WHAT'S WRONG WITH *EI* ? OR, IF IT AIN'T BROKE – WHY FIX IT?

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The theme of this paper is the nature of explanation in diachronic linguistics in general, and for sound change in particular. Explaining sound change by appeal to rules raises questions about the source of the change, the mechanism of the change itself, and the motivation for its spread. To answer these fundamental questions, I turn to evidence from studies of child language acquisition, experimental phonology, and sociolinguistics. In the paper, I review various approaches to explaining sound change; I argue that the source and the mechanism of sound change is variability in perception and production particularly during language acquisition, and I adopt the view that the spread of a change can be explained by sociolinguistic factors.

In 1786, Sir William Jones claimed that Sanskrit, Greek, and Latin 'have sprung from some common source, which, perhaps, no longer exists' (quoted in Crystal, 296). Sir Jones noticed several words that had the same sound and meaning in these three languages, and by doing so, established not only the Proto-Indo-European hypothesis, but the basics of the comparative method as well. Ever since, sound similarity and various claims about processes of sound change have been at the center of diachronic linguistics. However, these claims have little explanatory power; they are often more of a post-hoc, and indeed at times ad-hoc, analysis. The case of the Proto-Indo-European diphthong [ei] provides us with a good example (hence, the title of this work).

Scholars have postulated a reconstructed diphthong [ei] for Proto-Indo-European based on monophthongs in the vast majority of the proposed daughter languages. The usual discussion, though, takes the reversed direction. Namely, the various monophthongs are considered to be the reflexes of the PIE diphthong. Comparing a number of correspondence sets reasonably allows us to hypothesize that the monophthongs share a common ancestral vowel. The particulars of those monophthongs and their behavior in different environments might even point at the possibility of a diphthong as the shared ancestor. Because diphthongs are readily available in the synchronic sound systems of IE languages, we might wonder why did the PIE diphthong change in the first place.

As simple, almost child-like, as this question appears to be, I consider it the most fundamental question we should raise in the study of diachronic linguistics. Describing the evolution of a given sound, a structure, and even a whole language family allows us just to recognize some of the processes at work. But it is only the answer to this basic WHY question that allows us to understand such proposed processes and evaluate their validity. Obviously, I am not the first to raise this question, nor am I the only one to consider it crucial (see e.g., Hockett 1965, Jeffers 1974). However, few have taken this challenge beyond the traditional labels. For instance, in the standard textbooks available for Historical Linguistics, we find very little if any discussion about the forces underlying diachronic changes: In Lehmann 1962, we find no discussion at all; in Arlotto 1972, we find a short discussion focusing on factors which 'seem to interfere with our ideas of regular language change' (200); and in Bynon 1977 we find a three page subsection titled 'The mechanism of language change' (213) in which most of the discussion centers around social motivation for the SPREAD of a change. Hockett 1965 raises questions similar to the above, and says: 'These questions go beyond a mere classificatory or "taxonomic" handling of the how of linguistic change, and begin to ask about the why' (191). Although Hockett provides an excellent and intriguing review of the field, there are gaps in his discussion, as we shall see below. Focusing my discussion on sound change ('which is clearly the most widely studied and perhaps best understood of all types of language change', Jeffers 1974:231), I open with a review of the labeling tradition. Bringing converging evidence form experimental phonology and studies of child language acquisition, I outline one of the promising sources of explanation, namely, that children initiate changes into the language as they acquire it. I conclude by trying to reconcile some of the seemingly opposing views and by suggesting an eclectic approach.

By 'labeling', I mean the tradition of naming a change and considering that label as having some explanatory powers. Such is the case when we say that the reason for *know* to be pronounced as /no/ is 'cluster simplification'. I find two difficulties with this approach. One is the assumption that by labeling we have explained the process, and the other lies in the over-reliance on the spelling system as representing the sound system. Because the latter is a simple matter, I shall treat it first.

In a number of currently spoken languages which have a writing system, we find that the orthographic conventions do not necessarily represent the pronunciation in a systematic manner. In the case of Chinese and other ideographic systems the issue is beyond argument. But even with alphabetical systems we find difficulties such as with consonantal systems in Semitic languages, or with the supposedly full orthographic systems of English and French. Because the only evidence we have for languages which are no longer spoken is in written form, we have no other choice but to assume that at one point in the history of the language for which we only have a written record, the orthography attempted to capture the spoken form. Unfortunately, we have no way of testing this assumption, and the only thing we can do about it is to add a disclaimer as a preamble to every study in diachronics. This

suggestion is not trying to be humorous. It is extremely important for all scholars and students in the field to recognize the extent of the assumptions on which the field is based.

Although the problem with the correspondence between the orthographic and the sound systems is unavoidable, the problem with appealing to labels for explanation is. Early attempts to go beyond labels usually resulted in providing new labels to replace the old ones. Such has been the case with attributing sound changes to 'doing what comes naturally' or 'ease of articulation' (see further discussion below). One process that seems to fit this description is lenition. But if lenition was the overwhelming power in sound change, we would expect all language ultimately to reduce their vowels to schwas and their consonants to /h/. We know that this is not the case, and we know that there are several competing forces at work. We shall return to these competing forces shortly, but before we do that, let us explore this notion of doing things 'naturally' a bit further.

Assuming that the physical structure and limitations of the articulatory organs are practically identical in all people (in terms of the range of possible articulations, not necessarily acoustic features), we should expect all languages to move towards the same set of sounds. However, this does not happen. Furthermore, if by 'doing what comes naturally' we mean to say that languages move towards a basic 'natural' set of sounds, we may wonder what such a set may be like. Moreover, we can expect that 'natural' basis to have been the starting point of all languages which would bring us back to our original question -- if that was natural and if what is natural has not changed, why would the sound change? Recent theories about the innate (natural) basis of language lead in the same direction. Chomsky's (1981) proposal of 'hardwired' principles and a limited set of parameters that need to be set during acquisition, does not leave much room for changes. Likewise, Bickerton's (1981) hypothesis of 'bio-programming' raises the basic question of why are languages different if we all have the same 'default grammar'? Some change processes are similar across languages, particularly in the areas of grammaticization and metaphorical extension leading to semantic changes. However, the results of sound change processes, even though we might use the same labels, seem arbitrary as in the case of the proposed PIE [ei] developing into different vowels in different languages.

As for the competing forces, we must remember that the purpose of the sound system is to allow us to make meaning distinctions. In other words, we must maintain phonemic differences. Again, the inevitable question comes up: if at one time the language maintained all the necessary meaning distinctions using the available (and of course, articulateable) sounds of that language, why would there be any motivation for that sound system to change? And so to explore different attempts at answering this question, let us now turn to a brief historical review. (For a detailed critical analysis of the various positions reviewed below, see Baron 1977, Jeffers 1974, and Jeffers & Lehiste 1979, on which the following discussion is based.)

During the second half of the 19th century, the neogrammarians of Leipzig formulated the 'regularity hypothesis.' Led by Karl Brugmann and Hermann Osthoff, and later by Hermann Paul, the neogrammarians looked for explanations in phrasing 'rules' to which there were no exceptions. Any seeming exceptions were only the product of other 'rules'. '"There must," as Karl Verner put it in 1877, "be a rule for the exceptions to the rule"' (Jeffers & Lehiste, 90). These rules were descriptive in nature, the sort of 'A changes to B in environment C' that we are all too familiar with by now, and had no explanatory power. These are the rules that resulted in the common labels we still use today.

In his 1886 second edition of *Prinzipien der Sprachgeschichte*, Hermann Paul provided a comprehensive review of the neogrammarian approach and explicitly accepted 'ease of articulation' as a motivation for change. Nevertheless, Paul also acknowledged the psychological factors involved. He viewed the conditioning environment as physiological, but the process as psychological because it is the 'idea' of the sound not yet produced that affects the production of the preceding sound. Unfortunately, Paul's insight into possible psychological factors was largely ignored with the rise of the American Structuralism. (For a detailed discussion of the neogrammarians' positions and in particular the influence of Whitney and Bréal, see Nerlich 1990.)

For Leonard Bloomfield, the most notable figure of the structuralist school, Linguistics had to be a science; and for it to be a science, linguistics had to be formal and mechanical. Any appeal to 'mentalism' was a pure heresy, or at least beyond the scope to which linguistics should limit itself. For Bloomfield, sound change was simply the result of an alteration in speakers' habits of articulatory movement; nonphonetic factors, in his opinion, could never prove relevant to sound change (Bloomfield 1933). A later structuralist, Charles Hockett, did appeal to some nonphonetic factors, but still within a very mechanical framework. For Hockett, extralinguistic reasons for missing the proper articulation included moisture in the vocal tract, wax or dirt in the ears, and overall sloppiness (Hockett 1965). Hockett proposed that each speaker is trying to hit an articulatory target, but fails to do so, and over time the target drifts to become a new target. He says: 'This drifting of allophones, and hence of distinctive features, is SOUND CHANGE' (202). As an antimentalist, Hockett neglected to tell us where the original target comes from and how it is transferred among speakers. Overall though, the structuralists were interested in sound changes only in the cases that they effect a structural change, i.e. a change in the structural system of the language.

Around the same time period, a slightly different structuralist school was taking hold in Prague. Like their American counterparts, the Prague structuralists were interested in the language as a system and viewed changes as

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systematically affecting the whole. However, unlike the American structuralists, the Prague school introduced functionalism as an overriding factor. For linguists like Martinet and Jakobson, Hermann Paul's 'ease of articulation' became the widely accepted explanation for conditioned sound changes. Unconditioned sound changes were explained in terms of 'functional yield'. A phonemic distinction which carries a 'low functional yield' is likely to disappear over time, whereas a distinction which carries a 'high functional yield' is not a likely candidate for a change (Martinet 1953). Of a particular interest is Martinet's notion of 'push chain' and 'drag chain' which seem to account for a domino like effect in sound changes (such as the 'great vowel shift'). The chain notion is based, however, on an appeal to symmetry in a sound system. This appeal must be questioned, because there is very little reason to expect a sound system to be symmetric (Note that very little, if anything, in nature is symmetric.) Moreover, even if the chain notion could possibly account for subsequent changes, the trigger for the change is not explained. Nevertheless, the influence of the Prague school can still be easily seen in recent works such as when Ronneberger-Sibold 1987 says:

> 'The basic idea of any natural theory of linguistic change is that languages do not change randomly or due to certain entirely system-inherent evaluation criteria, but that languages are changed by their users in order to facilitate communication. This means that the language users are constantly adapting their "communicative instrument," i.e. their language, to their needs of production, e.g. the need for ease of pronunciation, and perception, e.g. the need for unambiguity.' (517)

A similar approach whereby a change is only of interest if it affects the whole language system was taken by the generativists of the late sixties. For Postal 1968, a sound change is a surface phenomenon which is of no consequences unless it indicates a change in the deep structure, in the underlying internalized abstract system. The neogrammarians' regularity of the sound change is the result of the underlying rule change which affects the whole system, not because of the conditioning environment as was originally proposed by the neogrammarians. Again, as before, labeling the change as one in the deep structure rather than the surface structure does not explain why or even how the change occurs. As an 'explanation', Postal offered the notion of 'stylistic variation'. Nevertheless, other generativists, in particular Kiparsky 1968 and King 1969 suggested to look at language acquisition as a source for explanations, and proposed 'imperfect learning' to be the motivating force behind diachronic changes. A similar notion to this 'imperfect learning' is Arlotto's 'discontinuous transmission' explained as follows by Arlotto (1972:197): 'Most of the words of a language then are transmitted over generations, with each generation perhaps adding variation which may modify the pronunciation of the word to a greater or lesser degree. The next generation then begins with these modifications and may extend them further.' Note that Arlotto uses four hedges within these two sentences.

At about the same time, David Stampe was developing the theory of 'natural phonology.' Like Kiparsky and King, Stampe 1969 turned to language acquisition as the source for sound changes. Stampe phrased the notion of imperfect learning as the failure to suppress or to properly order some natural innate phonological processes. This appeal to innate naturalness brings up the same questions we raised earlier concerning Chomsky's and Bickerton's approaches. (If it is innate, why isn't it the same in all languages?)

Still during that same time period of the late sixties and early seventies, a new direction has been pointed at by sociolinguists. With Labov 1972 at the lead, these scholars turned to social conditions as motivating change. Specifically, notions such as prestige dialect and other social variables were targeted as the force behind the spread of any change. Bynon 1977 adds: 'All this suggests that linguistic heterogeneity is in itself a constant source of change and that, at the phonological level at least, a great deal of language change is likely to have a social motivation' (215). This notion of 'heterogeneity' is implicit in many of the works which cite language contact and extensive borrowings as the main sources of change as well as in the work of dialectologists such as Jules Gilliéron and Hugo Schuchardt.

This same approach guides Laferriere 1980 when she says: 'The ontogeny of linguistic change lies in this very synchronic variation. Changes observed across several generations of speakers, as the work of Labov and others has shown, indicate that the same factors which condition synchronic variation also determine which variants will be carried through diachronically, and which will die out' (363). Again we are faced with a plausible account of the spread, but lacking an account of the change starting point, as Bynon tells us, 'All this unfortunately still tells us very little about how a change originates, apart from the fact that an innovation would appear to be associated initially with the speech of a particular subgroup of the community and to spread to adjacent groups given favourable socially determined conditions' (214).

Throughout all these years, language acquisition has been on scholars' minds as somehow relevant to language change. As early as 1886, Hermann Paul wrote: 'It is quite clear that the processes in the course of learning language are of the highest importance for the explanation of the variations in the usage of language – that they afford the weightiest reason for these variations' (quoted in Vihman 1980:303). This attitude, though, has taken various switches over time ranging from Bloomfield, who argued (1933:386) 'against the theory that sound change arises

from imperfections in children's learning of language'; to King (1969:65), who said that 'We are led to conclude that the ultimate source of dialect divergence – and of linguistic change in general – is the process of language acquisition' (65); and to Baron who in 1977 wrote a whole book with the explicit goal 'to make sense out of the LINGUISTIC TRUISM that normal first language acquisition is the source of much linguistic change' (v, emphasis added). And so it is time for us to explore language acquisition with the goal to make sense out of some of the available research literature.

The main reason for language acquisition to become such a tantalizing domain for the explanation of diachronic change is that children seem to produce 'errors' which correspond to established diachronic processes (at least to the commonly used labels). Bybee & Slobin (1982:29) draw our attention to the fact that 'The striking parallel between children's morpho-phonemic or analogical innovations and the morpho-phonemic changes that can be observed in the history of languages has long been noted.' Shepherd (1982:316) adds another dimension: 'In exploring the relationships between historical change, creolization, and child language acquisition we often come across what seem to be general tendencies in language and language change.' Ronneberger-Sibold (1987:517) notes: 'The needs of language acquisition, often mentioned in this [language change] context, coincide with some of these [communicative needs].' Demuth et al. (1986:453) go further in stating: 'It is found that historical changes which have resulted in the leveling of gender and agreement distinctions in Niger-Congo in many ways parallel the various stages of acquisition of the system in languages which still maintain these distinctions.' A stronger statement yet is made by Erbaugh (1986:399), who ascertains that 'Chinese children's acquisition of the Mandarin noun classifier system strikingly parallels the historical development of the Chinese classifier system from 1,400 B.C. to the present.' As we know however, correlation does not necessarily indicate causation.

Aitchison (1981:173) leaves no doubts about her approach: 'Let us begin by dismissing one popular but unfounded notion, that "ontogeny recapitulates phylogeny." This is the view that change in the language of an individual as she acquires language recreate the stages through which the language of the human species as a whole is presumed to have passed as it developed to its full mature stage.' Arguing against what Arlotto calls 'discontinuous transmission' and what Kiparsky calls 'imperfect learning', Aitchison continues:

> 'A second unfounded notion is more modern. Some linguists argue that, even if a child has the same speech output as his parents, his mental internalized grammar will be different. For example, earlier in this century the words *what, when, where* were regularly pronounced with an initial *wh* [hw]. Then, in several areas, [h] began to be dropped in casual speech. It is sometimes suggested that the older generation learnt the words with an initial *wh* but possessed a subsidiary rule which said, in effect, "Delete *h* in casual speech." The younger generation, however, grew up hearing the simple *w* more often, and so assumed that this was basic. They therefore had a subsidiary rule which was opposite to that of their parents: "Add *h* if talking slowly and carefully." Unfortunately, we have no evidence whatsoever that RULE INVERSION (as this phenomenon is called) actually occurs between generations in this way, since we have no means of looking into a child's mind and comparing it directly with that of his parents.' (174, emphasis original)

And Aitchison concludes with the following strong statement: 'In view of our lack of knowledge on these questions, it seems best to ignore topics about which we can do no more than weave fanciful theories, and concentrate on issues for which we have actual evidence' (ibid.) So we will, indeed, turn to examine empirical studies which focus on 'actual evidence.'

Vihman 1980 reviews some of the controversy surrounding the issue of sound change and child language, and then turns to analyze in details some of the perceived parallels between language acquisition and language change. She assumes that it is 'the burden of [her] paper to suggest that though many disparate parallels may be found, some of the most common or typical child language processes are either virtually non-existent or totally different in detail in adult synchronic processes and in sound change' (305). Vihman compares language acquisition data with sound change data focusing on the shared processes of consonant harmony, long word reduction, and consonant cluster reduction. She concludes her analysis by saying that 'as each of these [sound change] tendencies contradicts the corresponding child language data, there is little motive here to look to child language for the source of phonological change. In fact,' Vihman continues, 'in view of the creation of new consonant clusters in the course of syncope, we might add that the phonotactic preferences of children seem to have little effect even in preventing the completion of sound changes that will cause them trouble' (314). Vihman adds that 'Though it is of course true that each child must 're-create' his language from the start, the evidence seems strongly to show that the results of this re-creation are highly conservative' (315–16).

Like Vihman, Aitchison 1981 examines in details three aspects of child language that appear to correspond to diachronic change. Aitchison focuses though on somewhat broader aspects including simplification, parallel processes such as double negatives, and the issue of a critical period. As we can expect from our previous encounter with Aitchison, her conclusion is unambiguous:

> 'Overall, then, the arguments in favour of children initiating change turn out to be mirages. Simplification in child language is unlike that in language change, since the former affects common, rather than uncommon words. Apparent parallel processes turn out to have quite different causes. Furthermore, the critical period argument only applies to the fresh learning of a whole language, and not to the partial alteration of an already acquired one.' (179-80)

This notion of parallel processes having different causes is also claimed by Erbaugh 1986, who says about the 'striking parallels' between the acquisition and diachrony of Chinese noun classifiers that 'while the child's classifier use reflects his increasingly complex conceptual distinctions, the historical development of classifiers was triggered by the need for explicit surface distinctions among nouns as the language became increasingly homophonous because of massive phonemic mergers' (399).

At the other extreme end of the controversy we find Baron 1977, a book reviewed as 'the most comprehensive contribution to the topic' (Stein 1981:179). Baron's work concentrates on providing an empirical framework for the examination of ontogeny and diachrony from a single point of view while addressing specifically the issues of the ontogeny-phylogeny recapitulation and the extent to which child language data could be used to evaluate historical hypotheses. She concludes her book proposing 'Directions for a general theory of linguistic variation and change' and says:

> 'One of the world's keenest observers of biological structure, Aristotle, maintained that women have fewer teeth than men. The philosopher also hypothesized that in females, the jejunum may occupy any part whatsoever of the upper intestine, but in males it comes just before the caecum and the lower stomach. Medically, there is no basis for either of these claims. Most modern linguistic diachronists have tended to behave like Aristotle in his weakest moments ... Many diachronists have implied that it is not possible to observe the process by which language change occurs, but that it is only possible to observe its results. In the present work I have attempted to show that we may, in fact, be able to study the origins of some linguistic change *in vivo* by analyzing how children learn to speak, or, more generally, how children or adults acquire native or non-native languages.' (288-89)

In his review of her work, Stein 1981 raises several questions about Baron's approach and conclusions. He points out that, in places, Baron's 'hypothesis is irreconcilable with the facts of promulgation of linguistic change through SOCIAL space as demonstrated by Labov' (184). Stein criticizes Baron further arguing:

> 'What B [Baron] does, as far as her empirical investigations are concerned, is to construct a historical hypothesis on the basis of a synchronic analysis in terms of lexical decomposition, and then to put questions to child language acquisition in terms of this hypothesis: i.e., absolute logical precedence is given to the input historical hypothesis. Obviously in such a situation the filter-effect of the input hypothesis is such that B can only get out at the acquisition end what she puts in at the historical end, and at times the impression is that – even within her approach – B tries too hard to make awkward data fit her stance.' (185)

This critique not withstanding, Stein acknowledges the importance of Baron's work, particularly in establishing the empirical methodology necessary for studies of this kind.

Having looked now at two extreme views on the role of language acquisition in language change, we may want to search for some middle ground – and, what is more, for a clear direction in which to continue. One such a direction has been hinted at above and will be explored in the remainder of this paper.

Aitchison 1981 ascertains that 'Changes begin within social groups, when group members unconsciously imitate those around them' (180). Although she does not explicitly say so, it seems that Aitchison assumes that the actual starting point is idiosyncratic. This idiosyncrasy could be the result of Hockett's 'missing the articulatory target', of Arlotto's 'discontinuous transmission', or of Postal's 'stylistic variation'. Nevertheless, Aitchison's emphasis on the adults seems to be shared by Vihman (1980:316) who says that 'Innovations must be not only initiated but spread by adult speakers, whose offspring then duly learn to match what is to them the norm.' Bybee & Slobin 1982 make a stronger argument yet when they say that in 'cases where adult innovations differ from early child innovations, ... the adults and older children, who are in better command of the entire system, innovate in ways that manifest more precisely the on-going changes in the system. Thus it appears that both socially and linguistically the older children and adults are in control of morpho-phonemic change' (37). Nevertheless, however strong these data-based conclusions may appear to be, other studies point in a slightly different direction.

Boelens 1987 suggests that children may be reliable indicators of language change even if they are not completely responsible for it. In concluding a study of breaking in Frisian ('"breaking" involves the alternation of rising and falling diphthongs,' and 'Frisian-speaking children fail to apply breaking in forms where adults do use it'; 95), Boelens brings together the children, the adults, and the social factors:

> 'In our title, language acquisition is called an indicator of language change. For Labov (1972:319) an indicator is a new but not conscious variant. Here neither children nor adults perceive that breaking is omitted, let alone that they attach a social value to it. For Chen (1972:494) language change is "a dynamic process having a chronological profile of its own" or "a gradual extension of a linguistic innovation over the lexical domain as a function of time." So our inquiry could expose an incipient language change. Our four- or five-year-old kindergarten children do not have a sociocultural trigger to cause them to leave out breaking in the plural and diminutive of beam. They can say [bɪəmən] or [bɪəmkə] unnoticed, or at least unpunished. If in years to come breaking becomes a sign or symbol of "pure Frisian" some might avoid it, considering it overdone. Then a prestige factor will be introduced with some speakers in favor of breaking and others against.' (105)

This incipient imperceptible change is also reported in Janson's (1986) study of Swedish vowels. In a carefully constructed phonological experiment, Janson compared the perception of two vowels by young and old speakers of the same Swedish dialects. He found a statistically reliable difference between the two groups. In concluding his research, Janson says:

> 'We have, then, a systematic difference in perception of the phoneme boundary between the two groups, but it is so small that, in practice, it will never show up as systematic misidentifications of phonemes between members of the groups, since differences in perception among individuals within the group is a more important factor, and the sounds which might be interpreted in different ways by the two groups will at any rate belong to the region of uncertainty for individual listeners ... Thus, by studying perception and not production, we have found a way to discover even very slow and gradual sound change ... it is still quite possible, and indeed even probable, that a large part of all sound change is in fact slow and gradual (Labov 1981). And for this type of change, linguists have for a long time accepted the view of earlier generations that direct observation is not possible. But it seems that Hockett (1965), Jespersen (1922:167), and the others were wrong. If changes are going on, they can be observed if the right technique is applied.' (259)

And so it seems that children do initiate some changes but that these changes might be too slight to be noticed by the adults around them. It also seems that only after the children have become adults and probably after several generations the change is transferred sufficiently by social factors to become established in the language.

Bybee & Slobin 1982 find a different distribution of responsibility between the children and the adults. 'Our data suggest, then,' they conclude, 'that current changes in a language will be better reflected in adult innovations, and that adults are actually responsible for carrying out morpho-phonemic change. Young children, on the other hand, give a better indication of the full range of possible changes and are an important source of information for a theory of morpho-phonemic change' (37). However, with all the evidence presented above, it would seem that young children do more than just that.

Labov's work (1972, 1980, 1981) demonstrates that changes are likely to be promulgated through the social space and because of primarily social factors. Because children are perceived to eventually acquire the adult language, whatever stages they go through are argued to be irrelevant to language change. However, the research literature we have been examining in this paper would suggest that slow-to-develop imperceptible-at-the-time changes are, in fact, initiated by children. It seems likely, nevertheless, that changes which are more sudden are indeed the result of adult innovations. Thus, adult individuals and adult social groups play a major role in language change, but are not the only players. We therefore conclude that child language indicates the range of possible changes, indicates specific directions of change, and initiates changes which are likely to be slow and gradual. And so to answer the questions raised in the title of this paper, we may just say that there was nothing wrong with [ei], and that it was not broke. It is just the case that children may not have perceived it as the same [ei] as their parents were producing, and they did not think in terms of fixing it. It is also the case that the parents of these children did not perceive their children to be producing a different vowel than their own [ei].

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