GENERAL LINGUISTICS AND INDO-EUROPEAN RECONSTRUCTION

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There is good reason to be ambivalent about the usefulness of general considerations in linguistic reconstruction. As a heuristic device, a theoretical framework can certainly be helpful, but the negative potential of aprioristic considerations must not be underestimated. E.g., there is a whole range of phenomena which receive a natural explanation when we assume that glottalization is ancient in Germanic. The methodological question is: why have scholars been reluctant to identify the vestjysk stød with the English glottalization as a historical reality which may have been inherited from the proto-language? The role of general linguistics is to provide an idea of what can be expected in linguistic development, not by theoretical reasoning but by inspection of what actually happens.

Denmark has always been a superpower in linguistics. There is no need to list all famous scholars who worked in this country and left their imprint on the history of linguistics, but there are two names which I want to mention here, viz. Otto Jespersen and Holger Pedersen. The point is that we have a lesson to learn from these two great scholars in connection with the relation between general linguistics and Indo-European reconstruction.

Otto Jespersen was not only a great phonetician, but is regarded by some as the founder and by others as the forerunner of modern syntax. His *Philosophy of Grammar* is a classic for linguists of very different theoretical persuasions. The point I want to emphasize here, is that Jespersen was very well informed about the comparative linguistics of his time, and was therefore in a very good position to hold strong views about what his colleagues were and were not doing.

Holger Pedersen was perhaps the greatest comparative linguist of all time. But he also had a keen sense of scholarly atmosphere, as is evident from his history of 19th century linguistics. One of the characteristic features of his work is the insistence on comparison with what is actually observed in living languages, and on the role of naturalness in what is expected of linguistic development. He simply was a very good general linguist.

The fast-growing body of scholarly literature in the field of linguistics and the concomitant rise of specialization have led to a regrettable disintegration of the community of linguists. This is not to say that things were in all respects better in the past. Scholars were not always very nice to each other in former days, as can easily be gleaned from older issues of linguistic journals. There are many more jobs around nowadays. Yet I think that the discipline of linguistics has suffered from a fragmentation which could and should have been avoided.

It is clear that nobody can read more than a very small percentage of the total scholarly output in linguistics nowadays. This raises a fundamental question: how to choose what to read? The answer is simple: there is no general way to choose, because you never can tell where to find the unexpected clue. One can only try and look. It is therefore most important to have a general idea of what colleagues are doing elsewhere in the field.

A fair assessment of what general and comparative linguistics have to offer each other can only be reached if there is some consensus about the goals of the linguistic enterprise. The comparative linguist is in search of a picture which mirrors as closely as possible a historical reality, whereas the general linguist is primarily concerned with predicting the unknown. It is far from obvious that the former's reconstructions should conform to the latter's predictions. In the following I shall give a few examples of how these two lines of inquiry do not run parallel.

Perhaps the most common objection against a proposed reconstruction which is raised time and again on general grounds, is that a linguistic form is impossible because it does not conform to typological expectations. The classic example is Brugmann's reconstruction of *nasalis sonans* in 1876, e.g. in the first syllable of **kmtóm* 'hundred'. Brugmann published his article in a journal of which Curtius had made him co-editor before going on a journey. When the latter read the article after his return, he became so enraged that he dissolved the journal and started a new one, without Brugmann (cf. Pedersen 1962: 293). The new reconstruction has now been part of the *communis opinio* for over a century.

The case of the *nasalis sonans* is particularly instructive because the new theory soon gained general acceptance. The same cannot be said of the hypothesis that the Indo-European proto-language had no more than a single vowel. It is therefore important to compare the two cases in order to establish the reason for the different treatment. Note that I am not primarily concerned with the correctness of the reconstructions but with their reception by the scholarly community. If we can find out what motivates our colleagues to agree or to disagree, it may be possible to save a lot of time when trying to convince them.

There are two types of objection against the reconstruction of a single vowel for Proto-Indo-European. On the one hand, it is claimed that not all of the material can be explained from such a reconstruction. On the other hand, it is argued that there can be no such thing as a language with no more than one vowel. Both arguments have their counterparts in the reconstruction of the Proto-Indo-European syllabic resonants.

In the case of the *nasalis sonans*, there were two factors which rendered the new reconstruction more palatable. While the concept of syllabic nasal was an innovation, the syllabic liquids *l* and *r* were familiar from Czech and Sanskrit. The new theory did not therefore affect the idea of syllabicity as a vocalic property but only its distribution. Moreover, the class of possible reconstructed forms was not greatly affected because Brugmann recognized, beside the zero grade vocalism of the syllabic resonants, a reduced grade vocalism which could be invoked for those instances where others might see counter-evidence. It can be argued that the real victory of the *Sonantentheorie* was eventually achieved by the elimination of the reduced grade. That was a development which took much longer than the acceptance of the *nasalis sonans*.

The reconstruction of a single Proto-Indo-European vowel is a natural consequence of the laryngeal theory. It differs from the *Sonantentheorie* in two respects. Firstly, it strongly reduces the class of possible reconstructed forms. As a result of this much higher predictive power, it much more easily generates a class of counter-examples. Secondly, the way out which the reduced grade offered in the case of the *Sonantentheorie* is blocked by the fact we are now dealing with the vowel system itself. What is remarkable here is not that the new reconstruction of the vowel system met with a lot of opposition, which is only natural, but that it found any acceptance at all.

The far-reaching consequences inherent in the new reconstruction of the vowel system render the impact of the typological argument all the more important. It has been claimed that languages with less than two vowels are unattested or even impossible. This objection has been countered by the observation that there is a consensus among specialists of North-West Caucasian languages about the existence of minimal vowel systems, matched by extremely large consonant inventories, in that area. This shows the weakness of the typological approach: it causes a bias toward what is regular, normal, or frequent in the languages of the world and thereby renders the reconstruction of deviant patterns impossible (cf. Kuipers 1968: 78f.). The range of animal species living today would not allow us to reconstruct a dinosaur.

The typological argument against the reconstruction of a minimal vowel system for Proto-Indo-European is particularly regrettable because typological evidence could actually be used to support such a reconstruction. According to what probably is the majority view, the original homeland of the Indo-Europeans must be situated in the South Russian steppe. The non-Indo-European language family which is closest to that area is precisely the North-West Caucasian. If we start from the assumption that the Proto-Indo-European sound system resembled that of its neighbors, with which it may have formed a *Sprachbund*, the North-West Caucasian system is as close as we can get from a typological point of view. Moreover, we know that the area around Majkop, which is Circassian territory, was a cultural center in the formative years of the Indo-European proto-language. It is therefore easily conceivable that the Indo-European sound system originated as a result of strong Caucasian influence.

In fact, the typological argument is not only weak and ambiguous, but can even be harmful. It has long been recognized that cognate languages tend to develop along similar lines after the dissolution of their ancestor. The Romance languages of today resemble each other much more closely than any of them resembles Latin. As a consequence, the history of Indo-European reconstruction shows a gradual shift away from the principal languages (cf. Mayrhofer 1983). If Bopp's Indo-European resembled Sanskrit, and Brugmann's Indo-European resembled Sanskrit no more than Greek, and Cowgill's Indo-European resembled Sanskrit and Greek no more than Hittite, it is to be expected that future reconstructions will diverge more widely from our traditional image of what an Indo-European language should look like, and thus move farther away from our typological expectations.

What has just been said must not be taken as a plea against the use of typological evidence. On the contrary, I think that typological considerations are most useful as a heuristic device. They must never take the place of the evidence, however. In practice, the typological argument has too often served as a rationalization of traditional prejudice. Curtius' reaction to Brugmann's *nasalis sonans* is a case in point. I shall give two more examples of such unwarranted generalization about possible sound systems.

In Bella Coola, a Salish language, there are words consisting entirely of voiceless consonants, e.g. *t'x̄t* 'stone'. When Boas reported about this hitherto unknown phenomenon, his article is said to have been rejected by the editor of a journal because everyone knows that it is impossible to have words without vowels (cf. Hockett 1955: 57). On the basis of my own fieldwork I can testify to the existence of the same word structure in Heiltsuk, an unrelated, Wakashan language, which is also spoken on the Canadian Pacific coast, e.g. *qqs* 'eye'. Here again, it appears that the typological argument can indeed be harmful.

According to Jakobson's *Kindersprache, Aphasie und allgemeine Lautgesetze*, there can be no language without nasal consonants. The fundamental oppositions vowel – consonant and oral – nasal must be present everywhere: 'sie sind die einzigen die nirgends fehlen dürfen' (Jakobson 1941: 34). Unfortunately, the existence of consonant systems without nasals has been reliably reported for Quileute and for Duwamish and Snoqualmie, languages which are spoken in the state of Washington and which belong to two different language families (cf. Hockett 1955: 119). This counter-evidence subsequently led Jakobson to replace his 'universals' by 'near-universals', without, incidentally, mentioning the languages which forced his theoretical retreat (Jakobson 1962: 526). Here again, typological reasoning had an adverse effect on the progress of linguistics.

Since the universal character of the opposition between oral and nasal consonants has been disproved, we may wonder if the same can be done for the opposition between consonants and vowels. This has actually been achieved by Pulleyblank in his analysis of Mandarin Chinese, which is not a minor language. Pulleyblank treats all vowels as syllabic variants of glides with which they alternate (1984: 57). Since the vowels are derived by rules of syllabification, all morpheme structures consist of consonants only. In comparison with this analysis, the reconstruction of a single vowel for Proto-Indo-European looks rather conservative.

This raises the question whether our reluctance to admit certain possibilities may be a consequence of the tools we have been accustomed to use. In particular, is it possible that our conception of vowels and consonants is conditioned by our use of the Latin alphabet? Here it may be appropriate to have a look at the Japanese syllabary, which offers an instructive parallel.

Unlike the well-known Semitic and Indic scripts, the Japanese syllabary does not offer the possibility to denote a consonant without a following vowel. Consequently, it is impossible to describe the language in terms of stems ending in a consonant followed by suffixes beginning with a vowel. Thus, the paradigm of the verb ‘to speak’, indicative *hanasu*, infinitive *hanasi*, imperative *hanase*, subjunctive *hanasoo*, negative *hanasanai*, cannot be described as a consonantal stem *hanas-* followed by a variety of suffixes, but only as an alternating stem *hanasu*, *-si*, *-se*, *-so-*, *-sa-*, which may be followed by other syllabic elements. This is indeed what is done in traditional Japanese grammar, where the verb belongs to the so-called *godan-katuyoo*, or five-step conjugation.

The problem of notation as an obstacle to progress is not limited to linguistics, as any mathematician can testify. Consider the multiplication of 19 by 44. Accustomed as we are to the system of Arabic figures, we immediately see that 20 times 44 is 880, and when you subtract 44 you get 836, which is the correct answer. But now suppose that you live in Rome, two thousand years ago, then you have to multiply XIX by XLIV in order to arrive at DCCCXXXVI. There can be little doubt that this is a more cumbersome procedure.

Against this background, we may wonder if the difficulty of analyzing Japanese verbs with a consonantal stem in terms of the syllabary has a parallel in languages with an alphabetic script. In fact, it is not difficult to find examples. Take the English noun *house* and the verb *to house*. The latter is derived from the former by voicing the final consonant. It would therefore be appropriate to write the voicing feature as a suffix, if the writing system allowed us to do so. Conversely, the noun *use* is derived from the verb *to use* by devoicing the final consonant. Note that the direction of the derivation is different here: we can have a house without housing someone, but we cannot do any housing without a house; however, the use of something presupposes somebody using it, whereas we may use something without invoking the abstract notion of ‘use’. While a traditional analysis must treat these instances in terms of stem alternation, a generative analysis may postulate an underlying suffix. Such a suffix does not necessarily correspond to the suffix which a comparative linguist would reconstruct.

The example of *house* and *use* brings us to the problem of markedness. It is not always evident which of the two members of a pair must be considered the marked one. Since this may have consequences for linguistic reconstruction, it will be appropriate to look into the origin of the term. The concept of markedness as applied to the meaning of morphological categories can be traced back to Jakobson's essay *Zur Struktur des russischen Verbums*, which appeared in 1932. Jakobson's original example accompanying the introduction of the term is the Russian pair of words *telenok* 'calf' – *telka* 'heifer' (1932: 75). When he reprinted the article in his *Selected Writings*, Jakobson replaced these words by *osel* 'donkey' – *oslica* 'she-ass', without, incidentally, drawing the reader's attention to the fact that he changed his prime example (1971: 4). There are two remarkable things about this. Firstly, it turns out that the example was not very well chosen. It thus appears that the concept lacks the clarity which should render it applicable in an unambiguous way. Secondly, the ill-chosen example was evidently of no consequence to the theory because it was tacitly replaced by a different one. This does not inspire great confidence in the usefulness of the proposal. Since the *Urheber* apparently had a hard time making up his mind about the applicability of his theory to his own language, one can hardly blame others for avoiding the concept of markedness as a tool of analysis.

To summarize our findings thus far, it appears that there is good reason to be ambivalent about the usefulness of general considerations in linguistic reconstruction. As a heuristic device, a theoretical framework can certainly be helpful, but the negative potential of aprioristic considerations must not be underestimated. Since theory can easily embody the reflection of rationalized prejudice, it is important that comparative work be carried out inductively, as Holger Pedersen knew a long time ago. The accumulated experience of comparative linguistics offers a sound basis for a general theory of language change, which is part of a general theory of language.

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We may now examine the hypothesis that the traditional voiced stops of the Indo-European proto-language were actually glottalic. Before the advent of the laryngeal theory, it was generally assumed that the proto-language had the same four series of stops as we find in Sanskrit, e.g. the dental series *t*, *th*, *d*, *dh*. When it turned out that the voiceless aspirate was rare and must in a number of cases be derived from an earlier sequence of **t* plus a laryngeal consonant, the inductive generalization that no more than three series can be reconstructed for the proto-language left scholars with a typologically anomalous consonant system: *t*, *d*, *dh*. There are two ways out of this intuitively unsatisfactory situation. On the one hand, one may return to the traditional reconstruction of four series of obstruents, in spite of the fact that there is insufficient evidence for the existence of original voiceless aspirates. This possibility does not offer an explanation for the peculiar asymmetry in the attested material. On the other hand, one can try to reinterpret the three series of *t*, *d*, *dh* in such a way as to bring the reconstructed system into agreement with typological expectations. This research strategy invites scholars to look for additional evidence, which might change our views of the proto-language in a more radical way.

The first to pursue the latter possibility in print was Holger Pedersen, at the age of 84. Pedersen argued that there are no reliable Indo-European etymologies which point to an initial voiced labial stop **b-* (1951: 10-16). Since the voiceless labial stop *p-* is easily lost in a number of languages, he suggested that Proto-Indo-European **b* was originally voiceless and weak, while the traditional voiced aspirate **bh* may have developed from a voiceless aspirate. He compared the interchange of voiced and voiceless stops with the West Armenian consonant shift. The point to be noted here is the primacy of the empirical evidence. Typological considerations only served as a heuristic device for developing an explanatory hypothesis.

Pedersen's article inspired Martinet to propose two years later in a footnote that the Proto-Indo-European voiced stops could be derived from an earlier glottalic series without a labial member (1953: 70). He compared the absence of the labial with the same phenomenon in Proto-Semitic, for which he reconstructed a glottalic series as the origin of the so-called emphatic stops. Here again, typological considerations served as a heuristic device. The problem was posed by the unexpected absence of empirical evidence for the reconstruction of a labial stop.

A few years later, Andreev proposed an Indo-European proto-language without distinctive voicedness (1957: 7). He reconstructed voiceless fortes, voiceless lenes, and voiceless aspirates, corresponding to traditional *t*, *d*, *dh*, and suggested that this system is apparently preserved in Hittite. He introduced the incompatibility of fortes and aspirates in the root structure, which he (like Meillet) explained by an assimilation rule, into the discussion of the consonant system. His reinterpretation of the consonant shifts in the separate branches anticipates an argumentation which was put forward much later by the proponents of the glottalic theory.

A proposal which looks like an integrated view of the hypotheses put forward by Pedersen, Martinet, and Andreev, is Swadesh's theory that Proto-Indo-European and its neighbors had simple, glottalic, and aspirated stops, and that the difference between voiced and voiceless articulation was a matter of local variation (1971: 127). Since this theory was published posthumously, its origin is difficult to determine. Swadesh remarks that the traditional Indo-European voiced stops are equivalent to the glottalic series of other language families with respect to sound symbolism (1971: 219).

Twenty years after the publication of Martinet's suggestion that we may have to reconstruct glottalic stops for Proto-Indo-European, Gamkrelidze and Ivanov proposed the same (1972: 16), again on the basis of Pedersen's reasoning. Their proposal became much more widely known, probably because it was put forward time and again in different places. They explained the absence of roots with two glottalic stops by a dissimilation rule (1973: 153). They also reformulated Grassmann's *Hauchdissimilationsgesetz* as a Proto-Indo-European rule of allophonic variation (1980: 30-32). Here the primacy of the empirical evidence has been lost: the glottalic theory is not used to explain Grassmann's law, but Grassmann's law is adapted in order to serve as evidence for the glottalic theory. It seems to me that Latin *fīdō* 'I trust' < **bheidh-* suffices to show that the argument cannot be used.

Around the same time, a similar proposal was put forward by Hopper, who adduced not only the absence of **b* and the root structure constraints, but also the absence of glottalic stops from inflectional affixes (1973: 157). Here again, theoretical considerations evidently provided an obstacle to observation of the material, as is clear from the comparison of Latin *quod* with Old High German *hwaz* 'what', on the basis of which we have to reconstruct a Proto-Indo-European neuter ending **-d*.

On the basis of the proposals by Pedersen and Andreev, Rasmussen derived traditional *t*, *d*, *dh* from earlier *T*, *t*, *d*, where the first represents any emphatic stop, however phonetically realized: glottalized, pharyngealized, or just stronger (1974: 11). The same reconstruction is implied in Illič-Svityč's Nostratic dictionary (1971: 147). The problem with this hypothesis is that there is no reason to assume an emphatic or otherwise strong character for a glottalic series. There are many varieties of glottalization, some of them weak, others strong. The relatively weak character of glottalization in Georgian and Armenian is evident from the fact that we often find glottalic rather than aspirated stops in loanwords from Russian. This suggests that we have aspirated fortes and glottalic lenes in these languages. In Avar, a North-East Caucasian language, there is an opposition between tense and lax voiceless consonants which is independent of the opposition between plain and glottalic stops and affricates, e.g. *k*, *k:*, *k'*, *k:'*. Moreover, there is also an opposition between geminate and single tense consonants, so that we have e.g. *x̄ásel* 'winter' vs. *t'ás:a* 'from above' vs. *x̄ás:s:ab* 'special' (cf. Ebeling 1966: 63).

Thus, it appears that unwarranted generalization on the basis of theoretical considerations can easily interfere with observation of the facts and lead one astray in linguistic reconstruction. This can block scholarly progress for many years. Haudricourt reports (1975: 267) that as early as 1948 he arrived at the conclusion that the traditional voiced stops of the Indo-European proto-language were in fact glottalic and that the original pronunciation has been preserved in East Armenian. His argumentation was based on the types of phonetic development attested in the Far East. The negative attitude of Bloch and Kuryłowicz toward his view apparently kept him from publication. If Haudricourt, Pedersen, Martinet, Andreev and Swadesh had met at a conference in the late ’forties, the glottalic theory might have become popular a generation earlier than it actually did.

I conclude that the typological argument has too often been invoked as a constraint on linguistic reconstruction rather than as a device to broaden the horizon of possibilities. As a result, our reconstructions tend to have a strong bias toward the average language type known to the investigator. The more deviant the structure of the proto-language actually was, the stronger the bias and the larger the difference between reality and reconstruction we should expect. We must therefore first and foremost pay attention to the comparative evidence, which remains the ultimate basis for choosing between alternative options in linguistic reconstruction. It is remarkable that the comparative evidence has largely been left out of consideration in the discussion of the glottalic theory.

Glottalization is found in five out of the ten surviving branches of Indo-European, viz. Indic, Iranian, Armenian, Baltic, and Germanic. This is not the place to reconsider the comparative value of the evidence in the separate branches, which is very uneven (cf. Kortlandt 1985). My point is methodological: can we establish the circumstances under which certain facts are admitted as evidence for a reconstruction? The answer to this question is far from obvious.

There are two varieties of stød in Danish. As a rule, standard Danish stød appears in monosyllabic words which have pitch accent 1 in Swedish and Norwegian. Though the distribution of the stød has partly been obscured by analogical developments, it seems clear that it developed from a falling tone movement. I shall leave the standard Danish stød out of consideration in the following.

The so-called vestjysk stød is an entirely different phenomenon because it is characteristic of originally polysyllabic words, which have accent 2 in Swedish and Norwegian. It cannot possibly be connected with the Jylland apocope because it is also found in the northeastern part of vestfynsk dialects, where the apocope did not take place. While the vestjysk stød is clearly linked to a following plosive which represents an earlier voiceless stop, it does not represent original gemination because it distinguishes e.g. the verbs *dampe* [dam’b] ‘to steam’, *kante* [kan’d] ‘to border’ from the nouns *damp* [damb] ‘steam’, *kant* [kand] ‘edge’, which never had a geminate (cf. Ejskjær 1990: 64). As the glottalization in the infinitive *vente* [ven’d] ‘to wait’ is absent from the imperative *vent* [vend] ‘wait!’ (Ejskjær 1990: 65), it looks like a feature of the following stop which was lost in word-final position. This leads us to consider the possibility that it may reflect some kind of Proto-Germanic glottalization.

In his monograph on the vestjysk stød, Ringgaard concludes that ‘the v-stød is only found immediately before the plosives *p*, *t*, *k*, and that it is found wherever these stand in an original medial position, following a voiced sound in a stressed syllable. The exceptions to this are certain types of loan-words from a later period’ (1960: 195). He dates the rise of the vestjysk stød to the 12th century because it is characteristic of ‘all then existing medial plosives’ (1960: 199). The view that the vestjysk stød is a spontaneous innovation of the westernmost dialects of Danish, which Jespersen had in fact proposed almost half a century earlier already (1913: 23), can hardly be called an explanation. Moreover, it does not account for the vestjysk stød in the isolated pocket of dialects on the island of Fyn, which suggests that it is a retention rather than an innovation. The hypothesis of a local origin also neglects the parallel development of preaspiration in Icelandic.

Preaspiration is not only found in Icelandic, but also in Faroese, Norwegian, and the Gaelic dialects of Scotland. Phonetically, the preceding vowel is cut short and continued as a whisper, while a preceding resonant is partly or wholly unvoiced. The distribution of preaspiration in Icelandic is the same as in the Norwegian dialect of Jæren (cf. Oftedal 1947). We can therefore conclude that it is ‘an example of a feature taken to Iceland by the original settlers’ (Chapman 1962: 85).

Marstrander has argued that the preaspiration in Scottish Gaelic is due to a Norse substratum (1932: 298). He advances the hypothesis that the Norwegian preaspirated stops represent a retention of the clusters *hp*, *ht*, *hk*, which developed into geminates elsewhere (1932: 302). This theory implies three developments, viz. *ht* > *tt* in East Norse, *tt* > *ht* in West Norse, and *t* > *ht* in West Norse in those positions where the preaspirated stop does not reflect a cluster, e.g. Icelandic *epli* ‘apple’, *vatn* ‘water’, *mikla* ‘to increase’, *hjálpa* ‘to help’, *verk* ‘work’. Here the preaspirated plosives correspond to the traditional voiced stops of the Indo-European proto-language.

Both the vestjysk stød and the preaspiration receive a natural explanation if we assume that early Proto-Germanic possessed a series of preglottalized voiced stops *’b*, *’d*, *’g* (cf. Kortlandt 1985: 196, 1988: 8). Devoicing yielded a series of late Proto-Germanic sequences *’p*, *’t*, *’k*, the glottal stop of which was lost under various conditions. Then, weakening of the glottal stop in West Norse yielded preaspiration, while its assimilation to the following plosive gave rise to a series of geminates in East Norse, with the exception of Danish, where the sequences were subject to lenition and the glottal stop was preserved in the vestjysk dialects. It is difficult to escape the impression that the reluctance of earlier investigators to take the vestjysk stød and the Icelandic preaspiration seriously as comparative evidence in the reconstruction of Proto-Germanic deprived them of an insight which could have changed our view of Proto-Indo-European. What was the cause of their restraint? What kept them from regarding preglottalization and preaspiration as evidence on a par with other features? Was it the Latin alphabet which constrained their thinking?

Apart from the straightforward explanation of the vestjysk stød and the Icelandic preaspiration, the reconstruction of Proto-Germanic preglottalized stops has the advantage of accounting in a principled way for the existence of several layers of gemination, which can now be viewed as retentions rather than innovations (cf. Kortlandt 1988: 7). Firstly, it is possible that the unexplained gemination in Swedish, e.g. in *vecka* 'week', *droppe* 'drop', *skepp* 'ship', reflects a dialect which escaped an early loss of the glottal stop, in contrast with Old Norse *vika*, *dropi*, *skip*, Old English *wice*, *dropa*, *scip*. Secondly, *mp*, *nt*, *nk* yielded *pp*, *tt*, *kk* in the larger part of Scandinavia. This development becomes understandable if we assume that the nasal consonant was devoiced by the preaspiration of the following plosive and subsequently lost its nasal feature. Thirdly, **k* was geminated before **j* and **w*, e.g. in Old Norse *bekkr* 'brook', *røkkr* 'dark'. Similarly, **t* was geminated before **j* in a limited area, e.g. Swedish *sätta* 'to set'. (West Germanic geminated all consonants except *r* before **j* and is therefore inconclusive.) Fourthly, the stops *p*, *t*, *k* were geminated before *l* and *r* in West Germanic, e.g. English *apple*, *bitter*, cf. Gothic *baitrs*. The same development is found sporadically in Scandinavia, which suggests that we are dealing with the loss of an archaic feature rather than with an innovation. Here again, the geminate may have originated from the assimilation of a glottal stop to the following plosive.

In fact, the evidence for Proto-Germanic preglottalized stops is not limited to Scandinavian, but can also be found in English and German. It is common knowledge that standard English inserts a glottal stop before a tautosyllabic voiceless plosive, e.g. *sto'p*, *tha't*, *kno'ck*, *wa'tch*, also *lea'p*, *soa'k*, *hel'p*, *pin'ch* (cf. Brown 1977: 27). There is no reason to assume that this is a recent phenomenon. The High German sound shift yielded affricates and geminated fricatives, e.g. Old High German *pfad* 'path', *werpfan* 'to throw', *offan* 'open', *zunga* 'tongue', *salz* 'salt', *wazzar* 'water', *kind*, *chind* 'child', *trinkan*, *trinchan* 'to drink', *zeihhan* 'token'. These reflexes suggest a complex articulation for the Proto-Germanic voiceless plosives from which they developed. The origin of the gemination is unexplained in the traditional doctrine. If we start from the assumption that the Proto-Germanic plosives were preceded by a glottal stop which is preserved in the vestjysk stød and the English glottalization, the High German sound shift can be explained as a lenition of the plosives to fricatives with a concomitant *klusilspring* of the preceding glottal stop. Note that the High German sound shift has a perfect analogue in the English dialect of Liverpool, where we find e.g. [kx] in *can't*, *back* (Hughes and Trudgill 1987: 66), which again remains unexplained in the traditional doctrine.

Thus, it appears that there is a whole range of phenomena which receive a natural explanation when we assume that glottalization is ancient in Germanic. The methodological question is: why have scholars been reluctant to identify the vestjysk stød with the English glottalization, which according to Ringgaard gives the same auditory impression and apparently has the same articulation, as a historical reality which may have been inherited from the proto-language? Is there an implicit assumption that unwritten features must not be ancient? Is this the same factor which made Curtius reject Brugmann's *nasalis sonans*, in spite of the fact that we have a syllabic nasal in standard English words such as *button* and in the standard German infinitive ending of most verbs, e.g. *leiten* 'to lead', where both examples end in [tn]? Is it all the result of our Latin upbringing, which Jespersen blamed for our lack of insight into the grammar of modern English?

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It will be clear from what has been said that I am not particularly impressed by the contribution of theoretical reasoning to historical linguistics. Both Jespersen and Pedersen emphasized time and again that linguistics is an inductive enterprise, and I agree whole-heartedly. This does not mean that the comparative linguist can disregard what is going on in general linguistics, however. It rather means that we must look at those branches of linguistics which deal with language change in progress. Language is the interface between society and the individual, and sociolinguistics is the area of research where we can expect results which may be of immediate relevance to linguistic reconstruction. Rapid linguistic change in bilingual communities of nomadic traders and ethnically mixed groups offers a test-case for historical linguistics. There is no reason to assume that the sociolinguistic conditions of prehistoric linguistic development were very different from what can be observed today among comparable groups.

The remarkable spread of the Indo-European languages was determined by specific social and economic circumstances. It presupposes that a number of people moved from their original homeland to a new territory. As is now generally recognized, the domestication of the horse played a crucial role in the increase of physical mobility. However, the Indo-European expansions required not only the migration of Indo-Europeans, but also the adoption of Indo-European languages by local populations. This implies that a large number of people must have found it expedient to adopt the language of the intruders. As Mallory has pointed out, 'pastoral societies throughout the Eurasian steppe are typified by remarkable abilities to absorb disparate ethno-linguistic groups. Indo-European military institutions may have encouraged membership from local groups in the form of clientship which offered local populations greater advantages and social mobility' (1989: 261). This must have been the decisive factor in the spread of the Indo-European languages.

When we look at language interference in bilingual communities, it appears that there is a marked difference in the ease of linguistic borrowing between grammar and lexicon, between bound and free morphemes, and between verbs and nouns. As a result, the older strata of a language are better preserved in the grammatical system than in the lexical stock, better in morphology than in phonology or syntax, better in verb stems and pronouns than in nouns and numerals. The wide attestation of the Indo-European numerals must be attributed to the development of trade which accompanied the increased mobility of the Indo-Europeans at the time of their expansions. Numerals do not belong to the basic vocabulary of a neolithic culture, as is clear from their absence in Proto-Uralic and from the spread of Chinese numerals throughout East Asia.

The inequality between different parts of the language in linguistic borrowing is of particular importance when we are dealing with distant affinity. In a beautiful and convincing article which appeared a few years ago (1988), Michael Fortescue has demonstrated on the basis of case suffixes, pronouns and verbal morphology that Eskimo and Aleut are genetically related to Yukagir, which is most probably related to the Uralic language family. His reconstructions support the possibility that Tungus and Japanese also belong to the same language stock. It is clear that such affinity could never be demonstrated by the mere comparison of words.

In a study of the earliest contacts between the Indo-European and Uralic language families (1986), Rédei lists 64 words which were supposedly borrowed from Indo-European into Uralic at an early date. The material is divided into three groups: 7 Indo-European words which are attested in both Finno-Ugric and Samoyedic, 18 Indo-European or Indo-Iranian words which are attested in Finno-Ugric but not in Samoyedic, and 39 Indo-Iranian words which are not found either in Ugric or in Samoyedic. Now it turns out that the number of verbs in the oldest material is too large to support the hypothesis that they were borrowed: verbs constitute 43% of the first group, 28% of the second group, and 5% of the third group. This is strong evidence for the thesis that the oldest layer was in fact inherited from an Indo-Uralic proto-language. Though the material is very small, the case for an original genetic relationship is particularly strong because we are dealing with basic verbs, meaning ‘to give’, ‘to wash’, ‘to bring’, ‘to drive’, ‘to do’, ‘to lead’, ‘to take’ (cf. Kortlandt 1989). Moreover, it is difficult to see how Proto-Indo-European words could have been borrowed into Proto-Uralic if the Indo-Europeans lived in the South Russian steppe when the ancestors of the Finno-Ugrians and the Samoyeds lived on the eastern side of the Ural mountains. The earliest contacts between Indo-European and Uralic languages must probably be identified with the eastward expansion of the Indo-Iranians and the simultaneous spread of the Finno-Ugrians to the southwest.

Thus, it appears that we do not need a large number of obvious cognates, which cannot be expected in the case of distant linguistic affinity, in order to establish genetic relationship between languages. What we need to find are morphological correspondences and a few common items of basic vocabulary because these are the elements which are least likely to be borrowed. We can then try to match the linguistic evidence with what can be gathered from anthropological and archaeological sources. In my view, the last decade has brought decisive proof of genetic relationship between the whole range of languages from Indo-European to Eskimo. The next step should comprise an establishment of chronological layers in the material and a specification of the connections with the Altaic language family. The role of general linguistics in this enterprise is to provide an idea of what can be expected in linguistic development, not by theoretical reasoning but by inspection of what actually happens in situations of language contact. Language is a social phenomenon, and linguistic change must be examined in its social and historical context.

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[Cf. also 138, 142, 182, 203.]