

The Comparative Method

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1 Definition of the Comparative Method and its history

The Comparative Method (CM) is the systematic process of reconstructing the segmental and suprasegmental inventory of an ancestral language from cognate reflexes in the genetically related daughter languages. On the basis of the lexemes in which these reflex segments are embedded one may also reconstruct morphemes and lexemes. Since Schleicher (1852) it has been the key tool for investigating linguistic prehistory. Textual records for languages are in most parts of the world very recent, and even the earliest written records go back only a shallow 5,000 years or so. Thus if we were entirely dependent on written records little historical linguistics could be done on many languages and we would have no clear idea of the inter-relationship and prehistory of even relatively shallow language families like Slavic or Germanic. Fortunately, CM allows us to reach much greater time depths and reconstruct ancestral languages that existed thousands of years before the advent of writing.

2 The historical development of CM

Perhaps all people have stories about how the current diversity of languages came about, but many also tell a story about how different languages might be similar. The Salishan Sanpoil people recall how a bitter quarrel arose among the tribe about whether the noise ducks made on taking to flight was produced by their bills or their wings. Half the community favouring one theory went into exile and “finding new objects, and having to give such objects names, brought new words into their former language; and thus after many years the language was changed. Each split in the tribe made a new division and brought a new chief. Each migration brought different words and meanings. Thus the tribes slowly scattered; and thus the dialects, and even new languages, were formed” (Gould 1917: 111). In the fourth century CE Augustine of Hippo commented on the evident similarity between the Hebrew of the Bible and the Punic of his native North Africa (Weiss 2011: 487). But it was not until the seventeenth century that the first steps towards a systematic analysis of linguistic similarities were taken. The Dutch scholar Marcus Zuerius van Boxhorn (1612–1653) was one of the first to suggest that the lexical similarities observable among the languages of Europe resulted from a common origin in a language he called Scythian. The author of *De lingua vetustissima Europae*, 1686, whether Andreas Jäger or Georg Caspar Kirchmaier, was another noteworthy pioneer (Considine 2008). With increased knowledge of the ancient Indo-Iranian languages the comparison among the soon to be named Indo-European languages became ever more precise due to the pioneering efforts of Rasmus Rask (1787–1832), Jakob Grimm

(1785–1863) and Franz Bopp (1791–1867). The first to reconstruct items from the proto-language was August Schleicher, although others had recognised the possibility earlier (Koerner 1977).

3 Description of CM

The first step in applying CM is formulating a hypothesis that the given languages to be compared are in fact descended from a common source. It obviously makes little sense to apply the Comparative Method to languages that evidently aren't related – at any reasonable time depth – and the failure of the procedure to reveal any regularity of correspondence would be a strong argument against a theory of genetic common origin. Thus the Comparative Method, strictly speaking, is not a method for generating relationship hypotheses, but rather is a crucial tool for either confirming or not confirming such hypotheses. When the initial hypothesis of relatedness has been made, morphemes of identical or nearly identical meaning must be assembled for each of the subject languages. To the extent possible the lists must be purified of forms that are likely to be secondary. This includes forms that are thought to have been borrowed and forms that are the product of analogical remodellings. For example, if one were trying to reconstruct the Proto-West-Germanic root vowel for the plural of 'brother' one would compare archaic Mod. Eng. *brethren* with NHG *Brüder* and not the analogical *brothers*.

Once the lists have been assembled, the individual segments of the morphemes are compared between languages and grouped into correspondence sets. Correspondence sets may either be unique or partially overlapping. A unique correspondence set has no features in common with any other correspondence set. Unique correspondence sets are in practice relatively rare since they reflect either ultra-conservative situations (a proto-segment has undergone no conditioned sound changes at all) or an unconditioned sound change in one or more daughter language. The norm in fact is the partially overlapping correspondence set, i.e. two or more sets that are alike in one or more of their components. Those partially overlapping correspondence sets that occur in mutually exclusive environments are taken to be reflexes of one unitary proto-segment. For example, when one compares Latin and the closely related Italic language Oscan, one finds that Latin *s* corresponds to Oscan *s* (Latin *sunt* 'they are' : Oscan *sent* 'they are', but one also finds a partially overlapping set Latin *r* : Oscan *s* [z] (Latin *a*-stem gen. pl. *a:rum* : Oscan *-a:sum*). Since the set *s* : *s* never occurs between vowel and the set *r* : *s* always occurs between vowels, these sets may be merged and considered as reflexes of one original proto-segment, in this case **s*. This sorting of correspondence sets and merger, whenever possible, of partially overlapping sets is the essence of the Comparative Method or as Hoenigswald (1950) called it "the principal step in comparative grammar." As Hoenigswald noted, the reconstruction of a unitary proto-segment on the basis of partially overlapping sets is a diachronic counterpart of the classical phonemic analysis of allophones into phonemes. And this parallelism is not accidental since it is allophones and phonemes that are the basic units of Neogrammarian sound change. In Bloomfield's famous formulation "phonemes change" (Bloomfield 1933: 354). And indeed the regularity of sound change constitutes one of the best arguments for the role of phonology in addition to phonetics in sound change (Hale 2012).

In actual practice, however, reconstruction involves more than just an algebraic matching and merger of correspondence sets in complementary distribution. Most linguists, even those, like Meillet, philosophically committed to a formalist approach to reconstruction, are not willing to disregard the featural content of the reflexes. To do so would make judgements about what to actually reconstruct and what the histories of the individual daughter languages

were much harder. For example, imagine that segment *x* of Language 1 corresponds to a non-identical segment *y* of Language 2. This correspondence set is non-overlapping. Do we reconstruct **x*, **y*, or a third entity **z*. The purely formal approach cannot decide except to disfavour the reconstruction **z* on the grounds of Occam's razor. Both **x* and **y* are equally valid from the formal point of view. On the other hand if *x* is *s* and *y* is *h*, then it will immediately be the choice of linguists to reconstruct **s*. This is because the change from **s* to *h* is well documented in many languages (Ancient Greek, Proto-Iranian, Caribbean Spanish, etc.), and phonetically natural: anticipatory widening of the glottis to allow adequate airflow for the voiceless fricative leads to a period of breathiness on the preceding vowel. If the coronal gesture is reduced, the breathiness may be misinterpreted as *h* (Widdison 1997). A sound change in the opposite direction is not attested. Thus the optimal reconstruction seeks to maximise parsimony and naturalness.

4 Illustration of CM

For a first illustration of the Comparative Method it is a good idea to use a case where the results can be checked. In practice this means that we must examine a group of daughter languages with a recorded common quasi-ancestor, e.g. the Romance languages and Latin. Written Latin is only a quasi-ancestor because the written forms of Latin diverge in the direction of a higher register from the unrecorded spoken form, which is the true ancestor of the Romance languages. Let us consider the following lexical sets:

Sp.	Po.	Cat.	Occ.	OF	It.	Rom.	Sard.	Gloss
kampo	kāpu	kam	kamp	tʃamp	kampo	kɪmp	kampu	field
kanta	kanta	kantə	kanta	tʃantə	kanta	kɪntə	kanta	sings
θjelo	sɛw	sɛl	sɛl	tsjel	tʃɛlo	tʃɛr	kelu	heaven
seko	seku	sɛk	sek	sɛk	sek:o	sek	sik:u	dry
sako	saku	sak	sak	sak	sak:o	sak	sak:u	sack

Sp, Spanish; Po, Portuguese; Cat, Catalan; Occ, Occitan; OF, Old French; It, Italian; Rom, Romanian; Sard, Sardinian.

From these items we can extract the following correspondence sets:

- (1) Sp. *k*, Po. *k*, Cat. *k*, Occ. *k*, OF *tʃ*, It. *k*, Rom. *k*, Sard. *k*
- (2) Sp. *θ*, Po. *s*, Cat. *s*, Occ. *s*, OF. *ts*, It. *tʃ*, Rom. *tʃ*, Sard. *k*
- (3) Sp. *s*, Po. *s*, Cat. *s*, Occ. *s*, OF *s*, It. *s*, Rom. *s*, Sard. *s*

The first set partially overlaps with the second set (in Sard. *k*) and the second set overlaps with the third (in Po, Cat, Occ. *s*). Thus we are obligated to ask whether any of these sets can be merged. Set 1 and Set 2 are in complementary distribution with the first set occurring only before a non-front vowel and the latter occurring only before a front vowel. This means that Set 1 and Set 2 are to be merged into one proto-segment. Sets 2 and 3, on the other hand, occur in identical environments, i.e. before front vowels, and therefore cannot be merged. On the basis of the three sets we must set up two proto-segments. In the case of Set 3 the choice is obviously **s* since that is the sole reflex in all the daughter languages. In the merged set

1–2 the formal side of CM points to *k since that is the sole reflex shared by both sets, but in the absence of this piece of evidence – imagine Sardinian was not recorded – there would be no formal criterion for deciding what to reconstruct. At this point our knowledge of parallel sound changes and laboratory phonetics decides the question for us in favour of *k, because palatalisation of velars before front vowels is well established (occurring in Indo-Iranian, Slavic, Old English, etc.) whereas the backing and hardening of postalveolar or alveolar fricatives before non-front vowels is virtually unheard of.

Thus our method has led to the reconstruction of two protosegments *k and *s. *k was retained as such in Sardinian but in all other Romance languages *k was palatalised before a front vowel (or glide) with different outcomes in the different daughter languages. How does this stack up against the evidence of Classical Latin? Naturally – since I chose this example to demonstrate the validity of the Comparative Method – Classical Latin confirms this picture having exactly the segments reconstructed: *kampus* ‘field’, *kantat* ‘sings’, *kaelum* ‘heaven’, *sikkus* ‘dry’, *sakkus* ‘sack’.

This demonstration gives us an initial reason to have confidence in the validity and power of CM, but other proofs are available. If, for example, we know a certain number of cognates in an otherwise unknown daughter language, we may guess with a much greater than chance likelihood the form of other potential cognates (Hale 2012). An additional check on the validity of CM is provided by internal reconstruction. Internal reconstruction leverages the idea that non-suppletive allomorphy reflects the differential operation of sound change on an earlier non-allomorphic paradigm. If we compare forms like Lat. *erit* ‘will be’, and Skt. *a'sati* ‘be – 3SG.SUBJ’ we may suspect that an intervocalic *s has become an *r* in Latin. This hypothesis is abundantly confirmed by the presence of an allomorph *es-* of the root ‘to be’ in preconsonantal position, e.g. *es-t* ‘is’.

Let us consider a somewhat more complicated case where the evidence of reconstruction and the external check are not in perfect accord. Consider the following list of cognates:

Sp.	Po.	Cat.	Occ.	OF	It.	Rom.	Sard.	Gloss
abril	abril	əbril	abril	avril	aprile	—	abrile	April
ilo	fiu	fil	fil	fil	filo	fir	filu	thread
lepa	lepa	λεπα	lepo	lejpə	leppa	lemn	linnu	wood
mes	mes	mes	mes	mejs	mese	—	mese	month
sjerpe	serpe	serp	serp	serp-ent	serpe	jarpe	serp-ente	snake
jerba	erba	erbə	erbo	erbə	erba	jarbə	erba	grass

We will only consider the stressed vowel correspondence sets which are:¹

- (4) Sp. *i*, Po. *i*, Cat. *i*, Occ. *i*, OF *i*, Ital. *i*, Rom. *i*, Sard. *i*
- (5) Sp. *e*, Po. *e*, Cat. *e*, Occ. *e*, OF *ej*, It. *e*, Rom. *e*, Sard *i*
- (6) Sp. *e*, Po. *e*, Cat. *e*, Occ. *e*, OF *ej*, It. *e*, Rom. *e*, Sard. *e*
- (7) Sp. *je*, Po. *ε*, Cat. *ε*, Occ. *ε*, OF *ε*, It. *ε*, Rom. *ja*, Sard. *e*

For Set 4 it seems evident that the proto-segment should be *i and for Set 6 *e, but what about Sets 5 and 7? When we compare Sets 4, 5, and 6 we note that the reflexes in 5 overlap with those of 6 everywhere except Sardinian. On the other hand, the Sardinian reflex of 5 overlaps

with the Sardinian reflex of 4 (i.e. *i*) and the Sardinian reflex of 6 overlaps with the reflex of 7 (i.e. *e*). It seems that Set 5 would have to reflect a vowel intermediate in height between *i* and *e*, i.e. **i*. Set 7, which is realised as *ε* in Po., Cat., Occ., OF, and It., can be reconstructed as **ε*. Thus reconstruction leads to a front vowel system **i*, **i*, **e*, **ε*. This reconstruction correctly restores the total number of contrasts in this region of the vowel system, but it is unlikely that any stage of Latin had precisely this system of contrasts. Although Classical Latin did have two kinds of *i* and two kinds of *e* the distinction between the two sets was not height or tenseness but length. Our reconstructed **i* and **e* continue Classical Latin **i*: and **e*:. Reconstructed **i* and **ε*, on the other hand, continue **i* and **e*. To what extent can it be said that CM succeeded or failed in this case? Without the evidence of Sardinian we would have reconstructed **i* (Set 4), **e* (Sets 5 and 6) and **ε* (Set 7) and this would have corresponded to some real stage of Latin where the reflexes of Latin **i* and **e*: merged as **e*. The addition of the Sardinian information allows us to tease apart the two components of 5–6 but Sardinian has undergone its own ‘straight’ merger of long and short vowels and thus the crucial distinctive feature of the Classical Latin vowel system, length, has been eliminated everywhere. The application of CM with the maximal evidence available allows us to reconstruct the correct number of contrasts, but one cannot directly reconstruct what has been lost everywhere. In the absence of the evidence of Classical Latin there might have been some chance of speculating that the contrast had originally been one of length, but it would have been impossible to prove.

Cases like that represented by Set 5 above, where an originally contrasting segment has merged differently in different daughter languages and can only be identified by its distinctive correspondence set are wont to be controversial in the absence of an external check. One of the classic and still unresolved problems of Proto-Indo-European reconstruction is a case of this sort. Among the Indo-European languages there are two very well established correspondence sets involving dorsal stops (we consider only the sets involving the voiceless stops). These are as follows:

- (8) Lat. *k*, Gk. *k*, Go. *h*, Lith. *f*, OCS *s*, Aves. *s*, Skt. *c*
- (9) Lat. *kʷ*, Gk. *p*, Go. *hʷ*, Lith. *k*, OCS *k*, Aves. *k*, Skt. *k*

These two sets do not overlap at all (or only secondarily in ways not shown here) and are not in complementary distribution. Two distinct segments must obviously be reconstructed. For Set 9 the reconstruction of choice would be **kʷ*, a labialised velar, which straightforwardly accounts for the various reflexes. For Set 8 the exact reconstruction is not immediately obvious, perhaps either **k* with an unconditioned fronting in some languages or **kʲ* with a loss of secondary articulation in Latin, Greek, and Gothic. In addition to these two sets there is also a third set, which has fewer but a still significant number of representative reflexes:

- (10) Lat. *k*, Gk. *k*, Go. *h*, Lith. *k*, OCS *k*, Aves. *k*, Skt. *k*

In the first three daughter languages this set overlaps with the reflexes of Set 8, but in the next four languages it overlaps with Set (9). The first approach would be to try to unite this set with one of the partial overlapping sets, but this appears to be impossible, despite the continuing efforts of many notable linguists. With the new information provided by Set 10 it seems that the question of the reconstruction of Set 8 can only be resolved in one way: since **k* would be the most obvious choice for the ancestor of Set 10, it would follow that Set 8 cannot represent the same thing and hence the reconstruction **kʲ* would seem to be preferable

and indeed this is the standard choice of most Indo-European handbooks. But this choice is not unproblematic. If Set 8 really reflects $*k^j$, then Latin, Greek, and Gothic must have merged $*k^j$ with $*k$, but if this happened by backing or depalatalising $*k^j$ we would be positing a highly unusual sound change. Palatalised or front velars are rarely, if ever, known to become plain velars across the board. If the merger happened by $*k$ joining $*k^j$ then the absence of any further palatalisation in the daughter languages is very surprising. This difficulty makes it worth reconsidering the choice of $*k$ for Set 10. It seems counterintuitive to set up anything other than $*k$ since that is the almost unique reflex and the one exception, Gothic *h*, is a well understood further development of $*k$ by Grimm's Law. Nevertheless, suppose that Set 10 reflects $*q$. In that case we can again choose $*k$ for Set 8. This reconstruction has the advantage that the vectors of change are well paralleled. In rural Palestinian Arabic *q* is realised as *k* and *k* has become *tʃ* (Fischer and Jastrow 1980: 52). In Carrier (Athabaskan), the Proto-Athabaskan velars and uvulars (retained as such in Witsuwit'en) have become palatal affricates and velars respectively (Poser 2004). In New Caledonian, Proto-Oceanic uvulars become velars and Proto-Oceanic velars become palatal stops in initial position (Haudricourt and Ozanne-Rivierre 1982: 33). Thus from the formal point of view we can establish three proto-segments, but choosing the more plausible reconstruction is not easy. On the one hand, the traditional reconstruction has in its favour the fact that Set 10 is taken as directly reflecting the Proto-Indo-European segment with no featural change at all. But this simplicity comes at the cost of positing a difficult history for the reflexes of Set 8 which would have to have lost their palatalisation on at least four independent occasions: in Western Indo-European to account for Celtic, Italic, and Germanic, in Greek, in Non-Luvoid Anatolian, and in Tocharian. The uvular hypothesis is superior from the point of view of phonetic naturalness, but it suffers from the disadvantage that no daughter language preserves any trace of the uvular place of articulation. In the absence of new information neither the formal nor the phonetic aspects of CM can make a firm decision. Perhaps evidence about the distribution and phonological patterning of the proto-segments might be decisive, but this evidence would itself be the product of CM.

From those cases where a check is available we see that CM works, i.e. that it is based upon a fundamentally correct view of language change and differentiation. A test case like the Latin–Romance one or similar ones that could be constructed with, e.g., modern English dialects in comparison to Middle English, gives us confidence that the method should work within its built-in limits for cognate languages without a recorded quasi-ancestor.

In fact CM has been applied worldwide with great success. Bloomfield's application of CM to Algonquian (1925, 1946) is one of the most famous cases of a non-European success, but many others could be cited (Africa: Proto-Bantu [Meussen 1967]; Australia: Proto-Pama-Nyungan [Alpher 2004]; East Asia and Oceania: Proto-Tibeto-Burman [Matisoff 2003], Proto-Austronesian [Blust 2009; Wolff 2010]; Mesoamerica: Proto-Mayan [Campbell and Kaufman 1985]; South America: Proto-Cariban [Meira and Franchetto 2005]; Proto-Dravidian [Krishnamurti 2003; McAlpin 2003]). Thus there is no reason to believe that CM is applicable only when certain sociological preconditions are met (pace Dixon 1997). Bloomfield wrote in an oft-quoted footnote (1925: 130): “[a] principle such as the regularity of phonetic change is not part of the specific tradition handed on to each new speaker of a given language, but is either a universal trait of human speech or nothing at all, an error.” This statement naturally extends to CM as well.

5 Presuppositions of CM

5.1 Regularity

The crucial factor that distinguishes genetic relationships of the type amenable to comparative reconstruction from other types of similarities that may be observed when comparing languages is regularity of correspondence. This means: (a) that the reflexes that make up a correspondence set are invariant over all instances of the correspondence set; and (b) that the correspondence sets are normally confirmed by multiple instances. Let us examine the second claim first. If we compare languages known not to be related we may find individual cases of close phonetic and semantic matches, for example, Hawaiian *mahina* ‘month’ and Hindi *mahina*: ‘month’ or Scottish Gaelic *bò* /bo:/ ‘cow’ and Vietnamese *bò* /bo:/ ‘cow’. These are not, of course, evidence for previously unnoticed Hawaiio-Hindi or Gaelo-Vietnamese phyla but mere chance occurrences, and what makes this clear beyond any doubt is the fact that the putative correspondence set Haw. *m* = Hind. *m* or SGael. *b* = Viet. *b*. cannot be replicated. For example, Hawaiian *mahalo* ‘thanks’ does not resemble Hindi *d^hanjava:d* ‘thanks’ and Scottish Gaelic *boc* ‘he-goat’ looks nothing like Vietnamese *con dê* /kɔnɫ zɛɫ/ ‘goat’. If one wished perversely to pursue a Hawaiio-Hindi hypothesis, one could set up a new proto-phoneme for each ‘correspondence’ and nothing in the purely formal aspect of CM would exclude such a procedure. What makes such a hypothesis impossible is our linguistic knowledge that human languages do not have infinite or even 200 contrasting segments. Thus our genetic hypothesis quickly runs into trouble. A true correspondence set reflecting common origin should be replicable and this is for two reasons: first, correspondence sets exist because the daughter languages are reflecting in either changed or unchanged form multiple occurrences of the same segment of the proto-language. But, more importantly, when sound change occurs it obeys the principle of regularity, the so-called Neogrammarian hypothesis, i.e. a sound change affects all the eligible instances where the appropriate phonetic environment occurs. The reason why regularity exists is a question not discussed as often as one might think. The most plausible explanation is that regularity is the result of misacquisition in the first language acquisition process. The priming of misacquisition is phonetic ambiguity, but the generalisation over some subset of phonetic environments where the ambiguity may be greater or lesser and in any case not identical, which is the essence of regularity, reflects categorisation dependent on phonological knowledge. This is the only place where regularity in sound change is to be expected. That many acquirers should independently perform identical misparsings seems highly unlikely. So how is it then that the historical record and living languages do show so much evidence for regularity? The answer must be that in general what we are seeing is the successful acquisition or diffusion of misparsings originating with one or very few individuals. Thus although we expect perfect regularity at the micro level (the level of individual misacquisition, I-language) we may find something less than perfect regularity at the macro level, E-language). This residue of irregularity goes by the name of dialect mixture, intimate borrowing, or lexical diffusion.

Why is regularity crucial in practical terms? First of all it is the repeatability of sound correspondences that allows us to distinguish chance resemblance from genetic relationship. Most of the macro-families alleged by Greenberg and others do not rise above the level of statistical noise. See Matisoff (1990) and Campbell and Poser (2008) for details. Second, the regularity hypothesis allows us to distinguish between genetic relationship and convergence. If two languages show many similarities as the result of convergence rather than divergence from a common origin obviously the patent similarities will rise well above the level of

chance, but an examination of the evidence might reveal certain irregularities. These might result, for example, from words being borrowed at different stages in the historical phonologies of either or both of the two languages involved. Consider the following example. Mongolian once had $V_1\gamma V_2$ sequences, which are still preserved in Literary Mongolian. In most forms of Mongolian these sequences contracted to $V_{:2}$, e.g. LM *biteyü*, but Khalkha *bitü*, Buryat, Oirat *bütü*. In Tuvan, a language spoken in South Central Siberia, we find many forms similar to Mongolian forms including examples of the original sequence $V_1\gamma V_2$, but in these words the original sequence is reflected in two different ways. Sometimes the $V_1\gamma V_2$ is contracted to $V_{:2}$ as in the modern Mongolian languages (data from Khabtagaeva 2009):

- (11) Tuvan *büdü* ‘closed’ LM *biteyü*, Khalkha *bitü*, Buryat, Oirat *bütü*
 Tuvan *surgül* ‘school’ LM *surgayuli*, Khalkha *surgül*, Buryat *hurgüli*, Oirat *surgüli*

but in other examples the result is $V_{:1}$

- (12) Tuvan *elër* ‘steady’ LM *eleyür*, cf. Khalkha, Oirat *erül*, Buryat *elür*
 Tuvan *čalgā* ‘lazy’ LM *jalqayū*, cf. Khalkha *jalxū*, Buryat *zaxū*, Oirat *dzaxū*

If regularity were not the default assumption one might be liable to posit a historical rule which produced either result. But if we localise regularity in acquisition it is very difficult to understand how one acquirer could misparse identical sequences in opposite ways. In fact, we must be dealing with two historical strata. In the first stratum, the earlier Mongolian sequence $V_1\gamma V_2$ was resolved in favour of the quality of V_1 in Tuvan. The second stratum must represent recent loans from Mongolian after the Mongolian specific contraction of $V_1\gamma V_2 > V_{:2}$. In fact, since, as the absence of other regular correspondences shows, Tuvan is not a Mongolian language at all, but a Turkic language, the first stratum represents an early set of loanwords.

A second example shows the value of the regularity hypothesis as a heuristic tool. If we examine cognate sets taken from seven points (labelled 1 to 7 from west to east) for the Cornish English dialect we find the following partially overlapping correspondence sets (data from Wakelin 1975):

1	2	3	4	5	6	7	Examples
a	a	a	a	a	a	æ	adder, apple, matter
a	a	a	a	a	æ	æ	bat, cattle
a	a	a	a	æ	æ	æ	axle, bad, cabbage
æ	a	a	a	æ	æ	æ	badger, latch
a	a	æ	a	æ	æ	æ	thatch, catching
a	a	a	æ	a	a	æ	sack, saddle

Since the environments are not complementary there is no way to unite these six partially overlapping sets. But it is also impossible to set up six different proto-segments. For we may be fairly confident that no human language has or ever has had six distinct vowels in this narrow region of the vowel space. So is this perhaps a sound change that has not proceeded

on a segmental basis but on a word-by-word basis, captured at different points in its progression through the lexicon? No. It is clear that the distribution is not random but shows a distinct tendency for *æ* to be more common in the most easterly points and *a* to become more frequent as one moves west. In fact, the vowel in question in all of these forms goes back to Middle English *a*. The native dialectal reflex is *a*, but the Standard English reflex was *æ*, as it still is across the board in General American. Since the more eastern parts of Cornwall are in contact with other English dialects having *æ*, it is probable that the speech of these regions has replaced the native reflex with a form imported from the standard language (Wakelin 1975: 112–114). Since this importation is not a matter of misacquisition, but simply lexical replacement of the old form by the new, there is no reason to expect regularity. In many locales we find a messy mixture of native and standard forms. Compare the famously messy lexical distribution of tense *æ* in Philadelphia as discussed from different theoretical positions by Kiparsky (1995), Hale (2007: 130–133), and Labov (1994: 534–537; 2010: 351–352; on the incomplete diffusion of the distribution to the Philadelphia African-American community).

5.2 Directionality

Perhaps the one area where the power and precision of CM has most increased in the post-structuralist period is in the analysis of directionality in sound change. Fortunately for us, most sound changes are not equally common in both directions. Some are very strongly attested in one direction and virtually never in the opposite direction. The palatalisation of velars to post-alveolar or alveolar affricates or fricatives before front vowels or glides is the classic example of this sort. There are to my knowledge no cases of the reverse change at all. On the other hand, there are changes that show no strong directionality. For example, the change of dental *l* clusters to velar *l* clusters, e.g. *tl* > *kl*, is quite common, happening in the prehistory of Latin, and again in the prehistory of Romance, in Lithuanian, and in the Pskov dialect of Russian. But the opposite change, despite the standard view that it is not as common, occurs quite frequently as well, e.g. in some Northern English dialects (*tla:k* southwest Yorkshire for *clerk* [Wright 1905: 379]), in various German dialects, and in the Ladin dialect of Casamazzagno (Tagliavini 1998: 195). See Blevins and Grawunder (2009) for the phonetics of the change.

Traditionally, judgements about directional asymmetries were largely intuitive, based upon the sound changes that a particular scholar happened to remember or have access to, but with the advent of large-scale diachronic typologies such as Fallon (2002), Cser (2003), Kümmel (2007) we are no longer dependent solely on our personal database. Such information is particularly valuable where a scholar may have developed no intuition about the directional tendencies of unfamiliar sound classes. For example, some languages of the Anatolian branch of Indo-European have a velar or uvular fricative in post-vocalic coda position corresponding to vowel length in most other Indo-European languages. Many scholars have argued that this correspondence set reflects a Proto-Indo-European pharyngeal fricative (Beekes 1994). But the typological surveys of Simpson (2003) and Kümmel (2007) show that uvulars frequently become pharyngeals but pharyngeals don't often become uvulars. This means that the Hittite uvular or velar fricative is a strong argument against the common reconstruction of pharyngeals for Proto-Indo-European. Such typological surveys also highlight biases across language families. For example, in Australian languages a change from stop to fricative is rare, while a change from stop to glide is more common; frequencies which are reversed in

other language families (cf. Maddieson 2011). More diachronic surveys of these sorts are a desideratum.

The phoneticist approach of John Ohala and other scholars has contributed much to our understanding of why these asymmetries exist in the first place and have opened up interesting vistas for laboratory historical phonetics. Thus, for example, we find that perception studies show that subjects may misidentify a stimulus *ki* as *ti* 20 per cent of the time but never misidentify *ti* as *ki* (Chang, Plauché, Ohala 2001). This is a partial laboratory parallel to the change of the fronting of velars before front vowels. The truly common development to *tʃi* involves additional factors, but mishearing of *ki* as *tʃi* is also unidirectional (Guion 1998). But why does this asymmetry exist? The high F2 of a following high front vowel neutralises the formant transitions that are otherwise acoustic cues to the consonants' place and the spectra of the stop burst are similar too, but *k* has an additional feature that *t* does not: a compact mid-frequency spectral peak. In this environment this non-robust feature may easily be missed and the result is that *ti* is heard. If the mid-frequency spectral peak is edited out the edited *ki* is always perceived as *ti*. Chang *et al.* (2001) hypothesise that the reason the directional asymmetry exists is because hearers are more likely to fail to attend to a non-robust feature than to posit the existence of a non-existing cue. Similar directionality can be demonstrated in vision where subject readers are much more likely to misinterpret a Q as an O or an E as an F than vice versa because missing the distinctive bar is easier than 'seeing' it. Other factors are also undoubtedly involved in directional asymmetries including motor planning, aerodynamic constraints, and gestural mechanics (Garrett and Johnson 2013).

6 Universality of CM

If the regularity hypothesis is correct, then CM should in theory be universally applicable. Nevertheless, there are many factors that can make the application of CM challenging or virtually impossible. The most obvious limitation is the absence of comparative data. Language isolates are not uncommon and only fairly shallow linguistic prehistories can be discovered for them (see Campbell in press). To take the best known isolate, Basque does not have an extensive linguistic prehistory since we are limited to the evidence of internal reconstruction, and the examination of Latin loanwords.

There are also situations where a language does have evident relatives but the small number of cognates or very complicated phonological and morphological histories pose major problems to a successful application of CM. For example, Wolof is a member of the Northern Atlantic group spoken in Senegal and The Gambia, but shares no more than 25 per cent of its core lexicon with any other member (Sapir 1971; Segerer 2010). The chief reason for the absence of comparative material may be the antiquity of the split between Wolof and the other Northern Atlantic languages, but language-specific morphological innovation have also contributed to obscuring cognates. The ancestor of Wolof, like other languages in its group, once had a set of noun class markers prefixed to the noun, but in Wolof these have been incorporated into the nouns and classes are now marked by a postposed element. In some cases the initial consonant of the noun matches the initial consonant of the postposed element *su:f s-* 'earth', *we:r w-* 'moon'. This suggests that the noun-initial consonant results from the conflation of a classifying prefix and the pre-Wolof noun-initial consonant. When the present-day initial consonant is discounted the cognate forms in other Northern Atlantic languages are evident: for Wolof *su:f s-* 'earth', cf. Serer *lo:f* 'clay', Fula *lo:p-/lo:f* 'dirt' and for Wolof *we:r w-* 'moon', cf. Fula *lewru* 'moon' (Pozdniakov 1993). If, however, one relaxes the requirements for matching of the initial consonant in such a case for entirely

legitimate reasons, one must at the same time be aware that one is increasing the chances of picking up non-cognate lookalikes.

7 Limitations of CM

7.1 Complete loss

There have been since the late nineteenth century critics of CM. Some have found fault with the basic idea of regularity. Others have doubted how well the results of CM tally with whatever complicated historical reality lies behind a genetic relationship. The latter is of course a serious issue. As we have seen above, the match of Proto-Romance with Latin is far from perfect. Why is this? First of all, complete loss or merger cannot be recovered by CM. Comparison of the Romance languages does not permit the reconstruction of **h* because there is no evidence for a reflex of *h* in any of them and this contrasts with what we know of Classical Latin as written in the first century BCE. But in fact, this is not a failure of CM at all, since there are many reasons to think that spoken Latin even in the first century BCE did not have a segment *h* (Weiss 2011: 153). Thus the absence of *h* is an accurate representation of the proto-language. On the other hand, we could speak of a ‘failure’ or limitation of CM in a situation like the following. If we compare Gothic and Sanskrit we will find numerous examples of a correspondence *a ~ a*, e.g.:

- (13) Go. *akra-* ‘field’, Skt. *a'džra-* ‘field’
Go. *ahṭa:* ‘eight’, Skt. *aṣṭa:* ‘eight’

On the basis of these data one would be justified in reconstructing Proto-Indo-European **a*, but in fact the evidence of other languages reveals that two different segments have merged here:

- (14) Go. *akra-* ‘field’, Skt. *a'džra-* ‘field’, Gk. *agrós*, Lat. *ager*, ‘field’
Go. *ahṭa:* ‘eight’, Skt. *aṣṭa:* ‘eight’, Gk. *okto:* ‘eight’, Lat. *okto:*

The first set reflects **a* and the second **o*. Without this additional evidence a distinct segment **o* would not have been recoverable nor would it have been the case that the nearest common ancestor of Gothic and Sanskrit had already merged **a* and **o*. The merger of **a* and **o* happened independently in Proto-Germanic and Proto-Indo-Iranian since in the latter the merger was conditioned. In open syllables **o* merged not with **a* but with **a:*. There are no doubt many instances of this sort where the crucial evidence has been lost and we fail to reconstruct a contrast that has merged independently in the various branches.

Loanwords may also present complications for CM. Consider the case of the word for ‘coffee’ in Italian, Spanish, and Portuguese: It. *caffé*, Sp. *café*, Po. *café*. On the basis of this evidence alone it would be possible to reconstruct a Proto-Romance **kaf:e*, which would obey all the known sound correspondences and historical phonology deduced therefrom. One might have suspected that such a form was a borrowing into Proto-Romance on the basis of a number of unusual phonological and morphological features (geminate *f*, stem-final stressed *-e*) but it would be no different than other early loanwords incorporated into the Proto-Romance lexicon and correctly reconstructed from the daughter languages. When we add Fr. *café*, however, it becomes more difficult to maintain this reconstruction since a final short *e* would regularly have been deleted in French and the initial *k* before *a* would be expected to

become /ʃ/, cf. Late Latin *cappa* ‘hood’ > Fr. *chape* /ʃap/ ‘cloak’. Further evidence confirms that the coffee word is a relatively recent loanword from Turkish first into Italian and from there into the other languages mentioned (de Vaan 2008). It is entirely possible therefore that lexemes may be reconstructed through CM that in fact did not exist in the proto-language as long as they do not reveal themselves through irregular correspondences. This is not a problem for the reconstruction of phonological inventories since by definition the latent loanword obeys the regular correspondences, but it might present problems for morphological reconstruction and especially for any extra-linguistic inferences one might try to draw from the reconstructed lexicon.

Another oft-mentioned limitation of CM is that it reconstructs a system without variants, but we know that natural language always has variation. Thus it is said CM results in a fundamentally unrealistic picture of a proto-language. This criticism is partly misguided since the result of the Comparative Method is a proto-grammar, i.e. an I-language not an E-language. It is of course true that we cannot gain access to sociolinguistically conditioned variation through CM. In some favourable instances it might be possible to reconstruct morphological variants for a proto-language, for example, if two separate branches both preserve evidence for two isofunctional morphemes. A case of this sort may be seen in the genitive singular morphemes for the *o*-stem nominal class in Italo-Celtic where we can reconstruct both **-ī* (Latin *-ī*, Ogham Irish *-i*) and **-osjo* (Very Old Latin *-osio*, Lepontic [a Celtic language attested in Northern Italy] *-oisio*). Most scholars, however, would suspect that one morpheme (most likely **-ī*) is innovative in the *o*-stem genitive function and has spread beyond or entirely abandoned its original sphere of usage. Thus even reconstructable variation is input for further reconstruction.

7.2 Time depth

Some of the limitations mentioned above result from the degradation of information over time and this factor is what limits the depth of plausible reconstructions by CM. “One cannot reconstruct ad infinitum,” as Kuryłowicz (1964: 58) famously said. The reason for this dictum is not that a proto-language cannot itself be used as the basis for further reconstruction. In fact a proto-language can be a useful tool for investigating more ancient relationships. A proto-language is also a legitimate subject for the application of internal reconstruction. Rather, it is the fact that information loss over time eventually degrades the comparative material to such an extent that the remaining similarities are too few to be meaningfully distinguished from chance. Another aspect of the degradation issue may be the complete loss of related branches. Crucial evidence supporting a hypothesis of distant relatedness between two families may have been irretrievably lost with the extinction of other members of the family. For these reasons many linguists are pessimistic about the reconstruction of macro-families such as Nostratic, or even something more limited like Indo-Uralic. See Ringe and Eska (2013: 264–279).

7.3 Convergence

In a famous article published in 1939 Trubetzkoy argued that the data normally explained as the result of the divergence and differentiation of a common proto-language (Proto-Indo-European) could be equally well explained as the result of a convergence of several originally unrelated languages. This claim raises two questions: first, is Trubetzkoy’s hypothesis a valid alternative model for the Indo-European languages or any group of

languages? Second, can CM distinguish between the two types of similarity? There seems to be no theoretical reason why a ‘convergence family’ could not exist. Given enough time, it is conceivable that borrowing and structural convergence combined with overall degradation of information could lead to a situation where it would be impossible to distinguish the results of convergence from an ancient genetic family. The longstanding debate about Altaic may be a case of this sort where scholars still do not agree about whether there was a Proto-Altaic or whether the similarities between Turkic, Mongolian, Tungusic, Japanese, and Korean are the result of convergence. See Georg (2004) and Starostin (2005) for opposing points of view on the question of Altaic. But in more favourable instances it is possible to distinguish the two types of similarities. To begin first of all with phonology, in the convergence case lexemes and morphemes may in theory diffuse at any time throughout the *Sprachbund* area and may potentially derive from more than one source. Thus we may suspect that these items may show different states of phonological evolution or different initial phonologies. This would lead to multiple inconsistent correspondence sets. We would also expect the diffused items to show certain geographical patterns. For example, we might expect the greatest number of similarities to exist between contiguous languages and few cases of discontinuity where an item is found in the marginal languages but not in the central ones. We would also expect to find some semantic clustering correlating with distinctive sound patterns as is often the case in known instances of loanword strata. But most importantly we would not expect intricate morphological features to be robustly diffused. On all these criteria the Indo-European family of languages clearly does not result from convergence. For example, we find cases of lexemes and morphological features preserved only at the extreme margins like Tocharian B *spe* ‘nearby’ matching Latin *sub* ‘under’ < *supo, contrasting with *upo ‘under’ reflected by all other branches. A morpheme *-r marks the primary middle personal endings in Italic, Celtic, Anatolian, and Tocharian, but is not found in the other more central branches. It is difficult to construct a plausible convergence-based account of such distributions.

8 Evaluating reconstructed systems

The system reconstructed by CM, an entity bearing some relation to a prehistoric grammar, should be subject to whatever constraints I-language is universally subject to. But the evaluation of the typological plausibility of reconstructed systems has often been clouded by confusion between those constraints that are truly features of the computational component of the human genetic language endowment and those prevalent tendencies that result from the diachronic filter. A case in point would be final devoicing of obstruents. It is undeniable that final devoicing is a very widespread phenomenon, but is this because final devoicing is a realisation of some innate cognitive principle, e.g. the emergence of the unmarked in positions of neutralisation, or is it because there are multiple phonetic factors conspiring to prefer word-final devoicing? If the former is the case then the final *voicing* of obstruents would be excluded and any reconstructed language with such a feature (like Proto-Indo-European apparently where the evidence of Italic, Anatolian and Indic appears to point to the generalisation of voiced stops in word-final position) would require serious revision, for example by reinterpreting the apparent voice feature as something more phonetically plausible. On the other hand, if the explanation for the prevalence of final devoicing is phonetic bias, and if there are also possible diachronic pathways to final voicing, then the reconstruction of such an unusual phenomenon cannot be excluded.

Thus the neo-diachronic approach championed by Blevins (2004) and others has put the question of the typological evaluation of reconstructed systems in a new light. Some systems are undoubtedly excluded by universal principles of grammar or cognition, but the recourse to universal constraints to explain the absence or rarity of certain systems should not be automatic and diachronic explanations should not be ignored. This point of view in no way means that reconstructions should not be examined for typological plausibility. And given two possible reconstructions one would naturally prefer the one with the best typological support. In reality we often face reconstruction choices that are not as clear-cut. For example, the Proto-Indo-European short vowel system has four very solidly reconstructed vowels, *i, *e, *o, and *u, but the evidence for a fifth vowel *a is much scantier. If we eliminate *a then we have a vowel system with no low vowel, a type which is almost never found (Arapaho may be an example). This solution would eliminate a marginally attested segment at the cost of creating a typologically unusual system. The alternative approach recognises the presence of *a as a marginal segment. But is a marginal segment enough to make the system typologically acceptable? Most scholars follow this second approach, but in other very similar cases, e.g. whether to reconstruct a marginal voiceless aspirate series for Proto-Indo-European to remove the typological oddity of a voiced aspirate series with no corresponding voiceless aspirate series, they take the opposite view.

9 Mathematical/computational approaches

The formal aspect of CM is amenable to automation and scholars have experimented with CM algorithms (Kondrak 2009; Steiner, Stadler, Cysouw 2011). Large sound change databases should in the future put decisions about directionality on a firmer statistical footing (cf. chapters by Bowerman and Evans, Dunn, Garrett and Hamann in the current volume).

10 Beyond CM

There have been attempts to bypass CM in establishing language relationships. Most well known is Joseph Greenberg's Multilateral Comparison (2000), which essentially consists of examination of word lists for apparent formal-semantic matches across languages. Such a method may throw up some hypotheses to be tested but it is no substitute for CM. Most linguists do not accept the very large claims of its practitioners (Campbell and Poser 2008). On the other hand, more statistically sophisticated approaches to Multilateral Comparison using the Monte Carlo method can produce valuable results (Kessler and Lehtonen 2006). In this method word lists are randomised and compared. The results are then given a similarity score. These scores are then compared to the similarity score of the real correspondences. If the real similarity score is outside the random curve, this provides evidence for a greater-than-chance relationship (Ringe and Eska 2013: 276).

Another approach that can identify probable linguistic relationships is Johanna Nichols' 'individual-identifying threshold' approach (Nichols 2010). In essence the Nichols method is a quantification of the old idea that agreements in irregular morphological paradigms are the most powerful arguments for genetic relationship. Although these methods may well make it possible to prove genetic relationships that are very ancient, they do not replace CM, which still remains the primary tool for uncovering linguistic history.

11 CM in relation to other aspects of historical-linguistic investigation

11.1 Morphosyntactic reconstruction

CM strictly defined is limited to phonological reconstruction, but CM is also of vital importance for higher levels of linguistic structure. This is because the end result of a reconstruction is not merely a reconstruction of types, i.e. the contrastive segments of the proto-language, but also of the tokens of these segments, which are invariably embedded in morphemes and words. Thus CM also leads to the reconstruction of morphology and lexicon and, to the extent that syntactic relations are morphologically expressed, even syntax. Here is a simple example. The Old Germanic languages all attest a rather striking agreement phenomenon. When a masculine singular noun is conjoined with a feminine singular noun an adjective or pronominal form that modifies them is neuter plural. This agreement pattern survives today in Icelandic. From the perspective of the other Indo-European languages this agreement pattern is unique because the masculine is the unmarked gender and conjoined masculine and feminine nouns invariably take masculine agreement in Greek, Latin, Sanskrit, Old Church Slavonic, etc. Tempting though it may be to interpret this change of Proto-Germanic as an early attempt at gender-neutral language, what we know of early Germanic society makes such an explanation far-fetched. CM points to another more plausible explanation. The neuter plural ending of the predominant stem type can be reconstructed as Proto-Germanic **-o-*, but Proto-Germanic **-o-* is the result of a merger of Late Proto-Indo-European **-o-* and **-a-*. The neuter plural ending in fact continues Late Proto-Indo-European **-a-*, but given that we know the neuter plural ending was not used to modify mixed gender noun phrases in Proto-Indo-European, it is worth examining the possibility that the mixed-gender-agreeing form continues the other source of Proto-Germanic **-o-*, i.e. Late Proto-Indo-European **-o-*. When we consider this possibility it becomes clear that the mixed-gender-agreeing form derives from the old *masculine dual* form **-o-*. The masculine dual form would have been the expected agreeing form for a conjoined noun phrase made up of a masculine singular and a feminine singular since two conjoined singulars require dual agreement and masculine is the unmarked gender. As a result of the phonological collapse of **-o-* and **-a-* in **-o-*, and the withering of the dual category the old agreement pattern was interpreted in a new way. This reconstruction of the morphology and syntax depends crucially on a correct understanding of the historical phonology of Proto-Germanic, which in turn is a product of CM.

Morphological and syntactic reconstruction are also possible in ways that are quite independent of the principles of CM *stricto sensu* and will not be discussed here, but it is noteworthy that the principle governing choices of reconstruction in morphology is only partially similar to that governing phonology. Morphological reconstruction operates essentially on the *lectio difficilior* ‘more difficult reading’ principle, i.e. when choosing between two morphemes expressing the same function, one reconstructs the morpheme that cannot be explained in terms of the synchronic system. This principle establishes a kind of directionality, but it is not a directionality based in an external system (like phonetics in the case of sound change) but rather on the very structure in which the morphology is embedded (see Koch, this volume for further discussion). In syntactic reconstruction the question of whether directionality can be recognised and if so on what features of natural language it depends are open questions (see Barðdal, this volume). Semantic reconstruction is also a notoriously difficult subject and is treated in chapters by Urban and Deo. CM, however, plays a crucial role in semantic reconstruction. Once regular correspondences have been established

on the basis of perfect or very good semantic matches, one may then proceed to examine items illustrating formal correspondence but semantic divergence, items properly excluded from the initial application of CM. For example, Modern English *walk* is an exact match for Low German *walken* ‘to full (clothing)’. The original meaning, preserved in OE, ‘to roll, to toss about’, has developed differently in England and on the Continent.

11.2 CM and phylogeny

The establishment of a correct view of linguistic phylogeny is closely connected with the decisions that are made in applying CM. An incorrect reconstruction may potentially lead to an incorrect phylogeny and vice versa. For example, in the early days of Indo-European linguistics the tendency to regard the proto-language as essentially identical to Sanskrit encouraged scholars to reconstruct a three-vowel system (*i, *a, *u) for Proto-Indo-European. If such a system is taken as original then the five-vowel systems of Greek and Latin (*i, e, a, o, u*) must be regarded as an innovation. This would suggest a proto-Greco-Latin node in between Proto-Indo-European and the daughter languages. In fact, internal evidence of Sanskrit showed that this branch too once had the vowels *e and *o. With the correct reconstruction it became evident that Indic and Iranian, which shared the merger of the non-high vowels in *a and many other innovations, formed a legitimate sub-group, while Latin and Greek had nothing in common other than their ultimate Proto-Indo-European ancestry.

Conversely it is also the case that a branching established on other grounds may lead us to reconsider the historical phonology. For example, the morphological common innovations shown by the Italic and Celtic branches of Indo-European make it worth considering the possibility that parallel phonological developments, generally considered independent events, actually are to be dated to a common Italo-Celtic period (Weiss 2013).

Note

- 1 In polysyllabic words stress is assigned to the penult if it is heavy (i.e. contains a long vowel or has a moraic consonant in the coda) and to the antepenult if the penult is light. In the case of ‘April’ the *i* was long in Classical Latin and hence the stress fell on the syllable containing that vowel. Although length distinctions have been lost in Romance, the position of the stressed syllable is retained more or less faithfully.

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