

László Kristó *Linguistic reconstruction:
methods vs. interpretations**

0 Objectives

The aim of this paper is to investigate the central methods of linguistic reconstruction and the theoretical models associated with them. By “central methods” I understand the two basic ones, Comparative Reconstruction (CR) and Internal Reconstruction (IR). They can be considered central because, despite the advent of other methods (such as glottochronology), they still form the nucleus of reconstructive techniques, without which no serious reconstruction can be attempted. This is because both methods, unlike other, more marginal ones, are strictly based on the absolute *sine qua non* of any historical linguistic study: the Regularity Hypothesis. Both CR and IR will be considered in the light of their alleged theoretical background, which for CR is held to be the Neogrammarian model, while IR relies on structuralism. I will come to the conclusion that this distinction is irrelevant for the method itself, because Neogrammarians and Structuralists differ not so much in the method but in the interpretation of the results of reconstruction; the difference follows from the different theoretical models of phonological change. I will point out, furthermore, that because IR is not a historical method as such but, instead, it is the historical interpretation of a basically non-historical procedure (from which it follows that it has serious flaws), CR still remains the central (possibly only) really historical and exact method of reconstruction, but not because it is inherently historical, but thanks to the nature of the data it works with.

1 Comparative reconstruction¹

The theoretical basis of CR is the Neogrammarian doctrine known as the Regularity Hypothesis. In its strongest form, it claims that all sound change is regular in the sense that it occurs according to fully mechanical phonetically conditioned rules, and due to this it admits no exception. To take a simple example, if in a language L voiced stops are lenited to fricatives intervocalically, they will always do so in that environment, and no word can escape the consequences of the change. The Regularity Hypothesis makes it possible to set up regular sound correspondences between related dialects.² To return to the previous example, if L has a relative L'

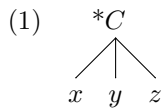
* My heartfelt thanks go to András Cser, Ádám Nadasdy, and László Varga, who have made valuable comments on this paper. Of course, I am alone responsible for remaining errors.

¹ For detailed descriptions of both CR and IR, see, e.g., Bynon (1977), Fox (1995) or Lehmann (1992).

² Or languages. I will use the terms ‘dialect’ and ‘language’ interchangeably, since there is no principled difference between them from a comparative viewpoint.

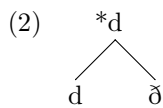
in which the given change does not take place, then any intervocalic voiced fricative of L regularly corresponds to a homorganic voiced stop in L' , and vice versa.³

The existence of regular correspondences is what makes CR possible. The method is a rather simple mathematical operation in its first stage, as pointed out by Lass (1993:161f). I will use his example here. Take three related languages X , Y , and Z ; let x , y , and z , respectively, be regularly corresponding units (sounds, for our purposes) in these languages. We can create a set $C = \{x, y, z\}$, where C stands for “cognateness”. This, of course, is a simple relation, which is symmetrical ($xCy \supset yCx$) as well as transitive ($(xCy \text{ and } yCz) \supset xCz$). So far C is nothing but a label to give a name to the set; as a result, we have not yet made any historical statement: all we have is a static pattern. We can, however, move on to the second step and assume that C is not a label but an entity, i.e., a physically existing object which is related to x , y , and z historically: it is their ancestor. The relation ‘ancestor of’, of course, is neither symmetrical nor transitive. We can conceptualize the relation in the form of a tree as in (1):



If $*C$ is a physically existing object, it is possible (or even obligatory: see §3.1) to assign physical properties to it. In case x , y , z are sounds, these will be, of course, phonetic properties. This is the final stage of reconstruction (at least on this level: we can move one level up and reconstruct morphemes, etc.).⁴

Let us take a specific example, using material illustrating a correspondence that derives from intervocalic spirantization. Standard Portuguese intervocalic [d] regularly corresponds to [ð] in Castilian Spanish: Portuguese *cantada*, *lado*, *cidade* (‘sung-FEM’, ‘side’, ‘city’) correspond to Castilian *canta[ð]a*, *la[ð]o*, *ciu[ð]a[ð]*, respectively. We can set up $C = \{d, \delta\}$, and assign C historical-ontological status, which makes it possible (or even requires: see below) to assign a phonetic value to it. Since we know that intervocalic position is a typical lenition site, we assume that $C = [d]$ in Proto-Western-Romance, the ancestor of both languages.⁵ We can represent this as in (2):



³ Assuming that no other change has taken place in either language as well as that there are no voiced fricatives in the proto-language, at least intervocalically.

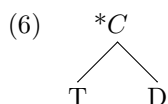
⁴ Cf. Fox (1995:58ff) for proposed stages of reconstruction.

⁵ This stage itself represents a lenited state of intervocalic stops, since the ultimate source of these cognate items is Vulgar Latin **cantata*, **latu*, **civitate*, with an intervocalic voiceless stop.

Take as an example the word-final devoicing of stops in German. Let T be any voiceless stop and D a homorganic voiced one. There are many stems which sometimes surface with a final T (if it is also word-final), sometimes with a final D (if it is not word-final but followed by suffixal material). So, for example:

- (5) Ra[t] ‘wheel-NOM.SG’ ~ Ra[d]es ‘wheel-GEN.SG’
 Wei[p] ‘woman-NOM.SG’ ~ Wei[b]es ‘woman-GEN.SG’
 Ta[k] ‘day-NOM.SG’ ~ Ta[g]e ‘day-NOM.PL’

We can now set up a correspondence set $C = \{T, D\}$ as the first step. As the second step, we interpret C as the ancestor of both T and D, as in (6):



As the final touch, we assign phonetic properties to $*C$. We can safely assume, on both theoretical and language-internal grounds, that $*C = D$, i.e., historically, such occurrences of voiceless stops derive from voiced ones via the regular sound change of Word-final Devoicing.

Anyone familiar with phonological analysis will have noticed that this is the same as what one does in a process-based paradigm (such as the SPE model) when analyzing synchronic alternations. Indeed, Anttila (1989: 264) says, “Internal reconstruction [...] is *exactly* the same as morphophonemic analysis” (emphasis mine). But then, where is the difference? After all, synchronic analysis is *not* Internal Reconstruction. The answer is that the difference lies not in what one does but in how one interprets the results. In synchronic analysis, we set up C as a set and assign a theoretical status to it, and we may as well stop there, but we can go on and claim that the alternants are actually derived from it (if we believe in phonological processes); in other words, we can regard C as an underlier. Internal Reconstruction is none other than assigning historical status to C ; that’s where IR is, for the historian, more than simply a synchronic analysis: that’s why it’s something historical. In other words, set up an alternation, label it, and whether you do IR or synchronic analysis depends on the content you give to your set: in synchronic analysis, it is “alternates with”; in IR, it is “cognate with”.⁹ I will return to this point later, but now let us see the limits of IR and its fundamental dependence on CR.

As Anttila says, IR is but morphophonemic analysis as far as the method goes. In fact, I take the opportunity to correct Anttila here: IR is not necessarily based on *morphophonemic* alternations, although this is indeed the majority case: any purely *phonologically governed* alternation is liable to such an interpretation. (See below for an example involving English R-Liaison.) Second, I must disagree with Anttila in equating IR with synchronic analysis. IR is not the same as synchronic

⁹ “Cognate with” is understood here, of course, in a non-comparative sense (roughly, “having the same ancestral form”).

analysis: it is a historical interpretation of the same data as used for synchronic analysis. Or, to put it differently, IR = synchronic data + historical interpretation.

Nonetheless, IR has serious flaws. I illustrate this with two examples. First, take English Spirantization, illustrated by pairs like *defend* ~ *defens(ive)*, *omit* ~ *omiss(ory)*, etc. At first sight, we might be tempted to use IR to reconstruct an earlier single stem form underlying the present-day alternation. But we know that these words are Latinate borrowings, in which the alternation is already present: in other words, English borrowed the alternation hand in hand with the words. It would be wrong to assume a Spirantization Rule as a sound change in the history of English: a synchronic rule, then, is not necessarily a historical change.

Second, although an alternation may point to a historical change, it may do so in the wrong way. Consider those non-rhotic accents of English which have obligatory full R-Liaison, i.e., both Linking-R and Intrusive-R, such as London English. This means that a set of words ends either in a non-high vowel or a non-high vowel + /r/, as in *car* /kɑ: ~ kɑ:r/, depending on what follows the word. In a synchronic analysis, we can assume a rule of R-Insertion to handle the alternation. But we know that historically, there are two distinct processes: (i) R-Dropping, (ii) R-Insertion. If we did not have any historical information at our disposal, we could not choose which process to assume; it is due to the testimony of other accents (as well as orthography, grammatical descriptions, etc.) that we know what happened. For example, take the words *spa* and *car*; both have R-ful and R-less alternants, in exactly the same environments; there is no difference between the two words. Historically, though, one of them is R-ful, the other R-less; but based on the present-day language alone, we can't tell which is which. The appearance of Intrusive-R results in what we can regard as a kind of merger: the historically distinct categories -Vr# and -V# merge, yielding a situation where they have become context-dependent variants. This reflects a basic problem one must face when doing IR: unconditioned merger, which renders previous contrasts unrecoverable for the method. To sum up, IR requires comparative backup, and therefore, it is insufficient to solve this particular problem.

This much has often been said. Yet, we must be careful here, because CR is not almighty, either. Consider another type of accent, in which there is no R-Liaison whatsoever, such as Southern US English (SUSE). Here, it would not even occur to anyone to reconstruct anything, because we have no alternation: *car* is always pronounced /kɑ:/. Let us now imagine the situation that all we have access to is SUSE and London English. In an analysis of the latter, we are faced with the problem described above; but would SUSE provide the necessary comparative backup? It would not. We are still faced with the same problem, because the difference between two accents can still be accounted for in two ways: either by assuming that SUSE is conservative and London E innovates (via R-Insertion) or that SUSE is innovative (R-Dropping). We need even further comparative support, either from rhotic accents or from ones which have linking but no Intrusive-R (if there are any such accents left; maybe conservative RP speakers have it). The possibilities of rhoticity and R-Liaison are summed up in (7):

- (7) > London E: ka: ~ ka:r, spa: ~ spa:r
 > Southern US E: ka:, spa:
 > GenAm E: ka:r, spa:
 > ? Cons RP: ka: ~ ka:r, spa:

To sum up this lengthy discussion: the fact that IR is not flawless is not in itself an argument against it or in favour of CR, because CR is not flawless either. The point is that when we have access to both IR and CR, and the two disagree, CR takes precedence, but this is a logical consequence of the fact that CR works with data from several dialects. It is in this sense only that CR is superior.

3 Comparative reconstruction: the method vs. its interpretations

In this section, I will examine CR against the two theoretical models widely used in reconstruction: the Neogrammarian and the Structuralist models. I will argue that the two theoretical frameworks differ not so much in their reconstructive methods but rather in how they interpret the results. First, however, I will discuss two differing views on the status of reconstructed entities: the “idealist” versus the “realist” positions. I will argue that the idealist position is untenable, not only because of practical and/or linguistic reasons, but because, on a more general plane, it is hopelessly unscientific.

3.1 Idealism vs. realism

In section 1, I established three steps of reconstruction: (1) setting up the correspondences; (2) assigning historical status to the set label; (3) assigning phonetic properties to the reconstructed item. Step 1 is the basis for any reconstruction whatsoever; but what about the rest? Why not omit Step 3? Or, why not omit Step 2 as well, and say, with Meillet, that “the reconstructions are merely symbols with which we express the correspondences in an abbreviated form”?¹⁰ Indeed, this stance has been taken by many linguists, including Meillet, and it has been labelled the “idealist” or “formulist” position (as opposed to a “realist” stance).¹¹ For an idealist, then, there are either no “proto-segments”: reconstructed forms are just set labels; or, a bit less abstractly maybe, “proto-segments” are not labels, but quite abstract (past) entities, whose phonetic content is immaterial; what counts is that the entity underlies the correspondence set. (I do not see an essential difference between these two idealist views; the second is but a softer version of the first.) The main reasoning behind this view is that we cannot know the exact phonetic quality of the proto-segment anyway, which means we can only speculate about it; and science should end where speculation begins. Such arguments are, though they sound good, quite misguided (and misleading).

Defendants of the “realist” position have pointed out several weaknesses of the idealist argument, and I will not enumerate all of them here.¹² I provide one main

¹⁰ Meillet (1964: 42); my translation.

¹¹ Fox (1995: 7–17), Lass (1993, 1997: 270ff).

¹² Lass (1993) is an elegant overview.

argument only, elaborating on Roger Lass's observations and criticism. Imagine a situation when we end up with five correspondence sets for the vowel system of the proto-language *L. Furthermore, the daughter languages show remarkable unity in the phonetic quality of the vowels; say, we can safely reconstruct two high vowels, *i and *u, two mid ones, *e and *o, and a low one, *a. Now, the idealist would say that we do not really know the quality of these vowels: the high and mid vowels may have been tense or lax; as for the low vowel, was it front, central or back, rounded or unrounded? We do not know. The logical conclusion, the idealist says, is that the whole thing is hopeless, and we had better think of *a, *e, *o, *i, and *u as mere labels. (We might use perfectly different symbols, too, to represent them, e.g., * a^{sc} , * e^{sc} , etc., avoiding any association with phonetic qualities.) But the idealist is totally wrong here. For we might not tell exactly if, say, *a (or * a^{sc}) was back or rounded or whatever; but we know that it was low. Similarly, we know that *i was front high unrounded; and so on. Therefore, using *a is not quite the same as using * a^{sc} ; in fact, the latter symbol gives no reason why it should have low vocalic reflexes in the descendants. Of course, it is much more ideal if we can tell that it was "low back rounded"; but the fact that we are unable to do so does not mean that we should not say anything. And even the most extreme idealist would not in practice reconstruct * a^{sc} , because he knows that to say that it was low (or that it was a vowel in the first place) *is* something. The dividing line is not between knowing that it was "low" and knowing that it was "low back rounded": the dividing line is between knowing something about it or not knowing anything. Schematically:

(8) (Nothing) \longleftrightarrow (V < low V < low back V < low back rounded V)

Therefore, once we say *anything at all* about the phonetic quality of our proto-segment, we are in principle realists.¹³ I do emphasize this very strongly, because once we have specified *some* phonetic content, there is no principled basis how far we must go into details to abandon idealism and turn into realists. This is a science-theoretic requirement: either 1 or 0.¹⁴

But there is more to it: Why should we give up and say *nothing at all* about something just because we can't tell *all* about it? As Lass (1997:272) says quite appropriately, our reconstruction "may not get us as far as we'd like, but it does get us somewhere, and that isn't a bad place to be." After all, science is about going as far as we can; going beyond that may be speculation (and science should indeed stop there), but not going as far is a grave mistake, too. No branch of science is almighty: we have our limits. Yet, no archeologist in his right mind would ever suggest that we should not reconstruct what we can of a ruined church or house just because we cannot reconstruct the exact structure of the roof. For science, it is imperative to define its limits; but it is also imperative to reach out as far as

¹³ "Anything at all" should be taken literally: if I say "this proto-whatever was a vowel", that is already a phonetic statement.

¹⁴ This is why concepts such as "heavy" or "long" are unscientific and are not defined in physics: how heavy must something be in order to be heavy? We can't give a principled answer.

the limits are. The idealist position, in my view, is unscientific (on two counts). And this I find a very strong counterargument.

3.2 Neogrammarians and Structuralists: what's the difference?

So far, I have dealt with two topics: first, I pointed out that IR is not a historical method and that it requires comparative backup (if available, of course); second, I argued that the idealist position in (not) assigning historical status to reconstructed entities is untenable. We are now in the following situation: CR is the only “real” historical reconstructive method, whose results are best interpreted phonetically (though not over-interpreted speculatively). In what follows I will consider CR as a historical method; then I compare the Neogrammarian and the Structuralist models as far as their reconstructive techniques are concerned.

Step 2 of (any) reconstructive technique, i.e., the assignment of some “thing” status to a set label, yields tree diagrams such as the one in (1). This is what we get either by synchronic analysis, IR, or CR. In each case, take a correspondence set, label it, and call the label an object, which “turns into” other objects. In synchronic analysis, the “object” is an underlying unit from which surface forms are derived by (ordered) rules; in IR and CR, it is a historical (past) object, from which attested (present) ones derive by chronologically ordered sound changes. The method, then, is no different, and we have seen this already in the case of IR and synchronic analysis. We can now see that CR is based on the same method. There is, however, a crucial difference: x, y, z are, for CR, from different languages. This is very important, because C, whatever it is, cannot be sensibly interpreted as a common “underlying representation”: the only sensible interpretation is genetic, i.e., historical: the relation “cognate of” is reinterpreted as “ancestor of”. Alternatively, we are left with the possibility of not interpreting it at all, or rather, not even calling it an object: this is basically the idealist position which has been found unacceptable. If we do not want to be idealists, we must interpret C as a historical object; as it is a historical object, from which physically existing present-day objects are derived, it must have some physical (phonetic) form. Note, however, a very important point: CR as a method does not start out as a historical method, either: the basic procedure is the same as for synchronic analysis or IR. What makes CR historical is the nature of the data it has to work with: that the data are from related languages, assumed to be related because they derive from a common ancestor: relatedness equals ultimate monogenesis. CR is not inherently historical: it is forced to be historical (if we want to avoid idealism). A synchronic alternation in itself does not force one to do a historical analysis. But if one does choose to do so, it is called IR. We can sum up the similarities and differences in a table, as in (9):

(9)

	SYNC. ANALYSIS	IR	CR
Set up correspondence?	Yes	Yes	Yes
The set's content	“Alternates with”	“Cognate of”	“Cognate of”
Interpretation	“Underlier of”	“Ancestor of”	“Ancestor of”
Phonetic content?	Yes (or No)	Yes	Yes

The position explained above, i.e., that of a full interpretation up to assigning phonetic content, is basically the position of the Neogrammarians, for whom the question of “idealism” would not have occurred. As opposed to them, the Structuralist school found it not only acceptable, but often desirable (of course, not necessarily everyone!). Of the two central reconstructive techniques, CR and IR, the former is said to be a Neogrammarian invention, while IR is considered to be a structuralist one. We have seen that the two methods are fundamentally the same in their procedures: it is the nature of the data they differ in. IR, then, is none other than CR applied to a different type of data, and as such, it does not constitute a principally new method.¹⁵

Furthermore, let us recall the Neogrammarian (comparative) reconstruction of the “back stop series” of PIE (cf. (4) above). Why are three different series reconstructed? After all, no daughter language has so many; each has two series only. The answer is, because there are three different correspondence sets. There’s no direct phonetic proof for the “palatal” series at all; the whole system is typologically suspicious (it is at least highly unusual for a language to have palatal *and* velar *and* labiovelar stops); moreover, the {K,K} correspondence is extremely rare. Why did the Neogrammarians reconstruct such a system? Because they applied the comparative formula in a rigorous manner. In this sense, they were more structuralist than the Structuralists themselves. It is highly enlightening to consider Meillet’s position on the PIE stop system: he (and many other linguists) represents the view that PIE had only two back series, *K^w and *K; the Centum group preserves both intact, while the Satem group palatalizes the velar (*K) series (which then ultimately turn up in the attested languages as sibilants), and de-labializes the labiovelar series. Meillet uses typological and phonological-historical arguments against the Neogrammarian position, pointing out that the paucity of {K,K} correspondences may be an indication of “deviant” developments. But there is something wrong here: Meillet considered himself to be an idealist, which means that he is rather inconsistent. He is, in fact, much less “structuralist” than the Neogrammarians, and, moreover, a realist in practice.

The two schools, then, do not really differ from each other so much as far as reconstruction goes. The main difference is in the interpretation of the results. We can now ask where the differences come from. The answer lies, as far as I can see, in the different conceptions of phonological change. Neogrammarians thought in terms of sound change; Structuralists concentrated on changes in the phoneme system. For them, a sound change in itself was not a linguistic change if it did not alter the phoneme system. For example, if in a language [u] is fronted to [y], which is a novel segment, nothing changes at all: the number of phonemes does not increase or decrease, only the phonetic realization of one phoneme alters. This, of course, is an abstract view that few if any phonologists would nowadays take.¹⁶ But it explains the origins of the idealist position: it is phonemic differences that count, so we must reconstruct phonemes (we cannot reconstruct allophones anyway,

¹⁵ Anttila (1989 : 229).

¹⁶ In fact, Structuralists were also interested in the phonetic content of oppositions, so I am being somewhat unfair; my excuse is that I use these *extreme* examples to refute an *extreme* position.

unless we have some special clue). Phonemes, however exactly one conceptualizes them, are abstractions. This is what makes the idealist position possible (but not obligatory: that is a further step). Neogrammarians dealt with sounds, hence idealism was impossible for them.

4 Conclusion

To sum up, the basic difference between Neogrammarians and Structuralists is that for Neogrammarians, all three steps of reconstruction are self-evidently compulsory; Structuralists can omit the last step(s). We have seen, however, that (although allowed by Structuralist theory) this is undesirable, and we had better be realists. As most of us indeed have been in practice, either admitting it or not. In other words, correspondence sets ought to be given ontological status, and interpreted phonetically.

As far as IR is concerned, it is but a historical interpretation of a non-historical method. This is not to deny its significance: there are many instances where comparative evidence is not available, either because the language has no (close) relatives or because we want to reconstruct earlier stages of a proto-language (as Saussure did). But the central core of reconstruction is still the comparative method: IR is the application of CR, a method devised to handle data from a historical perspective, to data which need not be interpreted historically. Furthermore, IR—generally associated with Structuralism—is not a Structuralist invention: the 1870's had seen several examples. Linguistics may have undergone revolutionary changes during the twentieth century, but in reconstructive techniques, we still use what our Neogrammarian predecessors invented. This note is not meant to devalue the work of twentieth-century historical linguists (including Structuralists—they have done an excellent job, especially in our understanding of language change) and to imply that we have no reason to be satisfied; but to turn the reader's attention to the extraordinary achievement of the old nineteenth-century masters.

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