On the unpredictability of contact effects

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Historical linguists know that any search for deterministic predictions of language change is bound to fail, whether the focus is on internally-motivated change or on contact-induced change. But the urge to explain linguistic change is strong, and many linguists have proposed generalizations that make limited predictions about what can and can't happen in language history. Some of these generalizations, of course, are more successful than others; in particular, the ones that focus on tendencies rather than absolute constraints are more likely to be useful.

The goal of this paper is to show how and why language change is unpredictable, with particular attention to externally-motivated change. Section 1 is a very brief glance at the state of prediction in internally-motivated change: after reviewing three basic assumptions that underlie all of historical linguistics, I will give a few examples of sound changes to show why prediction is impossible; I will then argue that predicting change in other grammatical subsystems is even more difficult than in the phonology. Sections 2 and 3, focusing on contact-induced change, examine the most commonly claimed linguistic and social predictors of externally-motivated changes and show how and why they fail. Three very different linguistic routes to language death will serve to illustrate the problems with deterministic predictions (section 4). Finally, section 5 explores the role of speaker creativity in linguistic innovations.

I will argue that all these examples and discussions point to two major conclusions (section 6). First, language change is unpredictable because even the most natural changes often fail to occur; therefore, although it is often possible to state NECESSARY conditions for change, it is never possible to state SUFFICIENT conditions for change. And second, since speakers can and do change their language deliberately, and since counterexamples have been provided to all the linguistic constraints that have been proposed on contact-induced change, it is extremely unlikely that there are nontrivial linguistic constraints on contact effects.

1. THE STANDARD VIEW OF INTERNALLY-MOTIVATED CHANGE.

Three fundamental assumptions of historical linguistics are (1) that all living languages are constantly changing; (2) that language change is unpredictable; and (3) that, as a result of (1) and (2), the speech of two separated subgroups of a language community will, given enough time, diverge into dialects and then separate languages. Unpredictability, in this context, means that the separated subgroups will undergo some different changes (although of course they will also undergo some similar and identical changes, since they inherited the same pattern pressures from their common parent language). In addition, and equally of course, a structure that undergoes change in one subgroup might well remain unchanged in the other. 'Separated', in assumption (3), means no further contact of any kind between the two subgroups, so that no changes can spread directly from one to the other. 'Enough time' is an elastic notion: 500-1000 years might be a reasonable estimate, depending on such factors as whether one of the groups comes into intimate contact with another language. (Apparent counterexamples to the 500-1000-year estimate generally evaporate when social information is taken into account. For instance, Modern Arabic dialects have arguably not diverged into separate languages after ca. 1500 years of separation; but in this case, as in many others, the separation is by no means complete, and Arabic speakers all share a common religious language which helps to promote overall linguistic unity.)

Historical linguists have a great deal of evidence to support their belief that these three assumptions are valid. To be sure, in some instances a linguist can look at a particular bit of grammar and predict with great confidence that it will change in the near future; but in all such cases the change is already under way, so this is not the same as predicting the beginning of change. An example is the loss of sentence-initial *whom* in Modern English, a change which has been under way for many decades and which can be predicted to continue until there are no more speakers or contexts in which sentence-initial *whom* occurs. Comparable examples can be found in cases of chain shifts, as for instance when SVO word order is replaced by SOV word order and then, later, other changes connected with that change occur, such as the replacement of prepositions by postpositions.

Historical linguists know much more about sound change than about change in any other grammatical subsystem, in part because we have many more examples of sound changes than

of other types of change and in part because phonetic considerations offer the possibility of explaining some, many, or possibly even most sound changes—explaining, that is, why x is more likely to change to y than vice versa. But even here the most natural sound changes (the most common changes, the most expectable changes from a phonetic viewpoint) often fail to occur.

Here are two examples to illustrate this point. The devoicing of obstruents in word-final position is a common change. But, though it occurred in the history of German and Russian, it didn't occur in some of their closest relatives, e.g. English and Serbo-Croatian, respectively.

Or consider the palatalization of velars, one of the most common of all sound changes. The strongest environment for a change from (for instance) *k to $\check{c}$ is before a high front vocoid, i.e. i or y; this happened in early Romance > Dalmatian, in Proto-Wakashan > Nootka, in Pre-Cree > Eastern Cree, and in many other languages as well. The next most likely environment for palatalization is any following front vowel, as in Pre-Sanskrit > Sanskrit, Pre-Slavic > Old Church Slavic, and early Romance > most Romance languages (other than Dalmatian). Variations on the same theme are found in Pre-English > Old English, where the change occurred before any unrounded front vowel, and in Pre-Dakota > Teton Dakota, where it happened after a front vowel. Finally, and with little direct phonetic motivation in its most general form, this change occurred in all environments in two sets of Salishan languages independently—fourteen Coast Salishan languages and two Southern Interior Salishan languages.

But it certainly isn't true that *$k > \check{c}$ in ALL languages, because many languages simply keep their k. Modern English has k before front vowels, for instance (though it is pronounced farther forward and affricated before i), and five Interior Salishan languages preserved Proto-Salishan k unchanged.

This highlights one of the main points of this paper, as true for contact-induced change as it is for internally-motivated change: one can't predict when change will occur; one can only, with luck, predict (or, more often, retrodict) the direction of a change once one knows that it is occurring. We don't know why the *$k > \check{c}$ change happens in one language and not in another, but we do know that it is much, much more likely than a change *$\check{c} > k$,

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for phonetic reasons. Similarly, we know that *$k > \check{c}$ is more likely before i than before (say) u, and that obstruents are more likely to devoice word-finally than to voice, again for phonetic reasons. In other words, implicational and probabilistic predictions about changes are possible, but deterministic and absolute predictions are not.

2. Contact-induced change: Linguistic factors as predictors.

Various linguistic factors have been claimed as predictors of contact effects. I will not attempt to survey all of them here; Thomason & Kaufman (1988) (henceforth T&K) give numerous counterexamples to all the linguistic constraints that were proposed in the earlier literature—including, most prominently, predictions based on markedness, predictions based on typological distance, and the prediction that the only linguistic features that can be borrowed are those that are compatible with the structure of the borrowing language. Here I will give just two further examples to show that the unpredictability of contact effects is pervasive.

One proposal in T&K is that phonological and syntactic interference can be predicted to go together: that is, if one type of contact effect is found, the other type ought also to be found, in roughly the same amount. This prediction has a great deal of solid support in a wide variety of contact situations. But there are at least partial exceptions. One is Indian English, as spoken in India by educated Indians. Phonological interference from various Indian languages—notably retroflex consonants—is strong in Indian English, much stronger than syntactic interference. The discrepancy can be explained as the result of the differential availability of native-speaker models for learning the two subsystems: the syntax of English is readily available through the written language, while the phonology is not.

A social circumstance, though admittedly a much vaguer one, can also be adduced to explain the partial failure of another prediction made by T&K (p. 74): 'We know of no exceptions—and would be astonished to find any—to the rule that nonbasic vocabulary is always borrowed first'. The claim in T&K is that nonbasic vocabulary is borrowed not only before basic vocabulary, but also before any structural (nonlexical) features are borrowed. But I am less easily astonished now than I was in 1988, and although I know of no actual, definite exceptions to this robust generalization, I can now imagine how an exception could

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arise.

Consider, for instance, two Northwest Native American languages of the U.S., Nez Perce and Montana Salish. In both of these speech communities, speakers are more likely to use native morphemes to build words for new objects than to borrow the words. For instance, the Nez Perce word for 'telephone' is *cewcewín'es*, literally 'a thing for whispering'; and the Montana Salish word for 'automobile' is *p'ip'úyšn*, literally 'it has wrinkled feet' (so called because of the tracks left by car tires). Now, both Nez Perce and Montana Salish do have some loanwords from English (and also some from French), but they have very few, especially considering the very intense contact situation: the few remaining speakers of both languages are all fully bilingual in English, and the languages are seriously endangered. So the reason for the lack of lexical borrowing in these languages must be sought in the speakers' attitudes—a vague and poorly understood concept, but clearly a crucial social factor that must be taken into account in any effort to explain contact-induced change.

Montana Salish, at least, has undergone no, or virtually no, structural interference from English (or French), so it is not a counterexample to the prediction in T&K about lexical borrowing (of nonbasic vocabulary) before basic vocabulary or structural borrowing. But a simple thought experiment shows how a genuine counterexample could arise: suppose that speaker attitudes militate against the adoption of foreign words, so that there are no loanwords at all. Suppose further that structural interference does occur, and that (as is often the case) the speakers don't notice it because the borrowed structural features are less salient than the vocabulary. This hypothetical but not implausible situation would be a counterexample to the T&K prediction about what gets borrowed first.

Notice the common thread in the examples cited in this section: the starting point in each case is a very well-supported generalization about types of expected interference and/or co-occurrence of interference features. And in each case the failure to fit the predicted linguistic situation is explained by a confounding social factor. The moral is clear: when a robust linguistic generalization is violated, look for a social reason for the violation.

3. CONTACT-INDUCED CHANGE: SOCIAL FACTORS AS PREDICTORS.

Once we turn to social factors, we see immediately that it is possible to make one truly

exceptionless prediction about externally-motivated change: contact-induced change cannot occur without language contact; specifically, the lending and borrowing languages must be in direct or indirect contact with each other. Unfortunately, though, this constraint is completely trivial and uninteresting. A somewhat less trivial version would be that contact must be relatively intense for structural features to be transferred from one language to another; this constraint is useful in spite of the fact that 'relatively intense' is extremely difficult to define, but it is hardly an absolute constraint, because structural features are easy to borrow if the two languages in contact are typologically very close.

Sadly, nontrivial social constraints on contact-induced change don't seem to be any easier to find than linguistic constraints are. Space constraints preclude a systematic survey of the literature, but here are two typical examples.

There is good evidence to support the generalization (in T&K) that extensive shift-induced interference is much more likely to occur when a group shifts rapidly to a target language. (The reason is that speakers who shift quickly are less likely to have achieved full bilingualism in the target language, and imperfect learning is what characterizes shift-induced interference.) But there are exceptions, among them Ile de Groix French, which has been heavily influenced by an Ile de Groix Breton substratum in spite of the fact that the shift by local speakers of Breton to French has taken several centuries so far. The reasons for this unusual case must be social, but they haven't (as far as I know) been identified.

The second example is another prediction made by T&K (p. 212): 'a case in which structural interference is firmly established, while loanwords are few...must be the result of language shift, not borrowing.' The reason for this prediction, or (more precisely) retrodiction, is that, unlike borrowing—in the narrow sense of T&K, in which features are introduced into one's native language from another language—shift-induced interference does not begin with the transfer of nonbasic vocabulary. Instead, it begins with phonology and syntax. But again a thought experiment shows that an exception is possible and unsurprising: imagine that Montana Salish does not vanish, but acquires more native speakers (for some social reason) and thrives. Imagine further that the language has no English loanwords at all (thanks to speakers' preference not to borrow words), but that it does have substantial structural interference from English. Finally, imagine that a linguist examines the language several

hundred years from now, and that there is no longer any documentation or other knowledge preserved about the nature of the contact between Montana Salish and English. Now, would that future linguist be fooled by the data into concluding that English speakers must have shifted to Montana Salish and influenced it through that route, rather than what actually happened, which was that Montana Salish speakers borrowed structural features from English? I think the answer is yes—the future linguist would probably be fooled into drawing the wrong conclusion.

As the Montana Salish examples, real and hypothetical, show, social factors can and do skew the historical picture. Not even the most intense contact is guaranteed to produce change in a pressured language, because speakers' attitudes can block such changes completely. In addition, generalizations with solid support can lead historical linguists to the wrong conclusion about the nature of a contact situation if social factors operate to skew things. This does not, of course, make the generalizations useless; but, as with other predictive factors in the study of language change, it does make it necessary to use extreme caution in interpreting the inevitably fragmentary historical record.

4. THREE LINGUISTIC ROUTES TO LANGUAGE DEATH.

An especially dramatic illustration of the problems with deterministic predictions about language history in contact situations can be found in the variety of linguistic responses to imminent language death. Of course there are also social forces operating in any instance of language death: occasionally a language dies because all its speakers die (through massacre, or pestilence, or natural disaster), but in the vast majority of cases language death happens as a by-product of a group's shift to a dominant group's language. Here I will consider only the majority case, and only the linguistic aspects of language death through shift.

The growing literature on language death emphasizes one linguistic route, attrition—that is, the loss of structure without compensatory replacement. This has the overall effect of shrinking the linguistic resources of the dying language, though borrowing from the dominant group's language is likely to be occurring simultaneously. There are now numerous case studies of this phenomenon, as well as theoretical discussions; see, for instance, the essays in Dorian 1989. The processes of change in languages that are losing structure are not

necessarily simple, however. Fenyvesi (1995) found, for example, that only a relatively small number of changes in a dialect of American Hungarian could safely be attributed to attrition alone. Most changes either made the language more similar to English as well as simpler, or made the language more similar to English but not simpler overall. Still, the hallmark of attrition is the loss of structure, and this is well established for many cases.

A second route to language death, apparently much rarer than the first, is gradual replacement of the dying language's grammar by another language's grammar. One example is Ma'a (also called Mbugu), a language spoken in otherwise Bantu-speaking communities in northeastern Tanzania. Ma'a was apparently originally Cushitic. Older sources show traces of Cushitic, or at least non-Bantu, structure; the most recent fieldwork, however, reveals a language that has almost exclusively Bantu structural features, with nothing remaining of the original language except some vocabulary (including much basic vocabulary). This case is not unique; a comparable example, though one in which the two languages were fairly closely related to begin with, is reported by Collins (1980:14): 'Laha has maintained its indigenous language in the face of increasing pressure from Ambonese Malay [AM] but only at the expense of drastic revision of its grammar...Bit by bit the grammar of Laha has become nearly interchangeable with AM grammar'.

The third (and, as far as I know, final) linguistic route to language death is the one currently being followed by Montana Salish. The contact situation is one of great intensity and the language is highly endangered—only about 60 fluent speakers remain, most of them elderly and all bilingual in English—but, aside from a handful of loanwords, there is no evidence of any interference from English in its lexicon or structure.

Obviously social differences must be responsible for the linguistic differences in these three responses to the phenomenon of language death. But it isn't at all clear what social factors one might appeal to; time, for instance, won't explain the difference between the American Hungarian case and the Montana Salish case. The fact that the Hungarians are fairly recent immigrants while the Salish are indigenes is likely to be relevant; but other immigrant languages in the U.S. have vanished without any attrition, and in other places dying indigenous languages have both lexical and structural borrowings from the dominant language. At least at present—and I believe permanently—we may be able to predict that

a language will die, but not what will happen to its lexicon and structure as it dies.

5. Speaker creativity: Language change is unpredictable because speakers are unpredictable.

So far we have seen one major reason why contact-induced change is unpredictable: change can't be predicted because there's no way of knowing that a change will take place. There is also another source of unpredictability, however—in effect the reverse of the 'no-change' source: speaker attitudes can cause change as well as blocking it.

The structural evidence to support this assertion comes from cultures far from the European-American culture region, but lexical evidence is readily available even in Western civilization: human beings are quite capable of inventing new words, and they do so with great frequency. Teenagers are especially adept at this skill—witness the prevalence of ever-changing in-group slang in the teenage population—but adults also invent words. Indeed, some adults earn their living by thinking up names for new commercial products.

To many linguists it seems a great leap from deliberate lexical innovation to deliberate structural innovation, or for that matter to deliberate resistance to change; but to others the gap does not seem so great. We can all agree, however, that it is usually difficult or impossible to know whether a change is deliberate or not. We don't know, for instance, whether Nez Perce and Montana Salish speakers decided consciously to invent new words rather than borrow them from English; the same is true of the massive structural replacements in Ma'a and Laha and of Bretonized Ile de Groix French. The attrition in American Hungarian is relatively unlikely to be deliberate, because some current speakers haven't used the language much for the past forty years or so, and others never learned it properly in the first place. Or, to take an example of non-change, we may wonder whether it is accidental that the Baltic and Slavic languages are the only Indo-European languages that have preserved large portions of the Proto-Indo-European system of noun declension. We do know that Uralic speakers, whose languages have highly-developed case systems in their noun declensions, shifted in sizable numbers to Baltic and Slavic languages; shift-induced interference could therefore be responsible for this non-change, but if so it is impossible to tell whether they innovated deliberately or not.

Sometimes, however, deliberate change (or deliberate non-change) seems likely. Ma'a speakers, for instance, probably made a conscious decision to use their non-Bantu /ɬ/ phoneme in Bantu words while speaking Bantu, because that phoneme is one salient contributor to the community-wide view that Ma'a is a hard language to learn.

More dramatic examples from widely scattered parts of the globe come in the form of deliberate language-withholding: various groups have been reported to use a kind of foreigner-talk or pidginized version of their language when talking to outsiders, so that the foreigners can't learn their real language. In some cases they say explicitly that the watered-down version is enough for the outsiders, and in all cases the withholders appear to know exactly what they are doing. These activities do not in themselves amount to language change, but when pidgins arise from the contact situation, the foreigner-talk version tends to be a major component of the new contact language's structure. Among the groups that have been reported to practice upon outsiders in this way are the Delawares (eastern U.S.), the Motus (coastal New Guinea), and the Hamers (Ethiopia).

An even more dramatic type of case is the creation, in a short time, of a new language that incorporates large chunks of structure derived from two different languages. These new languages typically serve as markers of a new (sub-)ethnic group: Michif, one language of the Métis of Canada and the northwestern U.S., is comprised of French noun phrases in a Cree matrix; originally Quechua-speaking communities in Ecuador created Media Lengua, a mixture of Spanish vocabulary with Quechua grammar; and mixed-blood Russian-Aleut inhabitants of Mednyj (Copper) Island, Russia, created Mednyj Aleut, in which Russian finite verb morphology is combined with otherwise Aleut structure. In these cases, and a few others that have not been so well described, it is difficult to imagine a developmental process that did not include some measure of a conscious decision to create a new language for a new people.

But perhaps the most striking cases of change via deliberate decision come from Papua New Guinea: 'New Guinean communities have purposely fostered linguistic diversity because they have seen language as a highly salient marker of group identity...[they] have cultivated linguistic differences as a way of "exaggerating" themselves in relation to their neighbors....' (Kulick 1990:1-3, citing Foley 1986:9, 27, and others). One community, for instance, switched

all its masculine and feminine gender agreements, so that its language's gender markings were the exact opposite of those of the dialects of the same language spoken in neighboring villages; other communities replaced old words with new ones in order to 'be different' from their neighbors' dialects (Kulick 1990:2, citing Laycock 1982:36).

Changes of these types call into question William Labov's recent comment about the limited possibilities for deliberate change: 'There is a part of language behavior that is subject to conscious control, to deliberate choice, to purposeful and reflective behavior. But as far as I can see, it is not a major part of the language faculty, and it has relatively little influence on the long-range development of language structure' (1994:598).

6. CONCLUSIONS.

Two main conclusions emerge from the examples and discussions in this paper. First, from a linguistic viewpoint any and all contact-induced changes are possible. There may well be social constraints that would permit limited predictions of possible changes, but essentially no progress has been made toward identifying any nontrivial social constraints. In any case, all the linguistic constraints proposed so far in the literature can be and have been counterexemplified (in T&K, in this paper, and elsewhere by other authors). Moreover, Kulick's examples suggest that, indeed, anything goes: if speakers can change their language at will, then linguistic constraints are doomed unless and until we can demonstrate that certain aspects of human language are either not available to speakers' consciousness or not amenable to change at all, or both. In any case, by now the burden of proof should surely be on those who would claim the existence of linguistic constraints on change, whether internally-motivated or externally-motivated.

Second, the fact that a change is possible does not mean that it is bound to occur. On the contrary, many possible and even common and natural changes don't occur in a particular language at a particular time. In this connection it is absolutely vital to distinguish sharply between NECESSARY and SUFFICIENT conditions for change: showing that (for instance) the social setting makes particular kinds of contact-induced changes possible and natural does not imply, much less entail, that any of those kinds of change will actually occur in that setting.

There is some confusion on this point in the recent literature. Most prominently, Silva-Corvalán argues on the basis of English-Spanish interference in Los Angeles that 'only those [linguistic features] that are compatible...with the structure of the borrowing language...will be adopted, disseminated, and passed on to new generations' (1994:134). The problem here is that she is generalizing from a single case study to the whole world. The absence of typologically disruptive borrowings in her data cannot prove that typologically disruptive borrowing is impossible in all contact situations; her empirical results would be relevant for the more general claim only if intense contact were a sufficient as well as a necessary condition for contact-induced change. But it isn't. As we have seen, no social setting guarantees change. In order to argue against the claim that 'any linguistic feature can be transferred from any language to any other language' (T&K, p. 14), it would be necessary to show that some type of change is impossible in ALL social contexts, not just the context of English-Spanish contact in a single U.S. city. And to do that, for her particular generalization about compatibility of structures, she would at least need to show that all the counterexamples that have been adduced in response to this very proposal are mistakes.

Currently such demonstrations are not available, and—for reasons given above—I do not believe they will ever be available.

FOOTNOTES

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