

Regularity and Exceptions in Sound Change

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ABSTRACT

The neogrammarian principle of the regularity of sound change has encountered strong opposition from the very time of its formulation, based essentially on the existence of a non-negligible proportion of exceptions to the postulated regular laws. For most of this century it seemed that peace had been established in the shape of a synthesis which was not fully explicit, weighted in favor of regular sound change but allowing for other processes as well. In recent years, however, this equilibrium has been brought into question, mostly following the work of William Wang, to the point not only of diminishing the part played by regular sound change, but of removing it from the core of the theory, and even denying its place altogether.

Whatever the outcome of the theoretical quarrel which we examine in the first part of this paper, it remains that exceptions are a major methodological problem for the historical linguist. Determining what constitutes a genuine exception and what does not is of fundamental importance to the further development of the theory of sound change. In the second part of this presentation we describe a computer program which we have developed in order to verify on large corpuses the rules proposed by the historical linguist, and to sort out the complete lexicons of the languages under study into "regular" and "non-regular" outcomes.

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Opponents of the neogrammarian principle of the regularity of sound change have from the beginning found their strongest argument in the existence of a non-negligible proportion of exceptions to the postulated regular laws¹. After the vociferous quarrel of the years 1870-1880 between proponents and opponents of the principle, for most of this century it seemed that peace had been established in the shape of a synthesis which was not fully explicit, weighted in favor of regular sound change but allowing for other processes as well. In recent years, however, this equilibrium has been brought into question, mostly following the work of William Wang, to the point of not only diminishing the part played by regular sound change, but of removing it from the core of the theory, and even denying its place altogether.

Whatever the outcome of the theoretical quarrel which we will examine in the first part of this paper, it remains that exceptions, real or apparent, are a major methodological problem for the historical linguist. Determining what constitutes a genuine exception and what does not is of fundamental importance to the further development of the theory of sound change. In the second part of this presentation we will outline a computer program which we have developed in order to verify on large corpuses the rules proposed by the historical linguist, and to sort out the complete lexicons of the languages under study into “regular” and “non-regular” outcomes.

I. The theoretical status of “irregular change”

1. On the psychologically destabilizing effect of exceptions.

It is currently believed by a rather large number of linguists working outside the field of historical phonology that this particular field, in its methods and theories, is a relic of the 19th century, a time when, as everybody knows, linguistic science did not yet exist. We happen to believe that there are advances in science, but also regressions, and that the very recent date of a theory is not automatically a proof of its value. Nevertheless, we could not

simply brush aside this fairly widespread view on our field of research². Was regular phonetic change as postulated by the neogrammarians a complete illusion? Was the part of the data which was irreducibly contingent so large that the apparently regular part would be statistically insignificant?

We were working on Asian languages, on relatively virgin ground and it certainly *was* true that whenever we tried to express a generalization, a swarm of exceptions would appear. Of course, historical linguists know how to handle this: they look for more and more precise phonetic contexts, sometimes getting down to the extreme case where only one item matches the law, and sure enough some of the offending exceptions disappear. Many remain. Discovering the odd analogy or loan word gets rid of some more exceptions. Still some remain.

Looking at the work of our masters, recognized authorities in the field of Asian comparative linguistics, one could plainly see that most of the brilliant insightful papers written on Chinese, Vietnamese, Karen, Lolo-Burmese just picked the examples that worked, those which supported the theory, and generally did not bother to engage in a detailed examination of all the apparent exceptions. Certainly, there was not to be found in our area the “detailed linguistic history” that Calvert Watkins was advocating, and which, he said, “is *de facto* the proof of genetic relationship”. (Watkins 1990:292)

The analysis of such a situation can take two directions. One, which is too plain to stir much interest, is to note that only a handful of scholars have worked in a field that encompasses several hundred Asian languages, while generations of scholars have polished the field of Indo-European. Only epistemological faith in the homogeneity of language and of language evolution can lead us to believe that the same explanatory principles which have been shown to work in Indo-European will be vindicated by Asian languages too, given time and manpower. It is with the view of testing the validity of this first, or “classical”, position that we developed a computer tool aimed at modelling the supposed historical evolution and applying that model to the complete dictionaries of modern languages in a group. If the hypothesis of regular change is reasonably adequate to the data, it can be modelled on the computer. We will give a brief overview of the program at the end of this paper.

Another analysis goes exactly the other way, and spreads back to Indo-European the doubts which have emerged from the study of other language families, making exceptions the rule, so to speak. This is the tack taken by William Wang (who, like us, cites mostly Asian languages) in his well-known 1969 article “Competing changes as a cause of residue”.

A third possible attitude, which could be illustrated by Greenberg’s large scale comparisons, does not reflect on theory but only on methodology, and we won’t discuss it here. Greenberg, like many others, suggests that the rigor of the comparative method should be relaxed when dealing with more remote or less well-known languages. Much discussion of Greenberg’s method has appeared in print or in talks. Since this controversy is not about the actual theory of sound change, we will leave it for the moment. We will come back to it later, with a discussion of our own experience in the light of Hoenigswald’s short and clear exposition (1990).

Of the other two positions, the classical one and Wang’s proposal, the second is clearly more exciting. If you adopt the first analysis of the cause of the incompleteness of our knowledge on most languages of the world you can only conclude “be patient and keep working”. A very dull program, everyone will admit. “Kicking the ant hill”, as we say in French, is sure to attract more attention. And so it has. So much so that, as we said earlier, linguists in other branches than historical linguistics hold it as the current wisdom on the field that the neogrammarian hypothesis has been refuted³. For, as someone explained, that point of view has not been convincingly refuted in the pages of *Language* where it appeared, so it stands. (This position is very much in keeping with the current legalistic view of scientific argumentation: if no contradiction arises, the point is won.) So we will have to come back to Wang’s article and look at it a little more closely. Before we do this it might be useful to state explicitly where we, as practitioners of historical phonology, think we stand.

2. The classical theory.

The theory of phonological change which underlies the work of most historical linguists is that first expressed by the neogrammarians, and modified in accordance with the structuralist and functionalist point of view as developed by the Prague School⁴. It is true that this set of hypotheses, which we will call the “classical approach”, has not been greatly modified recently, and it is also true that it is incomplete. We will see later that Henning Andersen’s

detailed account of a particularly vexing problem for the classical approach goes a long way toward filling one of its gaps. (Andersen 1973:773)

The classical approach posits a set of concomitant forces that influence the development of languages. The resultant of these forces, each pulling in its own direction, is the actual history of the language. This is probably not the way a historian of linguistics would phrase the neogrammarians theoretical outlook. But we are not speaking as historians, and we only want to consider here the neogrammarian inheritance as it has come down to us.

Such a global theory calls for several sub-theories to account for the different factors or “forces”, and for a theory of their interaction. We are very far from possessing such a complete set of theoretical tools. Clearly, as long as we do not possess this complete overall theory, any part of it can be considered as unfalsifiable, since any counterexample can be assigned to the domain of a different part of the theory which is not yet fully explicit. For instance, since the theory of contact and borrowing is still in its infancy, it can be considered an easy outlet for apparent exceptions to the principle of the regularity of sound change.

Sub-theories are not to be understood as defined by different points of view on the facts, but by different subsets of facts, each calling for a different explanation. Most historical linguists agree on which sub-theories are called for, at least in broad categories, and they differ mostly on the weight to be given to each one. Some of the theories we need will fall out of the domain of linguistics proper into that of sociology or history.

The basic tenet or fundamental sub-theory of the whole structure remains the principle that sound change occurs regularly across the lexicon in a phonetically slow progressive manner. We take the principle of the regularity of sound change to be near definitional. “Sound change”, as opposed to a simple “change in sound”, is defined as those phonetic changes which are found recurrently in the lexicon. Three things are asserted about it 1) it exists, 2) it occurs through progressive unconscious changes, and 3) it is at the root of “changes in sound”. Exceptions to regular sound change have to be explained by other laws. One weakness of the theory is that it does not provide a detailed account of how regular change is implemented between generations in a homogenous community, and how the interaction with the other forces (stylistic restoration, analogy, etc.) actually takes place.

We will see that Wang's proposal does not provide us with a set of theories any more complete or tightly knit, and that on the contrary it remains even further from explaining the effective implementation of the concepts he proposes.

3. Wang's radical "lexical diffusion" theory.

It will probably be useful to spell out what we read in Wang's theory, in order to discuss only its main point, and to do this we propose to rename it the "radical lexical diffusion" theory (or RLD) rather than the "lexical diffusion" theory. "Lexical diffusion" is a process of change which has been recognized for a long time as a particular case of linguistic change. Everybody agrees, and has agreed for decades, that those changes in sound which result from analogy, restoration, hypercorrection, imitation of socially prestigious groups and the like are often (though not always) propagated through the lexicon one or a few lexical items at a time. As we said earlier, some historical linguists give more weight than others to these "external" causes of change.

The type specimen of "lexical diffusion" according to the classical theory (as opposed to "radical lexical diffusion") is probably Bloomfield's presentation of the evolution of the pair 'mouse'/'house' in Dutch dialects (Bloomfield 1935: 329-331). Here we see early Germanic [**u:*] developing into a variety of phonetic realizations [*aw*, *y*, *y:*, *ø*, *ø:*] respecting the structural identity of the vowel in the two members of the pair, except in one border area between the [*u:*] and the [*y:*] zone, where we find [*y:*] for 'house' but [*u:*] for 'mouse'. Bloomfield explains the different reflexes by the fact that [*y:*] was a prestige pronunciation imported from the great coastal cities of Holland in the West, and a word like 'house' is more often used in conversation with outsiders, where one is using elegant pronunciation, than a homely word like 'mouse', whence [*hy:*] but [*mu:*]. [On the East of the zone, the North German Hanseatic cities constituted another source of cultural influence, tending conversely to maintain [*u:*]. "Our isoglosses of *mouse* and *house*, says Bloomfield, [...] are the result of the varying balance of these two *cultural forces*" [emphasis added].]

If Wang were only underlining the omnipresence of diglossia if not bilingualism, and emphasizing the importance of studying these factors of

language change to uncover the actual development of a language as opposed to the selective, overly-abstract account that the exclusive study of regular sound change would provide, then this would not be a new theory. Under this interpretation, Wang's position would simply be a methodological position advocating the kind of study which sociolinguists like William Labov are conducting, and which no historical linguist perceives as resting on a theory which contradicts his own, but only on a sub-theory which complements his own sub-theory. This is the way Martinet in a very conciliatory note of 1987 understood Wang's position, and so also Haudricourt and Hagège in their *Phonologie panchronique* (1978:53). This of course makes it acceptable to historical phonologists, but it also empties it of all new substance. This is not what Wang is claiming.

Labov's 1981 article assesses the main point of the theory correctly: Wang asserts that sound change even *in a homogenous community* proceeds *from its very inception* by lexical diffusion. The theory was formulated to account for irregularities which remained, says Wang, after all efforts at treatment with the tools of the classical theory — a thorough analysis of phonetic context, analogical influence, and possible foreign sources for the vocabulary — had been exhaustively tried and, according to Wang, had failed. Faced with such irreducible irregularity, Wang was led to propose that we should establish irregular change at the root of phonological evolution, and stop considering it as a sort of accident as the classical theory treats it⁵. When Wang and his collaborators talk of "competing sound changes", they do not refer to competing *cultural influences* entering the language from the outside, but posit these changes as internal linguistic evolutions, or at least do not think it necessary to distinguish between two different socio-linguistic situations (see for example Chen 1992). "Radical lexical diffusion" theory is meant to replace the theory of regular sound change as a global theory, not to complement it as a sub-theory⁶.

The theory of regular sound change is presented by Wang as a theory which has so to speak been shown wrong with honor. He professes great respect for the accomplishments to which that theory has led, and claims it was the best theory to hold for its time, as it was methodologically very productive; but as everyone knows a wrong theory can be a useful one to hold for some time.

So what theory does Wang offer as a replacement? Let us now compare the models offered by the two theories on a few specific issues.

4. Comparing the theoretical tools provided by the classical theory and by Wang's radical lexical diffusion theory.

4.1. The origin of change versus the diffusion of a change.

Wang's proposal dismisses the study of the origin of change: "Without concerning ourselves with the intriguing question of how the change came into the language in the first place, let us consider how it might actually implement itself." (1969:12). To us, "implementing" means coming into effect, and the intriguing question that Wang eludes is precisely what place the classical view assigns to regular, phonetically and phonologically based, change. So on this point at least we see that Wang's theory is deficient, in the sense that it lacks one rather important component, which the traditional theory provides for.

Distinguishing origin from spreading, is of course not a new idea. We could cite for instance Haudricourt 1940 admonishing linguists that after distinguishing externally motivated from internally motivated sound changes, they should in the study of internally motivated sound changes carefully distinguish between their origin and their spread (Haudricourt, 1940, repr. 1972:51).

It is quite reasonable for a linguist to decide that he is interested in the study of the diffusion of changes and not of their origin. But it is an epistemological mistake to present the resulting sub-theory as a global theory of sound change.

4.2. Marking the exceptions to a change vs marking the words which undergo a change.

Wang presents his new theory in the wording of generative grammar and uses the argument of rule simplicity to justify his proposal. In the case of a change which leaves some residue, Robert King proposes that the exceptional words be lexically marked for the non application of the rule. "It is not the role of grammatical theory [by which he means linguistic theory] to explain exceptions to general rules", he says (King 1969:136), and finds it sufficient to provide a notation for lexical items, or classes of such, which fail to undergo a rule. He admits, with reference to one example, "we have not endeavored

(here) to answer the separate question of *why* [the two words under study] remained exceptions to a regular phonological change". With Wang's proposal, exceptions no longer need to be marked; they are the normal case of a change that is not completed. How the words which *are* selected for the change are spotted by the rule is not specified. If they were to be "lexically marked", that, of course, would not be much more economical than the other way round. But Wang just says that there is no need to mark anything: chance applies⁷. Once more, the missing explanation is declared unnecessary.

So "radical lexical diffusion" does not improve on the classical theory on the above two issues. We will only mention in passing the obvious difficulty which RLD has to face and the classical theory does not, which is developing a convincing model of how and why a change which is supposed to begin as sporadic would, in the great majority of cases, by Wang's own admission, eventually affect the complete lexicon without exceptions. RLD has not yet developed such a model.

There is a single particular type of development where the classical theory is ill at ease, and where RLD would seem to fare better. This is the rare case where, *in a homogenous community*, a change seems sporadic at first, and is seen to become more and more general as time passes. This particular situation, although not the majority case, would constitute a genuine counterexample to the principle of regular change, so we should study it in some detail.

4.3. Sound changes which seem to become more regular as time goes by.

The most vexing problem which the principle of regular change encounters is not that of sound changes arrested in mid course, with a residue; these as we have seen, can be explained by some sub-theory (even if admittedly such sub-theories still need some reworking). Real potential counterexamples to the theory consist of cases where a sound change appears to be more regular at its end than at its inception. It is essential to our point to distinguish monolingual situations from contact situations, since, as we have seen, Wang's RLD theory claims its justification from data for which analogical and contact influences have already been ruled out (see section 3).

It should first be mentioned that, after careful study, clear cases are much less numerous than has been claimed. The founding example of the theory, the apparently unmotivated split of a tonal category in the Chinese dialect of Chao-

zhou (Cheng and Wang 1977) has been convincingly reanalyzed as a case of language contact by Soren Egerod (1982). Egerod shows that there exists in that dialect a colloquial level of speech and a literary level; the latter constitutes a different dialect, with its own regular development from Middle Chinese, distinct from that of the colloquial level. Cheng and Wang do not distinguish speech levels, and note that Middle Chinese tone category III is reflected by two modern tones, a rising tone and a low tone, without any phonological conditioning. Reworking Cheng and Wang's examples and adding some others, Egerod shows that the rising tone ([35] for C. & W., [13] for E.) belongs to the literary stratum, while the low tone ([21] or [11]) belongs to the colloquial stratum. When there is a concomitant difference in the segmental composition of the pairs of words involved, the difference is consistent with the two strata, as in the pair *ɿiaŋ* [rising tone] 'still' versus *tɿiẽ* [low tone] 'above', reflexes of two homophones [**ɿiaŋ*] in Middle Chinese; the correspondence of literary *ɿ* to colloquial *tɿ* is regular. When the same Middle Chinese word has two reflexes in Chao-zhou, they may, by contamination, acquire the same tone. In addition, some words of the literary language have filtered into the colloquial language, along with their tones, so that presently the colloquial level of speech has two reflexes of proto tone category III, low (or low falling) in the regular development, and rising in forms borrowed from the literary level. A detailed study of the history of the language can disentangle the reflexes from different sources, and it is not necessary to renounce the principle of regular change for the sake of such cases.

Nevertheless, there remain cases where actual language contact cannot be shown and where change seems to be diffusing from word to word in a monolingual situation. As a controversy of long standing, Labov cites the case of Gauchat's (1905) description of sound change in Charmay and its criticism by Goidanich (1926), but we could also contribute other such troublesome examples from our own studies. There is no denying the existence of such situations in "apparent time" (i.e. if synchronic differences between generations are taken as representing diachronic change). Briefly, in the lenition of /l'/ in Charmay, the oldest generation had /l' /, the youngest had /j/, and the middle generation had a stable distribution, with /l' / in some words and /j/ in others. (Labov 1981:273). What constitutes a problem here for the classical theory of gradual (lexically) regular change is the fact that some lexical items in the

speech of the middle generation seem to have acquired a stable pronunciation one way and the others the other way. The classical theory would rather have predicted a fluctuating pronunciation affecting the whole lexicon (see also Ohala 1988 on this point).

Labov does not offer an interpretation of this phenomenon in Charmay, but blames the neogrammarian-adepts for a rash dismissal of the facts.

Actually, from Labov's own account, it does not look as if the facts had been dismissed, but only their value as a counterexample brushed aside as "just a case of borrowing" without answering the questions of borrowing of what, from where, why. This was superficial, maybe cavalier, on the part of the "regularists", but not necessarily plain wrong. Dealing with a similar case in Czech dialects, Andersen 1973 handles precisely this most troublesome situation. Without going into the detail of the Czech case, we may follow Andersen in distinguishing two kinds of "evolutionary changes", defined as changes which are "entirely explainable in terms of the linguistic system that gave rise to [them]" : 1) changes which occur without stylistic variation being established and 2) changes where a stylistic variation gets established. In this second case, Andersen proposes schematically the following process: 1) A regular sound change occurs between one generation and the next. 2) A restoration to the old form occurs for some lexical items under the influence of the older generation. This second phase is a typical "contact" situation, the limiting case of "borrowing", and it occurs, like all such events, on an item by item basis, or perhaps one word class at a time. 3) The double reflex of the proto-phoneme is acquired as a stylistic variation by the third generation, which is exposed simultaneously to the speech of both of the earlier generations, with the newer form as the unmarked term. Stylistic variants typically apply on an item by item basis, and unless hypercorrection occurs, they eventually die out. **What is lexically diffused here is not the change, but the restoration.** The change has occurred regularly, at the inception of evolution, and in that sense it is fundamental; it is at the source.

With this scenario, which Andersen expresses as internalized rules of the grammar, and which we have retold in the psychological, or cognitive terms that the rules represent, it becomes possible to understand the fact that although most changes are most regular at their beginning and acquire irregularities as time passes, some changes seem to regularize themselves over a long period of time. It also becomes possible to reconcile the findings of

dialect geography with those of classical comparative phonology. It might be important to note here that stylistic variation is not as likely to be established in all types of social settings. If we want to study “internally motivated changes” as a clue to general phonological processes, we might want to select for study communities where stylistic variation is either small or strictly codified.

With the theoretical background provided by Andersen’s model, cases like Tore Janson’s study of “reversed lexical diffusion” in Swedish might become the norm rather than the odd case, and instead of supporting the case for “radical lexical diffusion” they might actually contribute to the demonstration that lexical diffusion occurs in “contact” situations of one sort or another. In a book devoted to the description of cases of irregular correspondence phenomena which would be likely candidates for an explanation by radical lexical diffusion (Wang 1977), Janson contributes a study of the apparently unmotivated rule of modern Stockholm Swedish which allows the optional deletion of a final *-d* in informal speech, in some specific lexical items. The synchronic description of the situation, with a kernel group of words where all 6 informants could drop the *-d*, surrounded by other groups of words where only some of the informants could, and a last group where no one could drop the *-d*, is reminiscent of what has been described as the spreading of a modification from a core. In this case, however, the core is not categorical as there is no phonological context where the optional rule is always possible. And indeed no criteria (phonological, morphological or semantic) were found for which words could undergo *-d* deletion. On the other hand, conditions were found for which words could not. Looking at history, Janson found that the evolution was the reverse from what could have been postulated by looking at the synchronic data alone: final *-d* deletion is not spreading, it is regressing. Janson suggests that at some point in the past *-d* deletion was a general process in colloquial speech. The narrowing and gradual suppression of this optional rule was introduced first in formal speech, as a refection, and gained ground in informal speech context by context and word by word. Refection with stylistic variation is in conformity with the scenario presented by Andersen, rather than with the RLD view of a *new* change spreading from word to word.

From a methodological point of view it could be considered that the Wang scenario and the Andersen scenario are equivalent: they end up the same. But from the point of view of the theory of linguistic processes of change they are very different. If we wanted to use the terminology of generative grammar, we could say that the one (Wang's proposal) consists in adding or changing a rule, which we should not allow to be lexically determined, while the other (Andersen's) deletes a rule (what is more, a stylistic rule), which is more natural.

4.4. Different phonological material as a cause for different scenarios for change.

Surprisingly for a sociolinguist, Labov follows Wang's suggestion in a later article (Wang 1979:69) that the phonetic or phonological nature of the material subjected to the change may determine different scenarios for change. He does not mention Andersen's proposed solution to the problem of apparent irregular change. So Labov proposes that "we have located neogrammarian regularity in low-level output rules, and lexical diffusion in the redistribution of an abstract word class into other abstract word classes" (Labov 1981:304).⁸ This formulation is a little abstract, but the conclusion of his survey of potential candidates for each of the two competing theories on the preceding page is more straightforward: "...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, and changes of place of articulation in consonants." (1981:303)

We could adduce an argument from our own studies (Mazaudon, in prep.)⁹ in favor of such a dichotomy. The place where we meet with plain irregularity (as opposed to sporadic exceptions) in the Tamang group of languages is in the disappearance of vowel length. Our interpretation of this fact though is rather functional and probably more in Andersen's line. Throughout the Tamang group, a closely related group of dialects spoken in Central Nepal, there occurs a loss of an old vocalic length contrast. In some dialects there is a transphonologisation (long *a → ɑ for instance, while short *a remains a), and this is neogrammarian regular. In other dialects there is no transphonologisation, but a simple merger, and this is either regular or not depending on the dialect. Our interpretation of this is that, in the case of a

merger, two phonemes which used to be distinctive become simple variants for some speakers. They remain perfectly perceptible and conscious, however, which allows for imitation on a word by word basis among speakers.

Transphonologisation, on the contrary, has to be automatic (and unconscious).

We might modify the Wang/Labov classification slightly in such a way that we have on the one hand lower level, output-level, more phonetic, and, we would add, less conscious changes, which would be relatively regular, and on the other hand higher level, more abstract, and more conscious changes, which would therefore be less regular. By reintroducing the cognitive dimension of the speaker's greater or lesser consciousness of the event, we can maintain the neogrammarian sub-theory of "contact", as refined by Andersen, as the major cause for exceptions to changes whose basic implementation remains lexically regular and phonetically gradual¹⁰.

5. Methodology: Is the comparative method applicable to all language groups?

We believe that we have established that the theory of regular change as the basis of phonological change remains our best available theory. It remains that the disparity of results which we noted at the outset between different language areas is still puzzling. If regular "neogrammarian" change is the basic motor of linguistic evolution, why the disparities? Leaving for now the theoretical for the methodological point of view, we could ask : Is the comparative method equally applicable everywhere? After expressing surprise at the fact that such a question is still raised in the 1980's, and reasserting that regular sound change does occur in all languages of the world, and that the comparative method is the only tool useable for its study, Hoenigswald (1990) shifts to a methodological consideration and distinguishes "reconstruction-friendly" languages from "relatively barren fields". "It is no accident," he says, "that the comparative method and its adjuncts were hammered out by Indo-Europeanists and not by Romance scholars." In the same way, Dravidian, Algonquian, and Austronesian are cited as "reconstruction-friendly", while Turkic, Finno-Ugric, and Athapascan are cited as procuring "less joy" to the historical linguist. This is a question not of amenability to the comparative method but of yield. "Apparently the productivity of the comparative method and its adjuncts depends a good deal on an optimal relationship between certain morphophonemic characteristics both in the descendants and in the proto-language at which the reconstruction aims." (1990:375-381)

As it happens, the Tibeto-Burman languages which we work with are not very reconstruction-friendly. They are reconstruction-friendly in the area of evidencing gradual regular change. Tamang dialects, for example, present all the intermediate stages of tonal evolution showing how a low tone, via a melodic tone, can turn into a high tone, which allows the linguist, by detailed analysis of sufficient data, to avoid positing as a process the abstract monster which some call a “flip-flop” (Mazaudon 1978). On the morphological level, however, Tibeto-Burman languages are extremely “unfriendly”, for they have very little synchronic morphology on which to do internal reconstruction, while they have numerous lexical traces of dead morphology, which gives rise to masses of word families whose members are seemingly impossible to match accross dialects.

Perhaps this is the reason why, although we believe that we have every reason to hold to the classical theory for the moment, we nevertheless wished to pay due respect and attention to exceptions, and thought they deserved the effort it took to develop a whole machinery in their honor, which we will briefly outline in the second part of this paper.

In conclusion, why do we think it is important to isolate the regular component of sound change and study it? One answer is that regular sound change is our best guide to the identification of phonological units and to the identification of natural classes of phonological units in general phonology. But only regular sound change has this value. It must first be isolated from other changes which may illustrate sociological or historical, but not phonological principles.

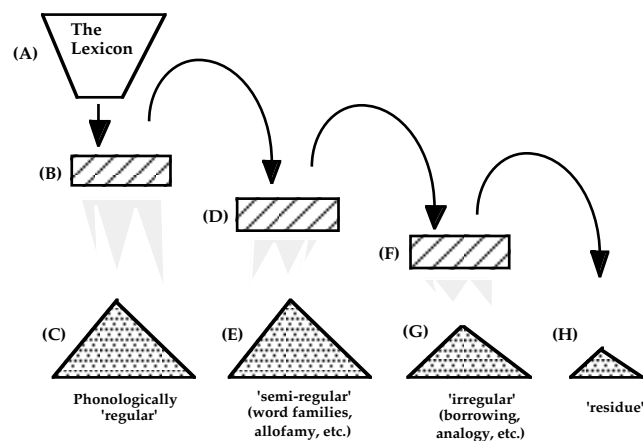
II. R. E., The Reconstruction Engine, a tool for evaluating the regularity of correspondences

A complete presentation of the computer model is out of the scope of the present paper. We have presented a detailed explanation of its workings, along with a review of previous attempts at modelling sound change on the computer in Lowe and Mazaudon 1994 . The program is still, in many respects, a prototype. It now works correctly in the application of one-step changes from

one phoneme (or phonemic group) to another. We are presently studying ways of introducing the chronological application of rules.

The aim of RE is to alleviate the work of the linguist, while making it more consistent by checking his own rules against the data. There are a number of side benefits too: constituting a data-base of verified historical changes with their examples attached is one.

A simple and vivid way of summarizing the workings of the comparative method and the use of RE within that framework is by the metaphor of a series of sieves, the sub-theories which we talked about earlier, through which the linguist tries to filter the data. This can be schematized as in Figure 1.



Key:

- A. The complete lexicon.
- B. Regular sound change (modeled by RE proper).
- C. Regular, “expected” reflexes of the ancestor forms.
- D. Domain of “protovariation”, perhaps due to morphological/derivational processes; handled by RE with “fuzzy” constituents.
- E. Sub-regularities elicited through relaxed constraints (word families, etc.)
- F. Sociolinguistic explanation. Domain of lexical diffusion and other sporadic processes.
- G. Borrowings, analogized forms, hypercorrections, prestige pronunciations, etc.
- H. The “mystery pile”: counterexamples and other troublesome words.

Figure 1: RE and the comparative method

As we can see in figure 1, RE comes into play, with different parameter settings, 1) in the checking of regular “neogrammarian” change (sieve B, data set C in figure 1); and 2) in the study of semi-regular phenomena, like word-families, regular analogies etc. (sieve D, data set E in figure 1).

RE works as a bi-directional rule application mechanism, either interactively or in batch mode as schematized in figure 2.

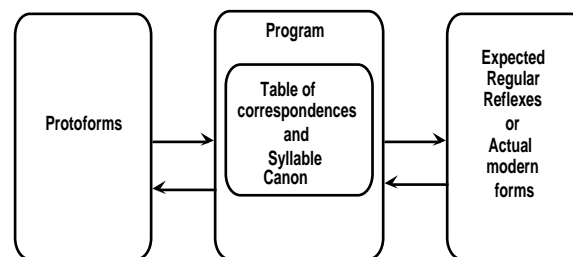


Figure 2: Input-Output diagram of RE's basic projection functions

6.1. The basic “data” components of RE (in computer terms) are:

- 1) The individual *machine-readable lexicons* of a number of putatively related languages, as complete as they can be, and without prior processing (such as identification of cognates, etc.). The absence of required pre-processing is essential to rule out any preconceived doctoring of the data with a view to comparison. It is also a convenience for the linguist who can use the same data-base to produce ordinary dictionaries of the languages and for comparative work. The format used of course has to be regular. It can presently be a tabular format, with the different rubrics of a dictionary entry separated by tabs, or a flat data-base format — we have used the format of Robert Hsu's LEXWARE — or a comparative data-base format.
- 2) A *set of correspondence rules* established by the linguist, and meant to be checked and refined against the data, which is called the “Table of Correspondences”.
- 3) A *description of the phonotactic relations* between the classes of proto-segments, which we call for short the “Syllable Canon”, but which can express, if the linguist chooses, a morpheme canon.

To these are added a number of program settings, and of parameter tables which allow refinements of the treatment.

6.2 Basic operation of R. E.

Using the basic components listed above, or replacing the first one (lexicons) by an item by item inputting of forms in the interactive mode, RE, given a supposed protoform, computes the values of potential derived forms in the various daughter languages, or, given a modern form, computes all its possible ancestors, according to the Table of Correspondences and the Syllable Canon. Following John Hewson's pioneering work, we call the first application "downstream", flowing with time, and the second "upstream", in the reconstruction mode. The downstream function in interactive mode is illustrated by Figure 3.

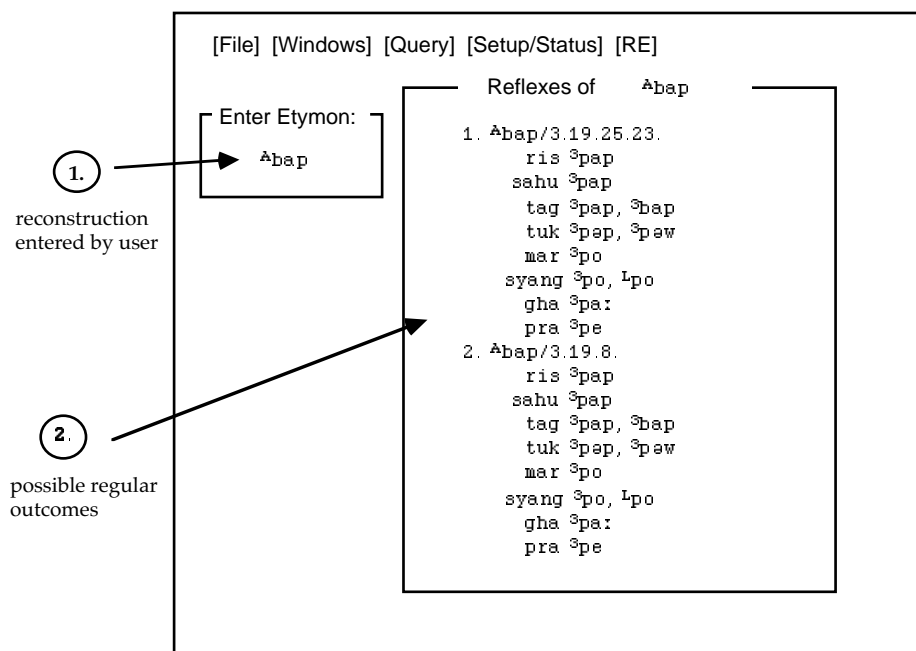


Figure 3: Example of the derivation of modern forms in several languages of the Tamang group from a proposed reconstructed etymon.

In upstream mode, each modern form in a given language is likely to derive from a number of different possible ancestors. By "triangulating" on the

possible ancestors of each form of a putative cognate set, R. E. can decide whether a common form is reconstructible for the set, and whether that form is unique or not. The process is illustrated in Figure 4.

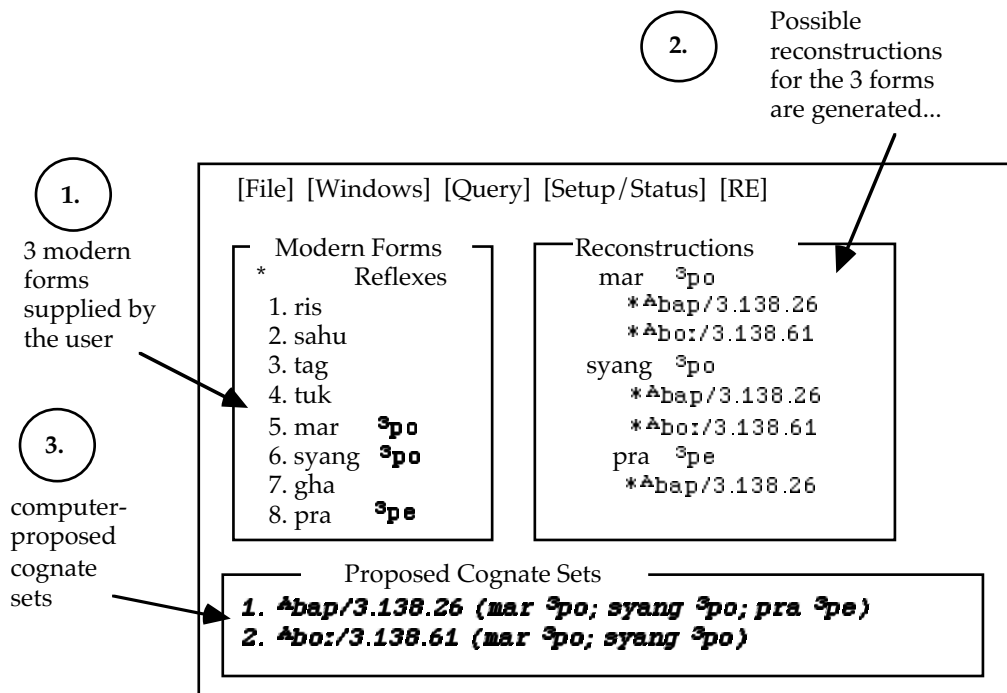


Figure 4: Reconstructing potential proto-forms, and triangulating on several series of proto-forms: the proto-form supported by all (or most) modern reflexes is proposed as the “correct” ancestor of the cognate set

When operating on whole lexicons in reconstruction mode, RE computes possible ancestors for each form of each language, conflates and sorts the lists, and proposes potential cognate sets. The process is illustrated schematically in figure 5.

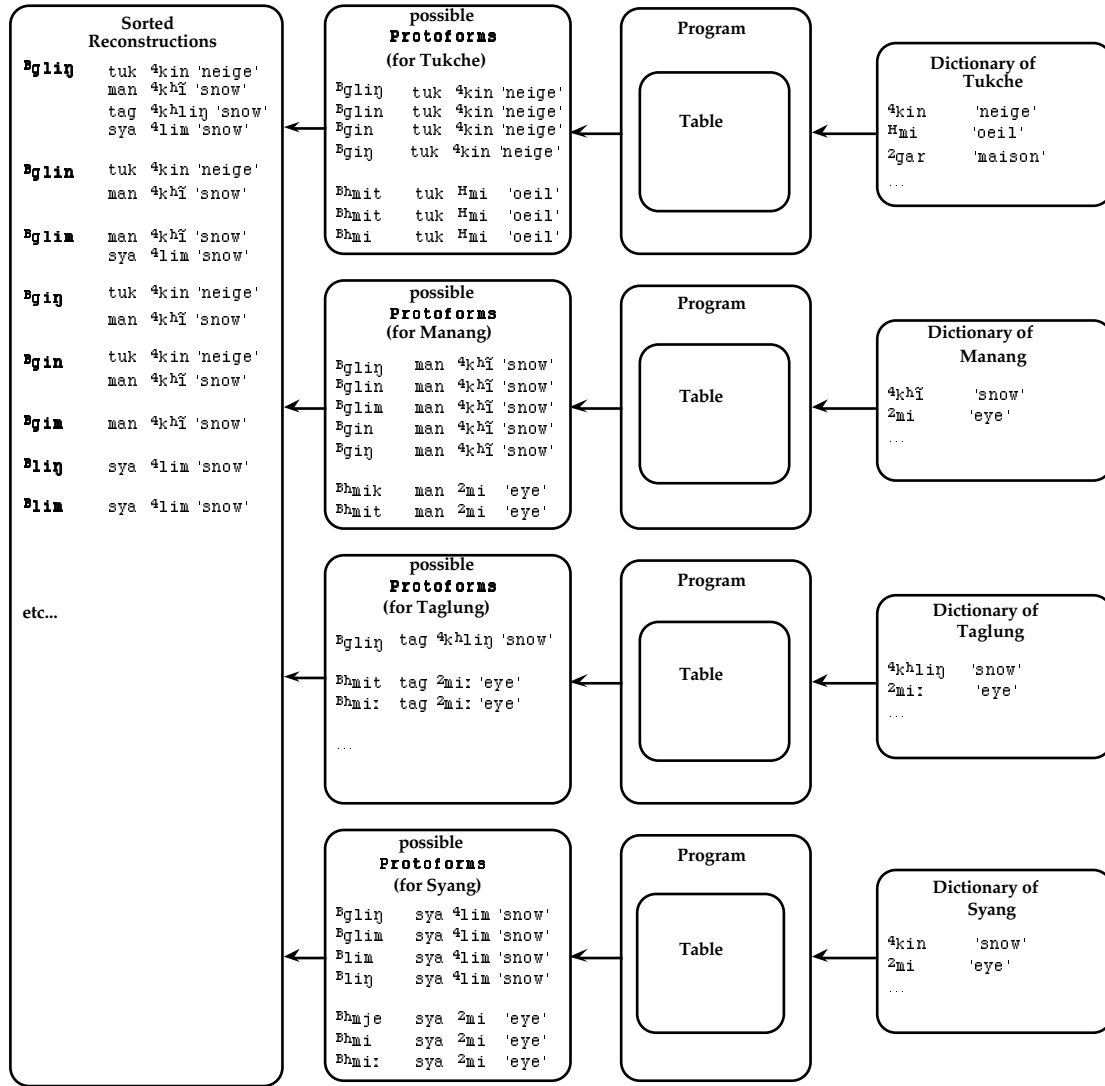


Figure 5: Upstream computation in batch mode

This computation is done independently of meaning, which is evaluated after the fact by the linguist. Some additional modules permit the linguist to record his semantic evaluations of proposed sets in order not to have to replicate his work on successive runs of the program.

The aim of the program is to verify the correctness of the Table of correspondences, to attach to each correspondence the list of example sets which illustrate it, and to enrich the list of cognates sets and reconstructed forms for the language group under study. The process by which this is done is by successive runs of the program, interspersed with corrections, done by the linguist upon examination of the results, to some or all of the “data”

components. “Data” in this sense of course means the analysis proposed by the linguist.

6.3 R. E. and semi-regular correspondences.

In its use for studying less fully regular correspondences, R. E. can make use of “fuzzy filters” which are parameter files instructing the program to conflate some particular phonemes, features, or groups of phonemes into classes and to operate on these classes. For example, we know that in the Tamang group there occurred in some unrecorded past a morphological process by which causatives of verbs were formed. This long dead process has left traces in modern languages in the guise of pairs of lexical items differing in mode of articulation of the initial consonant or in tone. As often happens, not all members of these morphologically related pairs have survived, and which one survived in related languages is not systematic. If a different member of the pair has survived in two daughter languages, we consider these as having an “allofamic” relation, following Matisoff’s terminology. In order to study such allofams, it is useful to be able to ignore selectively the mode of articulation of the initial consonant, or the tone, in the inter-dialectal comparison. Fuzzy filters are used for this purpose. This is the application of RE symbolized by the D sieve in figure 1.

We believe that tools like RE are now necessary in order to allow comparative linguistics to escape the criticism of ignoring any data which does not fit its theory or theories. When used in batch mode RE gives as its output lists of lexical items arranged in proposed cognate sets, and lists of lexical items which the program could not fit into sets according to the rules provided. Every item in the lexicon is thus listed in one category or the other, regular, or irregular. It remains for the linguist to re-examine the “irregular” category to discover more subtle regularities or modify his theory.

NOTES

¹ This paper benefitted from comments from participants in the 1993 Annual Conference of the Linguistics Society of Belgium, and discussions with Boyd Michailovsky, Françoise Ozanne-Rivierre, Laurent Sagart, Sylvain Auroux and Beatrice Goddard-Wendling, none of whom necessarily agrees with all the points developped here.

² A recent paper by Rebuschi addresses similar concerns. His first sentence reads as follows “Le but de ce travail est de réhabiliter la linguistique historique et comparative à ses propres yeux d'une part, puis à ceux de la grammaire générative [...]”

³ Kiparsky 1993 states “The existence of an important class of exceptionless sound changes grounded in natural articulatory processes is not in doubt of course. It is the claim that it is the *only* kind of sound change that is under question.” Unfortunately this is *not* the way the question is raised and the very existence of “neogrammarian” sound change has been put in doubt. It is this doubt that we address here.

⁴ Kiparsky 1988, 1993 seems to believe that the currently received theory, which he refers to as the “neogrammarian/structuralist” approach, is a direct heir of the NG view in its most extreme form, which Kiparsky states as “[...] sound changes originate through gradual articulatory shifts *which operate blindly without regard for the linguistic system*” [emphasis added], to which he opposes his own view that sound change is phonological rather than phonetic. This discussion is very surprising to historical linguists who have believed, for a good 75 years, that when they used the traditional expression “phonetic change” (*changements phonétiques*) or “sound change”, it was understood as a technical term meaning precisely “phonological change”. Maybe the subtitle of such a non-recent work as Martinet’s 1955 *Economie des changements phonétiques: traité de phonologie diachronique* would suffice to indicate that systemic pressure on evolution is not a discovery of generative phonology. How else could one understand the ever-present insistence on the role of “cases vides” as priming factors for evolution in the works of practioners of the “neogrammarian/structuralist” approach?

⁵ “But what about the residual forms which remain, after we have taken into account the phonetic and morphological factors and the multilayered structure of the vocabulary? What explanation can we give for sounds changing

differently *under completely comparable conditions?*" [emphasis added] (Wang 1969:10).

⁶ In later articles, Wang and his collaborators have given lip-service to "neogrammarian" type sound change, either in passing remarks in general conclusions (Wang 1979) or in the form of slightly more guarded ways of expressing their position "*most (not necessarily all) types of phonological changes are phonetically abrupt but lexically gradual*" [emphasis added].

⁷ In more recent papers Wang has appealed to frequency as a priming factor for change: the most frequent words would change first. This hint has not been developed, and there seem to be many counterexamples to such an hypothesis.

⁸ Also quoted in Baldi 1990:10.

⁹ Mazaudon, in prep., Dictionnaire comparatif et reconstruction des langues du groupe Tamang (Nepal).

¹⁰ Even in contact-induced changes, the degree of consciousness seems to play a role. For example, a regular pattern is established in the reintroduction into Vietnamese of glottalized stops by unconscious imitation of small non-dominant Austro-Asiatic languages which had not lost them. (Haudricourt, communication to the Society of Linguistics of Paris.)

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