-range group of changes, with a more vigorous age-grading of -3.54 Hz per year. This figure means that we can expect, all other things being equal, that a twenty-year-old speaker will have an F2 mean for (owF) that is 142 Hz higher than the mean for a sixty-year-old speaker.

Figure 3 shows the vowel system of Carol Meyers as analysed by Hindle. Each vowel shows not one but three means, for each of the three major social settings: the travel agency, dinner at home, and the bridge game. Free (uwF) is well front of center—not yet as far front as /i/, but well ahead of checked (uwC). As always, (ow) runs behind (uw), and (owF) is ahead of (owC). The

[14] One sort of bias in word lists is shown in Fig. 1b, typical of the late stages of a stigmatized change. Where there is less social affect, word lists can show the opposite effect: a concentration on an advanced target, eliminating much of the phonetic (and perhaps lexical) variation characteristic of spontaneous speech (LYS, Fig. 3-18; Yaeger 1975).

[15] The recordings of Carol Meyers were made with Nagra IVD and IVS tape recorders and Sennheiser 404 condenser microphones. The quality of these recordings is unparallelled in our records for variety and spontaneity of social interaction, and for the fidelity of the signal from a physical point of view.

[16] Pattern 3 chain shift (LYS, Chap. 3) involves the raising and backing of /a/ → /ɔ/ → /o/ → /u/ along with a variety of fronting patterns in vowels with /u/ and /o/ nuclei. It is found throughout Southern English, Midland and Southern American dialects, and in a variety of languages in Western Europe. This is the type of chain shift used by Martinet 1955 to illustrate his concepts of economy and function in the vowel systems.

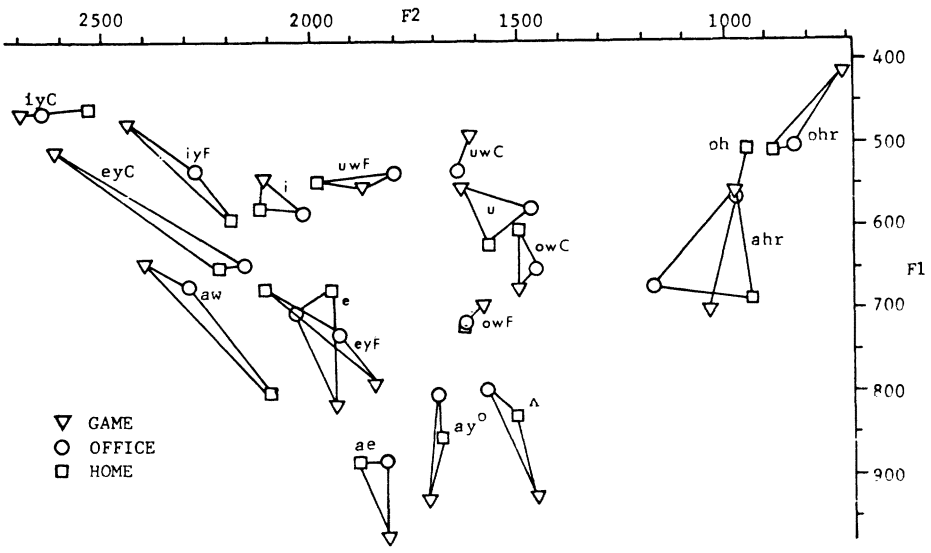

FIGURE 3.

two free classes contain many words of high frequency; and since they don't differ in final consonants, we have a good chance of finding homonyms. In particular, we can expect a fair number of tokens of *two* and *too, know* and *no*.

Table 2 lists, for Carol Meyers, all words in these classes with 3 or more tokens, and gives their mean F2 values.[17] In the case of (uwF), we are fortunate to have 40 tokens of *two* and 14 tokens of *too*. They differ by 61 Hz in F2, the

		N	MEAN F2
(uwF)	*two*	40	1743
	too	14	1682
	do	19	1743
	to	5	1842
	through	3	1879
(owF)	*know*	50	1574
	go	38	1548
	no	32	1573
	oh	16	1587
	okay	27	1554
	so	15	1585
	goes	6	1591
	though	5	1701
	show	4	1461
	lower	3	1509

TABLE 2 (N>2).

[17] The total number of tokens in the (uwF) category is 95; in (owF), 215. The three most common items analysed for each class represent a sizeable percentage of this total, 77% and 56% respectively.

crucial dimension. However, we do not know from the mean values alone if this difference is significant, since the two sets of tokens may differ in duration, stress, pitch—or, most importantly, in their distribution by setting, since there is a 220 Hz difference between home and office means of (uwF). We also have the opportunity to examine *do*, which is almost identical to *two* in F1 and F2 measurements, and differs phonetically only by the voicing of the initial consonant.

In the (owF) class, we find 32 examples of *no* and 50 of *know*, with almost exactly the same F2 value. We must be particularly careful with the word *no* to exclude the possibility that prosodic factors play a role, and that the apparent identity of mean values masks compensating differences between the two words. We also have *go*, which differs only by the /n/–/g/ contrast, with 38 tokens, about 27 Hz behind. This again is a small difference, compared to other factors.

A stepwise multiple regression analysis of these data allows us to distinguish the actual effect of phonetic environment from the effect of lexical identity. The Carol Meyers data then permit a precise test of the hypothesis that the fundamental mechanism of the fronting of /uw/ and /ow/ is the differential advancement of individual words. There is ample evidence for powerful phonetic conditioning: following /r/ prevents this fronting altogether; following /l/ similarly interferes with the process, yielding differences of 500–1500 Hz between allophones before /l/ and not before /l/. Checked syllables are well behind free syllables, with distributions that overlap not at all or very little. Within the free (owF) and (uwF) classes, finer phonetic conditioning can be anticipated—though weaker than that of Fig. 2, since preceding consonants generally have less effect than following ones. The contrast of *do–to* and *go–know* can be taken as representative of such finer phonetic differentiation. If the change is in fact proceeding word-by-word, we would expect that the differences between the two sets of homonyms would be at least as large as the smaller phonetic differences, and perhaps as great as the more powerful phonetic factors.

Table 3 shows, for Carol Meyers, the results of the stepwise regression analysis on F2 of (uwF) and (owF), including as independent variables the duration of the vowel, its fundamental frequency, stress, position in the phrase group, and the identity of the lexical item. The words *two* and *do* are opposed to the residual class, *too*.[18] The results show five coefficients, each about 100 Hz in magnitude, and each at about the .05 level of significance. The prosodic factors are in the direction we would expect for a change in progress: the minimum constraint on articulation yields the most advanced form. Thus duration favors higher F2, and secondary stress disfavors it. The social factors point in the same direction: the office setting disfavors higher F2, while the

[18] Qualitative variables like word class are treated as dummy variables in regression analysis: membership in each class is given the value 1, except for one class which is taken as the residual (0) member of each opposition, and all effects are calculated in terms of differences from this residual class.

	Coefficient	*t*
(uwF)		
Prosodic factors		
Fundamental frequency	—	
Duration [× 100 msec]	91	2.3[a]
Secondary stress	−96	2.0[a]
Word-final position	85	1.6
Social setting		
Office vs. game	−86	1.9[a]
Home vs. game	116	1.8
Lexical items		
two vs. *too*	—	
do vs. *too*	103	2.5
(owF)		
Prosodic factors		
Fundamental frequency	—	
Duration [× 100 Hz]	—	
Secondary stress	−72	2.3[a]
Phrase-final position	−69	1.9
Social setting		
Office vs. game	47	1.5
Home vs. game	−74	1.9
Lexical item		
know vs. *no*	—	
go vs. *no*	−165	4.4[b]

TABLE 3.
[a] $p < .05$. [b] $p < .001$.

home setting favors it. Finally, we observe no difference at all between the two homonyms—and, contrary to the unanalysed means, a fairly strong advantage of 103 Hz for *do* over *too*.

The findings for the (owF) class are somewhat different in the prosodic factors. Neither pitch nor duration is significant. Secondary stress again disfavors F2 as compared to primary stress; and phrase-final position also disfavors it. The home setting disfavors F2 as against the bridge game, typical of a more active stage of change. Finally, we observe no difference between the homonyms *know* and *no*, but a very large effect that predicts lower F2 values for *go* as against *no*.

The results of these two analyses are the same on the main point of interest. The homonyms show parallel behavior, with no significant differences in expected formant positions. The largest significant differences appear for words with different initial consonants, which may or may not represent phonetic conditioning. We cannot yet explain the *do* effect, since we have not identified any general influence of preceding voicing. But the lower F2 predicted for *go* as compared to *no* does fit in with all previous results that show higher F2 associated with initial nasals as well as final nasals. Thus *mad* regularly shows higher F2 than *bad* in Philadelphia; and Fig. 2 illustrates the higher F2 typically found with *not* in Northern Cities vowel systems.

This is a test of only two homonym pairs; but it is a sensitive test, more

closely connected to the mechanism of change than the analyses of completed changes in the DOC. We fail to find any evidence for lexical diffusion in these two changes in progress; all available evidence points to phonetically conditioned, gradual sound change in the spirit of the Neogrammarian proclamation.

One cannot doubt the conclusions drawn from the DOC. But our instrumental measures of sound change in progress give strong support to the Neogrammarian position. How can we reconcile two masses of evidence that point in opposite directions?

6. THE SPLITTING OF SHORT *a*. Most of the sound changes I have discussed so far are either unconditioned (like the raising of /æ/ in the Northern Cities) or conditioned by a simple and straightforward phonetic rule (like the fronting of /uw/ and /ow/ in Philadelphia, which applies to all members of the original word classes except those with final liquids). These are all output rules: movements in a relatively concrete phonetic space, not conditioned, bled, or further affected by any other processes. The case of short *a* in the Middle Atlantic states is quite different. There the raising and tensing affects only a subset of /æ/, following a complex set of conditions that vary systematically as we move from New York to Philadelphia to Baltimore.

The fronting and raising of short *a* continues a process that began in the 10th century. An earlier raising of long /a:/ to /ɔ:/ (in *boat, stone* etc.) had left a hole that was filled by lengthening of short *a* in open syllables. Fronting, raising, and participation in the Great Vowel Shift led to a merger of *name, grave* etc. with several other classes in modern /ey/. The residual set, all in checked syllables, is now being affected in all American dialects. The most favored subset for raising are words ending in final front nasals: *hand, man, ham* etc., which are raised almost everywhere.

There is a sizeable literature on the Middle Atlantic raising, beginning with a series of articles by Trager (1930, 1934, 1940), Cohen's 1970 analysis of New York City and surroundings New Jersey, and Ferguson's 1975 description of the Philadelphia system. It is the most complex conditioning of phonetic processes that I know of, and the most closely studied.

In these dialects, a set of words with short, low, non-peripheral [æ] is opposed to another set with nuclei that are fronted to a peripheral position—generally mid to high, long, with a centering in-glide, i.e., [e:ᵊ]; I will refer to this opposition as lax vs. tense. Figure 4 shows the set of following consonants that yield tense vowels (when the next segment is [+consonant] or a # boundary): the solid line outlines the New York City system, and within this, the dotted line shows the Philadelphia system, a proper subset of New York's. The Philadelphia set is close to the minimal or core set that conditions the tensing of low vowels in English generally—front nasals and voiceless fricatives.[19]

To this basic conditioning, we must now add a set of special phonetic,

[19] This environment of nasals and voiceless fricatives appears to be the most favoring environment for the tensing of English low vowels in general. As Ferguson first pointed out, it is the environment for the broad *a* class as well as the short *a* class. When we make the obvious adjustment of front nasals to back nasals, it applies to the tensing of short open *o*.

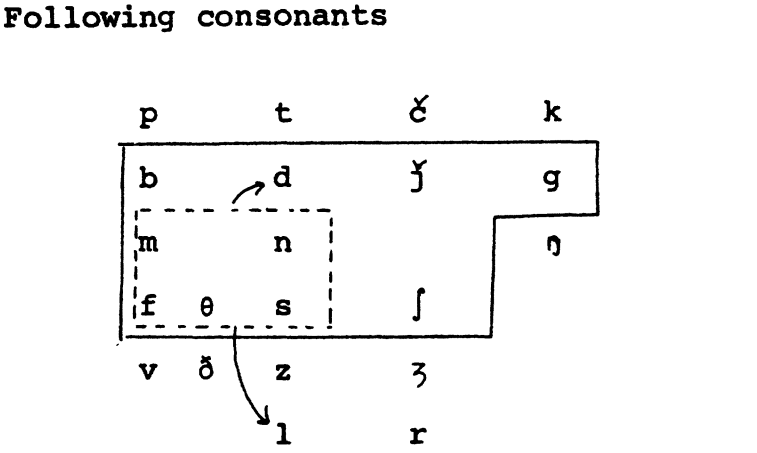

FIGURE 4.

grammatical, and lexical conditions. It will be useful here to consider the relation of the Philadelphia subconditions to the Neogrammarian hypothesis, and to see how the sound change that led to this situation might fit into that framework.

(1) The segment that follows after the following segment must be either an obstruent or an inflectional (or word) boundary. The boundary condition mentioned here is clearly grammatical information: can it be reconciled with the Neogrammarian position? By the general rule, *man* is tense, both as noun and verb, and *manner* is lax (since a vowel follows the consonant directly). But *manning* (as in *Who is manning the store?*) is tense, as the inflectional boundary after the consonant dictates: /man≠ing/. There is no problem for the Neogrammarians here, since this type of grammatical information can plainly be attributed to analogy: tense /æh/ occurs in the participle *manning* by analogy with the simple verb *man*.

(2) The vowel in question cannot be in a 'weak word', i.e. one whose only vowel can be shwa. This condition is stated as if it were a phonetic condition, but weak words are of course a subset of 'function words': auxiliaries, articles etc. The classic contrast is tense *tin can* vs. lax *I can*. But any grammatical implications of this condition can also be handled by analogy. Auxiliaries such as *am* or articles like *an* have shwa in normally unstressed position, and shwa

is a lax vowel; the marked stressed form, *I am*, can be said to be lax by analogy with the unstressed form.

(3) If a derivational boundary follows the consonant after the vowel, there is considerable variation in the frequency of tensing. Thus we find widespread variation in *Lassie*, [læsi] or [le:ᵊsi], and in *massive*, usually [mæsɪv] but possibly [me:ᵊsɪv]. But the variability might just as well be in the force of the analogy as in a grammatical condition, and this too can be put outside the scope of the Neogrammarian hypothesis.

(4) Strong verbs ending in nasals are retained as lax, contrary to the general rule. Thus Philadelphians show lax *ran, swam, began*, but tense *man, Dan, slam, understand* etc. There is some variation in the *ran, swam, began* class; but the condition that excepts irregular verbs ending in nasals from the general rule is a strong one. In Philadelphia, a vernacular preterit of the verb *win* has the form /æ/, and this is always pronounced with a lax vowel. This kind of grammatical information certainly cannot be handled in the Neogrammarian framework. Nor can it be characterized as a unique exception, in the light of the extraordinary finding of Toon's 1976 study of variability in the raising of West Germanic short *a* before nasals in Old English texts. In three sources—the Lindisfarne Gospels, the Rushworth Gospels, and the Durham ritual—Toon found that the sound change is complete except for Class III strong verbs, which are lax as a group. Thus, of the 106 tokens of these verbs in the Lindisfarne Gospels, 106 were lax. The Philadelphia *ran, swam, began*, /wæn/ are the surviving members of this class.

(5) All vowels followed by voiced stops are lax, except for *mad, had,* and *glad*, which are always tense. The three words involved are all common affective adjectives, and so we might want to construct some kind of general rule to account for them. But *sad*, another common affective adjective, is lax along with all other short *a* words ending in /d/.[20] This is massively regular for the entire Philadelphia speech community—a clear case of lexical diffusion, arrested in mid-career at some point in the past.

We must therefore concede that not all sound change in Philadelphia is Neogrammarian: one such process, at least in the past, did not share the Neogrammarian syndrome. To make further progress in understanding when regular sound change operates, and when sound change advances one word at a time, we will have to look more closely at short *a* in Philadelphia, to see what kind of rule is at work.

7. A SPLIT IN PROGRESS. By 1972, LCV workers had become fully aware of the need to differentiate sharply between the tensing of /æ/ to (æh) and the raising of the variable (æh). In LYS an abstract tensing rule, which showed grammatical conditioning and lexical irregularity, was distinguished from the

[20] The stability of *sad* in Philadelphia will be demonstrated below (Table 9). At the same time, there is no reason to doubt that the further progress of this sound change will make use of the obvious generalization. Some tendency to pronounce tense *sad* in word lists has been observed in Arvilla Payne's work in the Philadelphia suburbs.

lower-level, regular raising rule; and we warned that

> 'If there are at least two radically different types of rules which govern sound change, then we must be particularly cautious not to confuse generalizations about one with statements about the other.' (260)

The resolution of the Neogrammarian controversy would be accomplished, then, if we could specify what kind of rule the tensing of short *a* was or is—and, having established the general class, predict for any new case whether we are likely to find regular sound change or lexical diffusion.

I have used the term 'rule' for the tensing process; and it was in that framework that LYS approached the matter. But in the course of studying the evolution of the Philadelphia vowel system, LCV found strong evidence confirming the earlier conclusion of Trager 1940 and Cohen 1970: the Middle Atlantic differentiation of tense (æh) and lax /æ/ is not a complex phonological rule, operating on a single underlying form, but a lexical split into two phonemes, a distribution of two dictionary entries. This conclusion rests on three types of evidence.

7.1. UNPREDICTABLE DISTRIBUTIONS. Trager approached the problem of one phoneme vs. two in the framework of autonomous phonemics. Minimal pairs like *can* [N, V] vs. *can* [AUX], or lexical exceptions like NYC *avenue*, were not the only evidence. The fact that one could not predict whether *jazz* or *wagon* was tense or lax for any given speaker was the major factor that led Trager to entitle his 1940 article 'One phonemic entity becomes two: the case of "short *a*"'. Cohen found such extensive and unpredictable irregularity in the margins of the New York and New Jersey rules—before voiced fricatives, velar stops, in polysyllables—that he came to the conclusion that no rule could be written. Equally unpredictable areas of the lexicon are found in Philadelphia; e.g., short *a* followed by *-stV-* sequences (*master, plaster* etc.) shows extensive individual variation that cannot easily be reduced to rule.

7.2. THE DIFFICULTY OF ACQUIRING THE SHORT *a* PATTERN. An important component of LCV was Payne's 1980 investigation of the acquisition of the Philadelphia dialect by children of out-of-state parents in King of Prussia. This new suburban community was composed of 50% Philadelphians and 50% families from out of state—primarily high-status parents from Cleveland, Massachusetts, and New York, brought to the area by the electronic, chemical, and computational industries located nearby. Payne studied 24 families on six blocks, recording parents alone, children, and peers in extended interviews, with both spontaneous speech and formal experiments. The 108 children interviewed included 34 children of out-of-state parents who were brought to Philadelphia at various ages and were exposed for varying lengths of time to the Philadelphia dialect. For our present purpose, the crucial data consist of the comparative success of children in acquiring the two kinds of sound changes we are considering.

Table 4 shows the results for five phonetic variables involved in changes in progress in Philadelphia: (ay^0), the raising of /ay/ before voiceless consonants;

	Phonetic variables				
	(ay°)	(aw)	(oy)	(uw)	(ow)
Acquired	50%	40%	60%	52%	68%
Partially acquired	44%	40%	30%	48%	32%
Not acquired	6%	20%	10%	0%	0%
Number with parental pattern different from Philadelphia	34	20	20	25	25

TABLE 4 (from Payne 1976).

(aw), the fronting and raising of the nucleus of /aw/, with reversal of the glide target to a low back position; (oy), the raising of the nucleus of /oy/; and (uw) and (ow), the fronting of the nuclei of /uw/ and /ow/ except before /l/. Note that from 40% to 68% of the children have completely acquired these sound patterns: the variation depends on the relative age of the variable, since the newest sound changes, (ay^0) and (aw), show the smallest percentages.[21]

The critical variable which determined acquisition was the age when the child moved to King of Prussia. Table 5 shows that the Philadelphia pattern was acquired by 60–70% of those who came before age five, and by 40–67% of those who came from five to ten; a small number of cases suggest that those who came still later did not do as well.

VARIABLE	% COMPLETE ACQUISITION	NUMBER CONSIDERED
Age 0–4		
(aw)	60	10
(ay°)	65	17
(ow)	70	10
(uw)	70	10
(oy)	70	10
Age 5–9		
(aw)	40	8
(ay°)	50	14
(ow)	58	12
(uw)	67	12
(oy)	63	8
Age 10–14		
(aw)	00	2
(ay°)	00	3
(ow)	67	3
(uw)	00	3
(oy)	00	3

TABLE 5 (from Payne 1976).

[21] Data from both real time and apparent time enable us to stratify Philadelphia sound changes into five categories: completed, older and moving slowly, middle range, new and vigorous, and incipient classes. (ay^0) and (aw) represent two of the three new and vigorous changes (Labov 1980, Labov et al. 1980).

There is no need to show a comparable table for the acquisition of the Philadelphia short *a* distribution. Of the 34 children born of out-of-state parents, only one acquired this pattern. The contrast between the two kinds of variables is remarkable. All that is required to learn the Philadelphia vowel shifts is to be exposed to the dialect before the age of eight or nine. To learn the short *a* pattern, it is not enough to be born in the Philadelphia area; one must have parents born in Philadelphia! How can we explain this contrast? The only linguistic data which we must acquire from our parents, and cannot get elsewhere, are the first dictionary entries. It seems reasonable to conclude that the short *a* pattern is such a set of dictionary entries: that Philadelphia children acquire *mad* with an underlying tense vowel, *sad* with an underlying lax vowel.

This conclusion is reinforced by evidence from the comparison of children of New York City parents and children whose parents come from the Northern Cities area. The out-of-state systems have two different relations to the core pattern of the Philadelphia system. As Fig. 4 indicates, New Yorkers have two basic tasks: to become Philadelphians, (1) they must generalize the environmental feature [+front] so that only [+front] consonants permit tensing, and so lax their vowels in all words ending in /ʃ/; and (2) they must lax short *a* before all voiced stops except three common affective adjectives ending in /d/. If the problem were one of operating with rules, there would be no question as to which task would be easier: the first. But Payne shows conclusively that the New Yorkers did much better with the second task. However, the children of Northern Cities parents, whose original system simply has one tense /æh/, did comparatively better on the first, more general task. If we begin with the assumption that New York City children have two underlying phonemes, this result follows: they would naturally do better at a task that involves re-assigning words one at a time.

7.3. CATEGORICAL DISCRIMINATION OF SHORT *a* CLASSES. To test further the idea that children from the Middle Atlantic states learn two underlying forms, LCV carried out a series of experiments (Labov 1978) to see if any differences existed in the degree of categorical discrimination of the tense and lax forms for speakers from different dialect areas. The test series[22] was based on a sequence of ten forms of the word *mad*, resynthesized with formants systematically altered from an original form pronounced by a Philadelphian. The series followed the path of the sound change: from [mæ:d] to (mɛ:ˑᵊd] to [miˇᵊd], as shown in Figure 5, overleaf.

As in other categorical discrimination experiments (Liberman et al. 1957, Pisoni 1971), the subjects were asked to perform two distinct tasks. One was to categorize a randomized series of 100 items as 'Type 1' or 'Type 2', illustrated by the two extreme forms of the test series. The other was an 'ABX' task: to

[22] We are indebted to the research group headed by Max Matthews at Bell Telephone Laboratories, Murray Hill, NJ, for the use of facilities to prepare the stimuli tapes, and particularly to Osamu Fujimura for his guidance and assistance at many points along the way. We are also indebted to Laurel Dent for assistance at a crucial point in the effort to preserve the natural character of the original spoken version.

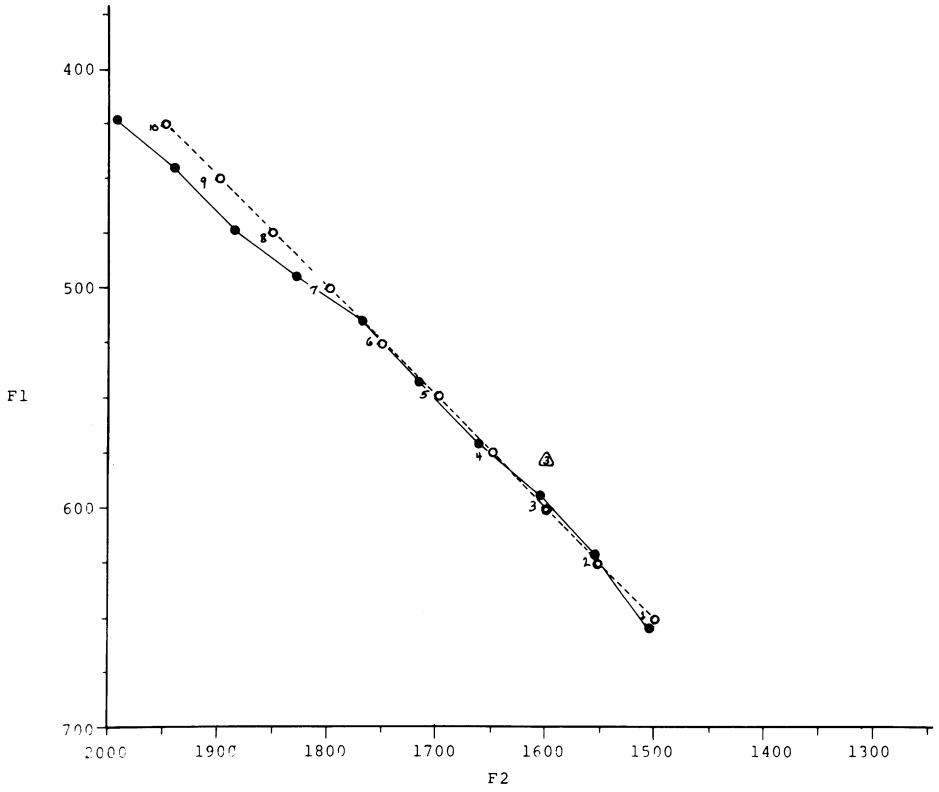

FIGURE 5. Stimuli for the *mad* categorization and discrimination tests.

△ Nucleus of original production of *mad*.
○ Target positions of F1 and F2 for altered stimuli.
● Formant positions of synthesized stimuli re-measured by LPC [linear predictive coding]

discriminate between Type 1 and Type 2 in listening to sets of three words where the first is Type 1, the second Type 2, and the third must be judged as one or the other. Figure 6 shows results for a New York City subject. On the horizontal axis are the ten forms of the word *mad*. The dotted line shows the percentage labeled Type 2. Categorization is sharp and precise: the only variation is for the form on the boundary, judged 'Type 2' 50% of the time. The solid line is the proportion correctly discriminated in a two-step test (where A and B of ABX are two units apart on the scale of 10). The discrimination peak on the categorization boundary indicates a high degree of categorical discrimination. Discrimination can be predicted from categorization on the assumption that correct ABX answers are entirely determined by the probability of differential labeling, corrected for chance guessing (Pisoni):

$$D = [1 - (P_{\text{Type 1}} - P_{\text{Type 2}})^2]^{1/2}$$

The dashed line on Fig. 6 indicates this prediction: for the New York City subject, discrimination is indeed sharper on the boundary and is predicted

within reasonable limits by the labeling function. This confirms other evidence for the existence of a phonemic boundary between tense and lax short *a*.

A series of such experiments was carried out with 19 subjects from Philadelphia and New York City: we had reason to believe that, for them, tense and lax /æ/ were represented as two distinct phonemes in the underlying dictionary entries. For a contrasting dialect with a single phoneme, Rochester and Syracuse were selected: here all short *a* words are tensed and are undergoing raising according to the consonantal environment. Eighteen upper New York

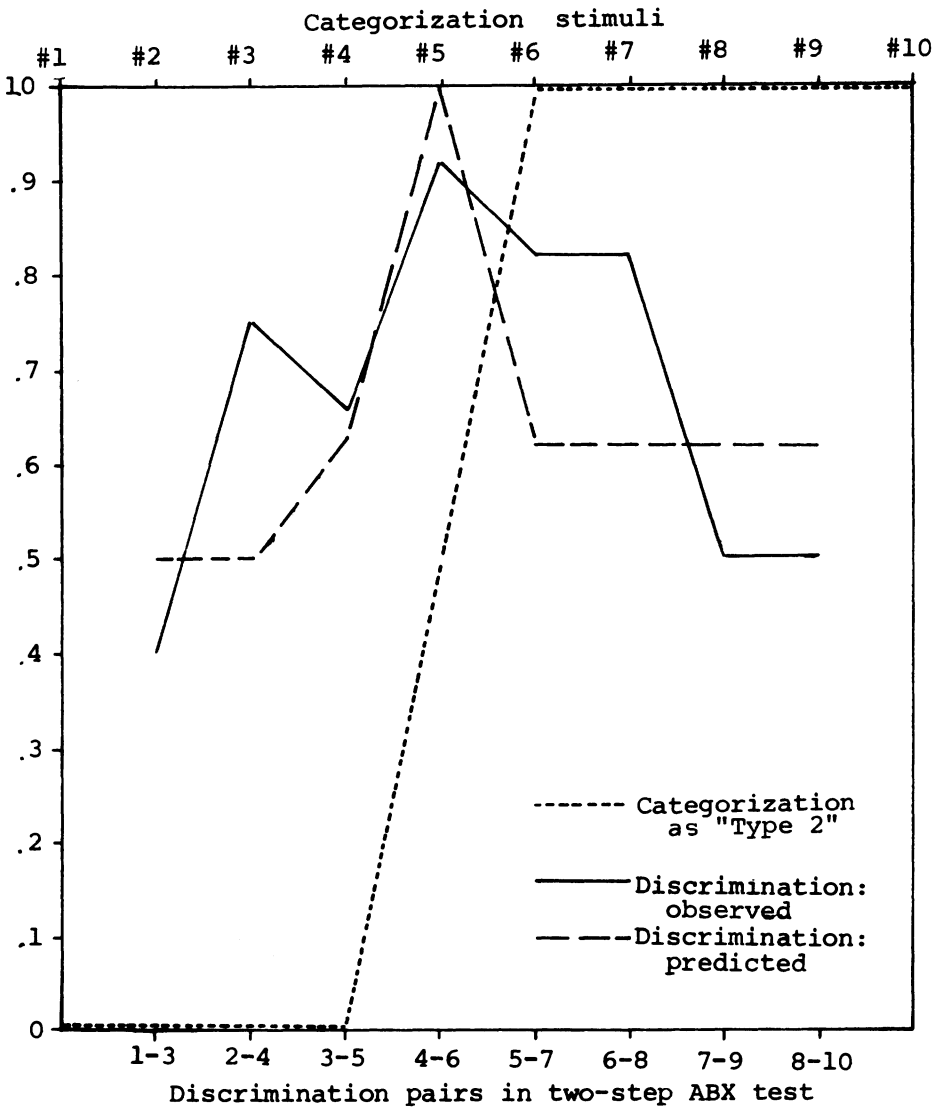

FIGURE 6.

State subjects were chosen who had minimal exposure to other dialects: their productions of short *a* in word lists were measured, and all showed a continuous range of uncorrected tense vowels.

The degree of categorization can be measured by fitting a regression line to the points that register the percentage of Type 1 judgments for each stimulus. The 'categorization range' is then the distance between the intersections of this line with the lines showing 0% of Type 2 and 100% of Type 2. This yields a figure even for those subjects who have only a vague tendency to categorize, variable for all stimuli: the range will be greater than ten, and this is the case with a number of New York State subjects. However, one Syracuse subject showed perfectly sharp categorization with only 100% Type 1 or 100% Type 2, and three showed variation for only one boundary stimulus.[23] The majority of subjects from both areas showed some variation for four to seven centrally-located productions of *mad*. Six showed no significant slope of the regression line, and so can be judged not to categorize Type 1 and Type 2 at all.

The extent to which these experiments yield evidence for a phonemic boundary is best shown by the degree of fit between prediction from the labeling function (D above) and the actual performance in the ABX test. A significant *r*-correlation between these two will yield such evidence, providing that there is a reasonably sharp categorization. If the subject shows an infinite or very high categorization range, then there is no interest in what is predicted from such random responses.

Table 6 contrasts the Mid-Atlantic (New York City and Philadelphia) subjects with the upper New York State subjects in this respect. Four levels of *r*-correlation are shown, and three degrees of categorization. For those subjects who show no categorization, no *r*-correlation is given. These are all from New York State: no Mid-Atlantic subject shows absent or weak categorization.

	Correlation of discrimination & prediction				
	None	Low	Medium	High	
Degree of categorization[a]	<.20	.20–.39	.40–.59	>.60	
Philadelphia, NYC					
Sharp (0–2)	1	0	0	3	
Moderate (4–7)	5	1	2	7	
Broad (9–12)	0	0	0	0	
None					0
Rochester, Syracuse					
Sharp (0–3)	1	0	3	0	
Moderate (4–7)	3	4	3	2	
Broad (8–12)	2	1	0	0	
None					6

Table 6.

[a] Degree of categorization is shown as number of stimuli with variable responses.

[23] Labov 1978 points out that these subjects showed the sharpest responses to the tensing and raising of short *a* on a semantic differential scale: they were the most sensitive to the social affect involved, which appears to be higher in Rochester and Syracuse than in other northern cities.

Ten of the 19 Mid-Atlantic subjects show a high correlation of categorization and discrimination, but only 2 of the 23 New York State subjects do so. Coupled with the qualitative difference in the presence of absence of categorization, this makes it clear that the two dialects are dramatically different in the treatment of short *a* words. At the same time, we must note the bi-modal distribution in the mid-Atlantic subjects: if we accept these experiments as evidence for a phonemic split, then we must also accept the fact that 7 of the 19 speakers do not show this split.

Bearing in mind that categorical discrimination of vowels is never as sharp as the corresponding phenomenon for consonants, these results indicate a distinct differentiation of the two dialect areas in the expected direction. To the extent that categorical discrimination does indeed indicate a linguistic boundary within a continuum, this result fits with the two previous ones, but reminds us that the differences in underlying forms may not be absolute. It is possible that some speakers in the Mid-Atlantic States derive all short *a*'s from a single source, and that some self-conscious Northern Cities speakers have categorized their local forms as different from the national network standard.

7.4. CONSEQUENCES. The convergence of these three kinds of data leads me to conclude that we have obtained a clear answer to the classic question, 'One phoneme or two?' Speakers of the Philadelphia speech community have two distinct sets of dictionary entries for lax /æ/ and tense /æ:/. The consequences for our examination of the Neogrammarian issue are evident. If the tensing of short *a* does involve a lexical split, then it would follow inevitably that further progress in that split must take place by a lexical redistribution. The sound cannot be the basic unit, because no sounds are involved. By the fundamental principle of the arbitrariness of the linguistic sign, membership in a new word-class is not assigned by any regular rule. Though a number of words may be affected at once, the fundamental unit of change must be the individual lexical item.

8. LEXICAL DIFFUSION IN PROGRESS. The present configuration of tense /æh/ and lax /æ/ in Philadelphia leads us to the strong inference that lexical diffusion operated at some earlier stage in the history of this redistribution. To this point, the view of lexical diffusion is no more direct than the view obtained from the DOC studies. However, in dealing with other aspects of linguistic change, LCV has found that the most profitable strategy is to track the mechanism involved by the observation of change in progress, rather than weighing the residues of processes no longer operating. It was then welcome news to find that, in a sub-area of the lexical distribution of short *a*, lexical diffusion is still at work.

The first subcondition for tensing given in §6, above, was that a consonantal segment or boundary must follow the first consonant after short *a*. Thus even with the most favored consonant, /n/, we would have lax *manner, camera, planet, damage, flannel*. I was more than a little surprised when we made an early exploration of another Philadelphia suburb, Radnor, and found that a group of twelve-year-olds read this list with *planet* tense, but the rest lax.

This was not an isolated event. In every area of the city, LCV found the same tendency for speakers to break the pattern of the earlier distribution, pronouncing *planet* as tense. Word-list data for 31 Philadelphians from King of Prussia are shown in Table 7. There is a trend to tense vowels throughout the _NV class, but *planet* leads the list with twice the frequency of any other word: two-thirds of the speakers read it with a tense vowel.

	N	% TENSE
—NV		
planet	62	68
damage	31	35
manage	31	32
flannel	31	23
camera	31	19
family	31	19
—LV		
personality	30	20
pal	31	6
algebra	30	0
California	31	0

TABLE 7.

Table 8 looks more closely at the situation by adding the data from spontaneous speech, and by separating children of age 9–15 from parents, 37–52 years old. It is clear that the pattern of lexical diffusion is not an artifact of word lists. For the more common words, there are enough occurrences in spontaneous speech to distinguish speakers who used only lax vowels in a given item from those who used both lax and tense, or those who used only tense vowels.[24] For both adults and children, in both styles, the leading position of *planet* is preserved. It also appears that Table 7 understates the extent of lexical diffusion: there is only a slight tendency for adults to use a tense vowel—perhaps more in *planet* and *flannel*, though the figures are too low for us to be sure. Children show an overwhelming tendency to tense *planet*; only two speakers use a lax vowel consistently. Other words are moving as well, but *planet* is obviously the leader.

	ADULTS 37–52 years		CHILDREN 9–15 years	
	SPEECH	WORD LIST	SPEECH	WORD LIST
planet	1/2/4	1/0/8	12/1/2	17/2/2
damage		0/0/9		10/0/11
manage		0/0/9		9/0/12
flannel	0/0/9	2/0/5		7/0/14

TABLE 8. A/B/C = no. all tense / no. tense and lax / no. all lax.

[24] In the King of Prussia interviews, Payne designed special techniques to concentrate occurrences of *planet* in spontaneous speech.

LCV also found lexical diffusion in the _LV subclass. In other cities, following /l/ is not a favoring environment for tensing. But Philadelphia postvocalic /l/ is most often realized as an unrounded mid back glide, and there is a general tendency for a nucleus followed by a lax glide to become tense. For adults as well as children, *pal* is homonymous with *Powell*: both words show a long tense nucleus followed by a back glide. Table 7 shows word-list data for the same 31 King of Prussia subjects, comparing *personality, pal, algebra*, and *California*. Here *personality* and perhaps *pal* are good candidates for lexical diffusion; the data from spontaneous speech point in the same direction.[25]

9. LEXICAL DIFFUSION IN THE WORKING-CLASS NEIGHBORHOODS. The vowel shifts that LCV has studied are initiated and led by speakers in particular locations within the social structure (Labov 1980, Labov et al. 1980). But the lexical diffusion documented above for the middle-class suburb is not limited to any one area. Data from the working-class neighborhoods in Kensington and South Philadelphia show the same pattern as the middle-class suburb King of Prussia.

Data from the working-class neighborhoods illustrate the extraordinary stability of the core pattern: i.e. words governed by subconditions 1 and 5 of §6. Anne Bower examined short *a* words in the spontaneous speech of 75 working-class speakers, and found no exceptions to the general pattern of tensing governed by the phonetic pattern of Fig. 4 and subcondition 1. It is even more remarkable to find such consistency in the highly specific lexical subcondition 5, as shown in the results of Table 9: in 185 spontaneous uses of *bad, mad, glad, sad, dad,* zero exceptions were found to the rule that the first three vowels are tense, the others are lax.

	TENSE	LAX
bad	97	
mad	62	
glad	10	
sad		6
dad		10

TABLE 9.

Table 10 shows much less stability in the marginal distributions that show lexical diffusion in King of Prussia. The _LV class provides the largest amount

[25] The selection of *planet, pal,* and *personality* in the process of lexical diffusion is of course far from arbitrary. The effect of frequency is present, as in the earlier stages of the short *a* rule. The role of affect in *mad, bad* and *glad* re-appears in *pal* and *personality*. Phonetic conditioning can also be detected, not in the precise form of output rules like the fronting of /ow/, but in a rougher approximation: e.g., *mad, bad,* and *glad* with grave initials, opposed to *sad* with a non-grave initial. The inclusion of the initial liquid cluster *gl* seems hard to explain, since such environments tend to lower F2 and F1 in the phonetic output (see *glass, trap* and *black* in Fig. 2). Yet it is repeated in the selection of *planet* as a leading element in the change. More inportantly, the following /it/ syllable of *planet* contributes to the tensing of the first vowel as opposed to the darker /əl/ of *flannel* (first pointed out to me by C.-J. N. Bailey, p. c.). This echoes the umlaut rule of the Atlanta dialect described by Sledd 1966, which opposes *picket* to *pickle* as relatively tense and lax. No similar phonetic conditioning appears for *pal* and *personality*.

		AGE			
		8–19	20–39	40–60	60–
All—LV words					
	Tense	7	6	(1)	0
	Lax	8	15(3)	7	10
Individual words					
alley	Tense	5	(1)	(1)	0
	Lax	6	3	3	4
personality	Tense	2	(1)	0	0
	Lax	2	3	(1)	0
Italian	Tense	0	1	0	0
	Lax	4	3	4	2
Allegheny	Tense	0	0	0	0
	Lax	0	4	0	1

TABLE 10. ( ) = unclear cases.

of data. One can observe a steady movement from 0% tense in the oldest speakers, a slight tendency toward tensing in speakers 40–60 years old, about 30% tensing among speakers in their twenties and thirties, and almost 50% among pre-adolescents and adolescents. This growth in the tensing pattern does not occur evenly across the gamut of lexical items. Table 10 also shows the record for the four most common words: *alley, personality, Italian,* and *Allegheny*. It is clear that the tensing is concentrated in the first two. The results show, as in King of Prussia, that *personality* is rapidly becoming a tense word. The same pattern can be observed in the _NV class, though the individual words are not as frequent.

10. WHERE IS LEXICAL DIFFUSION TO BE FOUND? The splitting of short *a* is clearly a classical case of lexical diffusion. The vowel shifts of Philadelphia are, by contrast, classic cases of Neogrammarian sound change. In the course of the discussion, we have isolated a number of features that characterize these two polar types of change in progress, as shown in Table 11.

	(ohr), (oy), (ay°), (uw), (ow), (æh)	/æ/ ~ /æh/
discrete	no	yes
phonetic conditioning	fine	rough
lexical exceptions	no	yes
grammatical conditioning	no	yes
social affect[a]	yes	no
predictable	yes	no
learnable	yes	no
categorized	no	yes
dictionary entries	1	2
lexical diffusion: past	no	yes
present	no	yes

TABLE 11.

[a] The Philadelphia study includes subjective reaction tests that show a consistent reaction to sound changes in progress, strongest for the raising of (æh), but also for the new and vigorous changes. No social consciousness of mergers or lexical distributions of short *a* has been detected (Labov et al. 1980).

It remains to be seen whether the same array appears in other cases of lexical diffusion or regular sound change, and if the two sets of properties turn out to show a regular association. We will then be able to approach the larger question: where in general can we expect to find lexical diffusion, and where can we expect Neogrammarian regularity? Since the cases examined so far involve vowel shifts in progress in English dialects, the logical first step is to examine the array of completed vowel changes in the history of English, at least insofar as the nature of residues or lexical irregularities is concerned. A scholarly review of the subject is beyond the scope of this paper, but some initial observations are so striking that they are difficult to avoid.

The differentiation of English vowel changes seems at first glance to be surprisingly clean-cut. We find regular sound change in a wide range of vowel shifts that represent movements within the subsystem of short vowels, or the subsystem of up-gliding diphthongs, or in-gliding diphthongs: raising, lowering, fronting, backing, rounding, unrounding, nasalization. The Great Vowel Shift consisted primarily of sound changes of this type, similar in their regular character to the vowel shifts studied by LYS in a range of English dialects, and by LCV in Philadelphia.

At the other extreme are shortenings and lengthenings, which would correspond to the tensing (or lengthening) of short *a*.[26] The history of English gives us a wide variety of lexically irregular cases, Middle English long *ē* was shortened in *head, dead, breath, sweat* etc.,[27] while the great majority of words in this class stayed long and rose to [i]: *bead, read, mead* etc. Then a second irregular shortening gave us *sick, silly, britches*, and other short [ɪ] forms. In the back, we have the result of two irregular shortenings (with rough phonetic conditioning) in *flood, blood, glove* etc., and later *good, stood* as opposed to *food, mood, fool* and the rest.[28] Wyld 1936 begins his discussion of shortenings and lengthenings in Early Modern English with the following troubled passage:

> 'The whole question is beset by various difficulties. Lengthening and shortening of vowels has occurred at various periods during the history of English, sometimes under conditions which are clear and can be formulated without hesitation, since the results are found with regularity, and the apparent exceptions can be explained by a specific analogy, sometimes under conditions which are more or less obscure, since the lengthening or shortening is apparently intermittent, being present in some words, but absent in others in which the phonetic conditions seem to be identical.' (253)

[26] The available orthographic conventions usually lead to a differentiation of 'long' and 'short' vowels in the historical record. Though there are some cases (i.e. Hungarian high vowels) where the difference is purely one of length, such oppositions are unstable (Chen & Wang); it is more common to find short and long subsets differentiated by a complex of many phonetic features which are best described with more abstract terms like 'tense' and 'lax'. The use of the /h/ symbol in /æ/ is appropriate for the tense long and in-gliding vowels, but for a general discussion it is probably better replaced by /:/—so long as the length sign is taken as an abstract representation of the class of long vowels, rather than phonetic length.

[27] See Jespersen (1949:242). This shortening shows great dialectal diversity; e.g., *head* remained tense in Scots, and is [hi:d] in Glasgow.

[28] This is one of the common examples explained by Neogrammarians as dialect borrowing (Bloomfield), or used to illustrate lexical diffusion (Fónagy 1956, Wang 1979). But many oscillations have occurred since the process was new and vigorous, and there is little that can be detected now of the original mechanism.

In addition to the lengthening and shortening of mid and high vowels, an even more striking set of parallels to the lengthening of short *a* can be found in the recent lengthenings of English low vowels. The broad *a* class in *after, glass, bath, aunt* etc. is now realized as a long low central vowel, but it is generally considered that the lengthening took place in front position at 'the time immediately following the change from /a:/ to a front vowel, while short *a* was still a back vowel' (Jespersen:307). This means that the tensing of what is now 'broad *a*' was originally accompanied by fronting and raising, almost identical to the modern [æ] → [æh]. The phonetic conditioning is almost identical to the Philadelphia pattern: voiceless fricatives and front nasals (cf. Ferguson). The amount of lexical irregularity in the broad /a/ class was even greater in early developments than we observe today. Jespersen notes that Holyband 1609 has broad *a* in *aunt, command, demand* etc., but has short *a* in *answer, branch, advance*, and wavers in *change*. Similar alternations are found in Hart's 1569 transcription of such words as *master* with both forms, echoing the Philadelphia situation today. Thus it is clear that the mechanism of change involved in this lengthening was lexical diffusion. Furthermore, the well-known irregularity of the broad *a* class has consequences for its learnability: it serves as an ideal marker of a class dialect, since it can apparently be learned correctly only by children who have attended British public schools from an early age (Wyld, 3).

The same rough phonetic conditioning may be seen operating in the irregular lengthening of short open *o*. Every dialect and subdialect of American English shows a different distribution. Thus my own speech shows tense *moth, wroth, cloth,* but lax *Goth*; tense *strong, long, song,* but lax *ping-pong, gong, thong*; tense *moral* and *coral*, but lax *sorrel* and *tomorrow*.[29]

It was noted above that the current lengthening of checked short *a* follows a much earlier lengthening in open syllables. This process was far from regular, and the lengthening of short *a* in open syllables in early Middle English showed many of the traits we have been examining. Many of the irregularities are explained as the result of irregular choice of the inflected form (open syllable) or the uninflected form (closed syllable), giving rise to doublets like *black* and *Blake, pass* and *pace, stave* and *staff*. Jespersen believed that much of the irregularity of the broad *a* development is a continuation of this pattern (1949:308). But there is a much stronger kind of grammatical conditioning here, quite parallel to the grammatical conditioning of short *a* tensing. The Middle English lengthening in open syllables did not affect the past participles like *written* or *bitten*, even though they had only a single consonant.[30]

11. CHANGES ACROSS AND WITHIN SUBSYSTEMS. Why should these shortenings and lengthenings be irregular? The explanation, I suggest, is that they are

[29] The role of frequency is obvious in these cases. Note that it is the back nasals that favor raising for the low back vowel, not the front nasals.

[30] I am grateful to Robert Stockwell for pointing out this grammatical conditioning of the original tensing process, which shows a much closer parallel to the modern complications than the variation explained by the choice of inflected or uninflected bases.

not sound changes in a literal sense. They are changes of membership in abstract classes of long or short (tense or lax) vowels. Long/short, like tense/lax, does not refer to any physical dimension—certainly not to duration alone—but to a set of features that may include length, height, fronting, the directions and contours of glides, and the temporal distribution of the over-all energy. In these shortenings and lengthenings, the whole set of phonetic features changes at once, at least in the cases that we have been able to examine in progress. We must recognize a hierarchy of features depending on the number of phonetic features involved in their realization—necessarily, a hierarchy of abstractness. In modern English, [±tense] will be at the top of this hierarchy; fronting and raising will be near the bottom.

This is equivalent to saying that there are subsets of vowels in English, and that the contrast BETWEEN subsets is greater than contrast WITHIN them. Within subsystems, changes are governed by three general principles of chain-shifting (Labov & Wald 1969; LYS, Chap. 4):

(a) Tense or long nuclei rise.
(b) Lax nuclei normally fall.
(c) Back nuclei move to the front.

LYS also discusses principles that govern movements between the subsystems of short vowels, up-gliding diphthongs, and in-gliding diphthongs; but here far less clarity emerges. Figure 7 (overleaf) shows the principal routes followed across subsystems for the front vowels, based on changes found in a wide variety of languages and language families. Path 2 at the bottom of the figure is the route followed by the tensing of short *a*. It is on this path that we have observed lexical diffusion—the earlier *mad, bad, glad*, and current *planet, personality*. The brief observations made of other changes in the history of English suggest that, in general, we can look for lexical diffusion in these shifts across subsystems, i.e. changes of abstract features, and Neogrammarian change within the subsystems.

Fig. 7 illustrates why the tensing of short *a* and short *ŏ* are favored sites for such shifts of subsystem. Principle (b) above indicates that short or lax nuclei fall, as indicated by the arrow in the vowel triangle at lower right. As in the Northern Cities, there is structural pressure on lax /æ/ to exit the system. The only available route is to the long and in-gliding vowels, i.e. tensing; from there come the inevitable fronting and raising of (æh) that we have witnessed.

Within a subsystem, vowels can be opposed on a relatively abstract level, through combinations of the three lower-level dimensions. The Milroy 1978 study of lexical diffusion in Belfast shows such a result for a late stage of the sound changes involved, perhaps better characterized as the social alternation of a stable opposition of standard and local forms. One such set involves the splitting of words in the short *u* class: *butcher, hull* etc. The phonetic alternants are opposed by height, fronting, and rounding: [ü] vs. [ʌ]. A second case concerns short *a* words, as in *carrot*, which show irregular alternation of extreme front and back forms, [æ] vs. [ɔ], differentiated by both fronting and rounding.

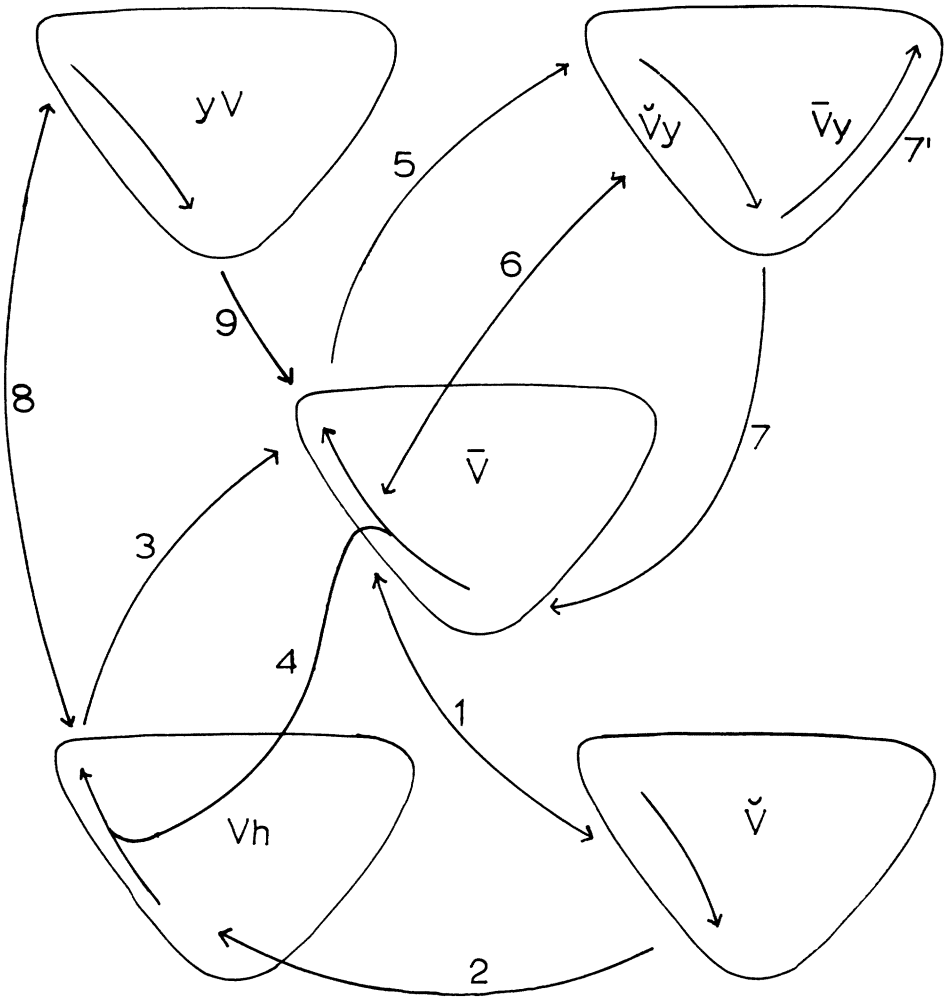

FIGURE 7.

What about diphthongization and monophthongization? These processes seem to hold an intermediate position. The first case of irregularity discussed by Wang 1969 dealt with the monopthongization of Chinese /ay/. Yet the diphthongization of high vowels that apparently initiated the Great Vowel Shift was quite regular, like parallel movements in other Germanic and in Balto-Slavic languages (LYS, Chap. 4). A detailed study of a case of monophthongization is to be found in Malkiel's 1976 paper on 'Multi-conditioned sound change'. This involves the sporadic reversal of the general diphthongization of Gallo-Romance short mid vowels. Thus typically Latin *frons, fronte* 'forehead' became Old Spanish *fruente*, La. *pressa* 'compressed' led to OSp. *priessa* 'hurry'. Malkiel's task is to explain the irregular process that led to Modern Sp. *frente*, *prisa*, and the contrast of French *siècle*, Sp. *siglo* 'century'. In

Malkiel's exhaustive discussion can be found all the features presented for short *a*: rough phonetic conditioning, not complete; morphological influence, intersecting from various directions; and finally, the ineradicable residue of lexical irregularity, 'a residue of cases unsolved if not insoluble' (768). At the same time, the parallel case of the perfectly regular monophthonigzation (*ie* → *e*) makes it clear that the addition or loss of glides seems to follow the Neogrammarian pattern as often as not, or more so.

A good source for further data on vowel change is the extensive study of sound change in progress in Copenhagen by Brink & Lund 1975. Their real-time data have greater depth and reliability than any other study of which I know: they include recordings of speakers born as early as 1810, and the sound changes are reported by means of an impressionistic transcription of considerable depth and precision. In a discussion of 'sound laws', Brink 1977 reports a wide variety of exceptions to sound changes. Some of the irregularities which he discusses are accounted for by the principles outlined above, e.g. orthographic influence on the late stages of change, and several shortenings which affect only a few words and are conditioned by frequency. But exceptions are also reported for simple lowerings and raisings. In the lowering of the upper mid nucleus of a diphthong with a low back glide, analogy has operated to prevent the lowering of the common word *læger* 'doctors'. A raising of long [a:] does not affect Swedish and German loan words. A lowering of [ø] before [n m f] (an environment strikingly similar to the tensing condition for Philadelpha short *a*) is characterized as 'very slow and affecting the vocabulary less systematically' than many other sound changes.

On the whole, the findings of Brink and Lund indicate that analogy, frequency, and foreign status can influence the course of low-level output rules. At the same time, their over-all findings support the principles presented above. Their most general observation on the consistency of sound change does not indicate a word-by-word mechanism, but rather a Neogrammarian regularity:

> 'Once these exceptions have been established, we must then admit that the more recent sound laws observed in the oral data generally do show an extremely high degree of consistency; they actually operate rather mechanically.' (Brink 1977:10)

12. CONSONANTAL CHANGES. I would like to consider for a moment some of the more obvious issues in consonant changes, though less work has been done in this area. In general, the importance of the feature of graduality, emphasized by Wang, is put into high relief. The most striking contradictions to the Neogrammarian notion of graduality appear in metathesis, haplology, and other discontinuous consonant shifts. The history of Dravidian initials traced by Krishnamurti involves such re-arrangement of consonant groups, and it is only natural for us to expect lexical diffusion here. The same can be said for changes in place of articulation.

A detailed view of such diffusion can be obtained in Kinkade's 1972 study of the shift of velars to alveo-palatals in the Salishan languages of the Pacific Northwest. At both geographic ends of this language family, we find alveo-palatals among the plain and glottalized voiceless stops and the voiceless fricatives; but in a central region, a large group of languages show the conservative

velars. Lower Cowlitz is exceptional in showing a split, 'a case of sound-shift caught in transit' (p. 2): roughly one-third of all the morphemes are in the alveo-palatal series. Kinkade explains 15 of the 124 cases by borrowing from Chinook Jargon, and accounts for several dozen by phonetic processes. Like Malkiel 1976, he exhausts all possible sources of explanation, including dissimilation, before he concludes that this sound change is phonetically discrete but lexically gradual.

Bonebrake's 1979 monograph on the labial-velar changes in English and Dutch considers one of the most striking examples of discontinuous changes of place of articulation: the shifts of [x] to [f] and [f] to [x]. She necessarily begins with a well-known list of lexical irregularities such as regular *cough* vs. irregular *dough* or *slough*, regular *daughter* vs. irregular *laughter*, etc. The overall model that she presents for multiple conditioning of sound change is as complex as those of Malkiel or Kinkade—involving the possibility of perceptual continuity, morphological and semantic influence, and social differentiation.

By contrast, it seems that changes in manner of articulation are most often phonetically gradual and lexically regular. This regularity appears in current studies of change in progress: the vocalization of liquids /r/ and /l/ that have been studied in New York and Philadelphia, but also in the lenition of obstruents. The various phonetic shifts of palatals in South America show every sign of Neogrammarian regularity: the lenition of /č/ to an affricate in Panama City (Cedergren 1973) and the devoicing of /ž/ in Buenos Aires (Wolf & Jiménez 1977). The sizeable literature on the aspiration and deletion of /s/ shows no evidence of lexical conditioning in the many detailed quantitative investigations of Spanish (Ma & Herasimchuk 1968, Cedergren 1973) and of Portuguese (Naro & Lemle 1976, Guy 1980).

In English, the progressive devoicing of final consonants seems to be a regular, Neogrammarian shift. A more dramatic lenition of postvocalic stops is to be found in Liverpool, where voiceless stops become affricates and fricatives—beginning with /k/, and now proceeding to /t/ and perhaps /p/ among younger children. My own explorations of this process show that it is both gradual and regular. Such a replay of Grimm's Law reminds us that it is no accident that the Neogrammarians' most brilliant successes were scored on the original leniting shifts of Grimm's Law. There is every reason to think that this was a gradual process, a phonetic output rule: as paradigmatic an example of Neogrammarian change as we might look for.

One way to approach this brief survey in a more controlled way is to examine as a whole a group of sound changes that were assembled in a different perspective. Fónagy 1956, discussed above, provides such a base: the studies he discusses are organized according to whether they show evidence of variation in individuals, in the lexicon, prosodic or positional influence, etc. Fónagy's conclusions bear on the variety and complexity of factors that influence sound change, rather than the types of change that might favor one mode or the other. If we consider all the studies of modern dialects presented by Fónagy that

include some evidence of change in progress,[31] the classification shown in Table 12 gives some support to the principles outlined in this section.

	NO LEXICAL CONDITIONING REPORTED	LEXICAL CONDITIONING REPORTED
Vowel shifts		
within subsystems	4	1
diphthongization and monophthongization	3	1
lengthening and shortening	0	7
Consonant shifts		
change of manner	4	0
change of place	5	2

TABLE 12.

The one case of lexical diffusion reported for vowel changes within a subsystem is in Sommerfelt's 1930 account of the Welsh high unrounded vowel: the older variant is preserved in three archaic words: 'werewolf', 'warrior', and 'lamentation'. Otherwise, these distributions support the observations that regular sound changes are in the majority. Where lexical diffusion does occur, it is to be found most often in changes across subsystems—particularly lengthenings and shortenings in vowels,[32] and changes of place of articulation in consonants. Diphthongization and monophthongization appear to be intermediate cases: in some of the studies of diphthongization cited by Fónagy, lexical conditioning was observed to be present; in others, its absence was noted.

13. THE RESOLUTION OF THE PARADOX. What progress has been made, then, in resolving the Neogrammarian controversy?—or, to the extent that we have come to recognize a substantive issue, in resolving the Neogrammarian paradox? Restated most simply, the paradox amounts to this: if Wang and his associates are right about lexical diffusion, and the Neogrammarians were righter than they knew about sound change, how can both be right?

One group has asserted that 'Phonemes change', the other that 'Words change.' Neither formulation makes much sense: they are abstract slogans that have lost their connection with what is actually happening. A close examination

[31] I have not included, for example, Hermann's 1929 discussion of the alternation of /θ/ and /h/ in Charmey, since comparison with Gauchat's data led Hermann to the conclusion that no change was involved. A number of observations of English data by Otto Jespersen and Daniel Jones seem to have no obvious connection with change in progress, such as the centralization of /i/ in *pretty* and *children*. However, there are a number of citations of English dialects with the same complex conditions of lengthening of short *a* that we find in the Mid-Atlantic states.

[32] Fónagy documents lexical differences in his own meticulous studies of vowel length in French and Hungarian. But these may not bear directly on the issue, since the French data deal with a low-level phonetic continuum, and the Hungarian high vowels present the less common case of a phonemic difference based on phonetic length alone.

of change in progress has been more fruitful. We have located Neogrammarian regularity in low-level output rules, and lexical diffusion in the redistribution of an abstract word class into other abstract classes. I do not propose to resolve the original confrontation into a simple dichotomy—that here words change, there sounds change. I have exhibited two polar types, and have analysed the clusters of properties that created these types. The whole array of sound changes will undoubtedly show many intermediate combinations of these properties of discreteness, abstractness, grammatical conditioning, and social conditioning.

Other dimensions should certainly be taken into account. On the basis of studying lexical diffusion in Breton, Dressler 1979 has suggested that we are more apt to find diffusion when fortition is involved than lenition.[33] Henry Hoenigswald has brought to my attention a dimension that must be relevant to several of the cases we have considered here: the scope or length of the conditioning environment. Hoenigswald points out that, when phonetic conditioning extends over two, three, or four segments, the probability that a grammatical boundary will be crossed increases dramatically—and with it, the probability of grammatical conditioning. Open-syllable lengthening is just such a case, since syllables are necessarily defined by segment-sequences of some length. The modern tensing of short *a* considers a minimum of two, and up to four, following segments. By contrast, the fronting of /ow/ in Philadelphia has one simple exception—when a liquid is the next segment; and this of course is never a grammatical formative in English. I am sure that other important properties must be considered. We will find some discontinuous shifts that are regular, like the shift of apical to uvular (r) throughout Western Europe. We will also no doubt find some lexical irregularity within subsystems, beyond those caused by correction at late stages of the change, as the materials of Brink & Lund indicate.

My resolution of the Neogrammarian controversy is a proposal for a shift of research strategies. I would strike from our agenda the questions, 'Does every word have its own history?', 'Is it phonemes that change?', 'Are the Neogrammarians right or wrong?' and start a research program of a different sort. We begin with respect for the achievements of our predecessors; but that does not mean that we rest content with the data they have gathered. An appreciation of their work is shown, not by the remanipulation of the original observations, but by adding a wider and deeper set of inquiries that will display the value and the limitations of these initial results. We can then ask, what is the full range of properties that will allow us to solve the transition problem—i.e., to predict with a high degree of certainty the way the phonological system will behave as the system changes?

In the course of this program, we can expect to illuminate the synchronic nature of the phonological system. We have a distance to go in restoring the balance between universalist and historical explanation, and in giving full

[33] It might be noted that the example analysed in detail by Malkiel 1976, to which Dressler also refers, is a case of lenition rather than fortition.

weight to the principle that, to understand a linguistic system, we must know how it came to be (Jespersen 1924, Ch. 2). Many of our synchronic explanations have in fact been simple readings of the historical record under another name, without considering whether new historical events may have wiped out those older processes. Let us accept for a moment the finding that the tensing of short *a* in Philadelphia is a lexical split. If so, the only reasonable representation for tense *mad* vs. lax *sad* at the most abstract level[34] is /mæ̆d/ vs. /sæ̆d/. But if *mad* is /mæ̆d/, there is no possibility of using the same representation for *made*, which is an integral part of the general proposal to use the Middle English vowels for an underlying representation of Modern English (Chomsky & Halle 1968). Such a representation would predict an abstract merger of *made* and *mad* that we know did not take place.

In this presentation, I have brought together the results of research in the speech community, acoustic analysis, and historical linguistics. With this convergence of data and methods, I have also tried to illustrate a particular conception of how linguistic theories may be constructed. This is not a new undertaking; we have seen convergent data and multivariate explanations in the work of Wang, Fónagy, Malkiel, and Kinkade. Insights accumulate gradually, and only for those who can read the work of their predecessors with understanding. These kinds of theories grow slowly; they emerge from the dirt and debris of the everyday world, never entirely free of errors of measurement and other vulgar irregularities. They take shape, they grow strong and dependable to the extent that they keep their connection with that everyday world, and as long as they are cultivated by those who have the feel of it. Their beauty lies not in their simplicity or symmetry, but in their firm connection with reality.

REFERENCES

BABBITT, E. H. 1896. The English of the lower classes in New York City and vicinity. Dialect Notes 1.457–64.

BICKERTON, DEREK. 1974. Creolization, linguistic universals, natural semantax and the brain. Working Papers in Linguistics, University of Hawaii, 6.1124–41.

BLOOMFIELD, LEONARD. 1933. Language. New York: Holt.

BONEBRAKE, VERONICA. 1979. Historical labial-velar changes in Germanic. (Umeå studies in the humanities, 29.) Umeå: Acta Universitatis Umensis.

BRINK, LARS. 1977. On sound laws. Paper given at the Society for Nordic Philology, Copenhagen.

——, and JOERN LUND. 1975. Dansk rigmal. 2 vols. Copenhagen: Gyldendal.

BRUNEAU, CHARLES. 1937. La diphtongaison des voyelles françaises. Zeitschrift für Romanische Philologie 57.170–92.

BURROW, THOMAS, and MURRAY B. EMENEAU. 1961. Dravidian etymological dictionary. Oxford: Clarendon.

CEDERGREN, HENRIETTA. 1973. The interplay of social and linguistic factors in Panama. Cornell dissertation.

CHEN, MATTHEW, and WILLIAM S-Y. WANG. 1975. Sound change: Actuation and implementation. Lg. 51.255–81.

CHENG, CHIN-CHUAN, and WILLIAM S-Y. WANG. 1977. Tone change in Chao-Zhou Chinese: A study in lexical diffusion. In Wang (ed.) 1977:86–100.

[34] As originally pointed out by Arvilla Payne.

CHOMSKY, NOAM, and MORRIS HALLE. 1968. The sound pattern of English. New York: Harper & Row.
COHEN, PAUL. 1970. The tensing and raising of short *a* in the metropolitan area of New York City. Columbia University master's essay.
DELBRÜCK, BERTHOLD. 1885. Die neueste Sprachforschung: Betrachtungen ueber George Curtius' Schrift 'Zur Kritik der neuesten Sprachforschung'. Leipzig: Breitkopf & Härtel.
DRESSLER, WOLFGANG. 1979. Diachronic phonology. Paper given at the MLA Symposium on Directions in Historical Linguistics, San Francisco.
FERGUSON, CHARLES. 1975. 'Short *a*' in Philadelphia English. Studies in linguistics in honor of George L. Trager, ed. by M. Estellie Smith, 259–74. The Hague: Mouton.
FÓNAGY, IVÁN. 1956. Ueber den Verlauf des Lautwandels. Acta Linguistica (Budapest) 6.173–278.
——. 1967. Variation und Lautwandel. Monologentagung. Wien.
GAUCHAT, LOUIS. 1905. L'unité phonétique dans le patois d'une commune. Festschrift Heinreich Morf, 175–232. Halle: Niemeyer.
GOIDANICH, P. 1926. Saggio critico sullo studio de L. Gauchat. Archivio Glottologico Italiano 20.60–71.
GUY, GREGORY. 1980. Variation in the group and the individual: The case of final stop deletion. In Labov 1980:1–36.
HERMANN, EDOUARD. 1929. Lautveraenderungen in der Individualsprache einer Mundart. Nachrichten der Gesellschaft der Wissenschaften zu Goettingen, Phil.-hist. Kl., 11.195–214.
HINDLE, DONALD. 1980. The social and situational conditioning of phonetic variation. University of Pennsylvania dissertation.
HOCKETT, CHARLES F. 1958. A course in modern linguistics. New York: Macmillan.
——. 1965. Sound change. Lg. 41.185–204.
HOENIGSWALD, HENRY M. 1978. The *Annus Mirabilis* 1876 and posterity. Transactions of the Philological Society, 17–35.
JANKOWSKY, KURT. 1972. The neogrammarians. The Hague: Mouton.
JANSON, TORE. 1977. Reversed lexical diffusion and lexical split: Loss of *-d* in Stockholm. In Wang (ed.) 1977, 252–65.
JESPERSEN, OTTO. 1924. The philosophy of grammar. London: Allen & Unwin.
——. 1949. A modern English grammar on historical principles, vol. 1. London: George Allen.
KING, ROBERT. 1969. Historical linguistics and generative grammar. New York: Holt, Rinehart & Winston.
KINKADE, DALE. 1972. The alveopalatal shift in Cowlitz Salitz. IJAL 39.224–31.
KIPARSKY, PAUL. 1971. Historical linguistics. A survey of linguistic science, ed. by W. Dingwall, 577–649. College Park: University of Maryland.
KRISHNAMURTI, BH. 1978. Areal and lexical diffusion of sound change. Lg. 54.1–20.
KUHN, SHERMAN, and RANDOLPH QUIRK. 1953. Some recent interpretations of Old English digraph spellings. Lg. 29.143–53.
LABOV, WILLIAM. 1966. The social stratification of English in New York City. Washington, DC: Center for Applied Linguistics.
——. 1978. The measurement of vowel shifts. Paper given at the American Association of Phonetic Sciences, Winter meeting, San Francisco.
—— 1980. (ed.) Locating language in time and space. (Quantitative analyses of linguistic structures, 1.) New York: Academic Press.
——, and BENJI WALD. 1969. General principles of vowel shifting. Paper given at the Annual Meeting of the LSA, San Francisco.
——; MALCAH YAEGER; and RICHARD STEINER. 1972. A quantitative study of sound change in progress. Philadelphia: US Regional Survey.
——, et al. 1980. Social determinants of sound change. (Final report on NSF SOC-75-00245.) Philadelphia: US Regional Survey.

LEHMANN, WINFRED P. 1967. (ed.) A reader in nineteenth-century historical Indo-European linguistics. Bloomington: Indiana University Press.
NARO, ANTHONY, and MIRIAM LEMLE. 1976. Syntactic diffusion. Papers from the Parasession on Diachronic Syntax, 221–39. Chicago: CLS.
LIBERMAN, ALVIN, et al. 1957. The discrimination of speech sounds within and across phonemic boundaries. J. of Exp. Psych. 54.358–68.
MA, ROXANA, and ELEANOR HERASIMCHUK. 1968. The linguistic dimensions of a bilingual neighborhood. Bilingualism in the barrio, ed. by Joshua Fishman et al., 636–835. Washington, DC: Office of Education.
MALKIEL, YAKOV. 1967. Every word has its own history. Glossa 1.137–49.
——. 1976. Multi-conditioned sound change in the impact of morphology on phonology. Lg. 52.757–79.
MARTINET, ANDRÉ. 1955. Économie des changements phonétiques. Berne: Francke.
MILROY, JAMES, and LESLIE MILROY. 1978. Belfast: Change and variation in an urban vernacular. Sociolinguistic patterns in British English, ed. by Peter Trudgill, 37–51. Baltimore: University Park Press.
NARO, ANTHONY, and M. LEMLE. 1976. Syntactic diffusion. Papers from the Session on Diachronic Syntax, 221–39. Chicago: CLS.
OSTHOFF, HERMANN, and KARL BRUGMANN. 1878. Morphologische Untersuchungen auf den Gebiete der indogermanischen Sprachen, I. Leipzig. [Translated in Lehmann 1967.]
PAYNE, ARVILLA. 1976. The acquisition of the phonological system of a second dialect. University of Pennsylvania dissertation.
——. 1980. Factors controlling the acquisition of the Philadelphia dialect by out-of-state children. To appear in Labov (ed.) 1980.
PEDERSEN, HOLGER. 1962. The discovery of language. Bloomington: Indiana University Press.
PISONI, DAVID. 1971. On the nature of categorical perception of speech sounds. (Status report on speech research, SR-27.) New Haven: Haskins Laboratories.
POSTAL, PAUL. 1968. Aspects of phonological theory. New York: Harper & Row.
REMACLE, LOUIS. 1944. Les variations de l' *h* secondaire en Ardenne liègois: Le problème de l' *h* en liègeois. Paris: Éditions Dion.
SCHUCHARDT, HUGO. 1980. The ethnography of variation. Selected writings on pidgins and creoles. Translated by Thomas Markey. Ann Arbor: Karoma.
SHERMAN, DONALD. 1973. Noun–verb stress alternation: An example of the lexical diffusion of sound change in English. POLA Reports, 2nd series, 17:46–82. Berkeley.
SLEDD, JAMES H. 1966. Breaking, umlaut, and the Southern drawl. Lg. 42.18–41.
SOMMERFELT, ALF. 1930. Sur la propagation de changements phonétiques. Norsk Tidsskrift for Sprogvidenskap 4.76–128.
STOCKWELL, ROBERT, and C. WESTBROOK BARRITT. 1951. Some Old English graphemic–phonemic correspondences—*æ*, *ea*, and *a*. (Studies in Linguistics, Occasional papers, 4.) Washington, DC.
STREETER, MARY. 1977. DOC, 1971: A Chinese dialect dictionary on computer. In Wang (ed.) 1977:101–19.
TOON, THOMAS. 1976. Lexical diffusion in Old English. Papers from the Parasession on the Lexicon, 357–64. Chicago: CLS.
TRAGER, GEORGE L. 1930. The pronunciation of 'short a' in American Standard English. American Speech 5.396–400.
——. 1934. What conditions limit variants of a phoneme? American Speech 9.313–15.
——. 1940. One phonemic entity becomes two: The case of 'short a'. American Speech 15.255–8.
WANG, WILLIAM S-Y. 1969. Competing sound changes as a cause of residue. Lg. 45.9–25.
——. 1977. (ed.) The lexicon in phonological change. The Hague: Mouton.

——. 1979. Language change—a lexical perspective. Ann. Rev. Anthropol. 8:353–71.
——, and CHIN-CHUANG CHENG. 1977. Implementation of phonological change: The Shuang-Feng Chinese case. In Wang (ed.) 1977:148–58.
WEINREICH, URIEL; WILLIAM LABOV; and MARVIN HERZOG. 1968. Empirical foundations for a theory of language change. Directions for historical linguistics, ed. by Winfred P. Lehmann & Yakov Malkiel, 97–195. Austin: University of Texas Press.
WHINNOM, KEITH. 1965. The origin of the European-based creoles and pidgins. Orbis 14:509–27.
WILBUR, TERENCE (ed.) 1977. The Lautgesetz controversy: A documentation (1885–1886). Amsterdam: Benjamins.
WINTELER, J. 1876. Die Kerenzer Mundart. Leipzig.
WOLF, CLARA, and E. JIMÉNEZ. 1977. A sound change in progress in Buenos Aires. Unpublished paper.
WYLD, HENRY CECIL. 1936. A history of modern colloquial English. 3rd ed. Oxford: Blackwell.
YAEGER, MALCAH. 1975. Speaking style: Some phonetic realizations and their significance. (Pennsylvania working papers on linguistic change and variation, 1:1.) Philadelphia: U.S. Regional Survey.

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