

The comparative method in syntactic reconstruction

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Declaration

This dissertation is the result of my own work and includes nothing which is the outcome of work done in collaboration except where specifically indicated in the text.

Abstract

This thesis investigates the question of whether it is possible, or desirable, to use the comparative method as applied in phonological reconstruction to identify syntactic correspondences. I show that approaches proposed in the literature (e.g. by Lehmann 1974 or Harris & Campbell 1995) are problematic either because they do not follow the comparative method or because they do not do so in a principled enough fashion; objections raised in the literature (e.g. by Lightfoot 2002a) are then assessed, and I argue that most of these constitute no obstacle to syntactic reconstruction. I then sketch a method for applying the comparative method to syntactic reconstruction through comparison of the features of lexical items, including exponents of functional heads, applying an idea popular in current Minimalist thinking. This approach is then illustrated using examples drawn from the older Germanic languages: the Old Norse middle voice, the West Germanic inflected infinitive, and V-to-C movement. I suggest that pursuing an isomorphism between phonological and syntactic change has the potential to bear fruit in syntactic reconstruction, even if the parallels cannot be universally maintained.

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Introduction

The question of whether it is possible to reconstruct the syntax of protolanguages at all has long occupied both Indo-Europeanists (e.g. Brugmann 1904:624; Friedrich 1975:6; Clackson 2007:157-186) and theorists of syntactic change (e.g. Lightfoot 1979a:154-166 *et seq.*, Harris & Campbell 1995:344-376, Roberts 2007:357-368). The debate continues to grind on: a volume on the subject, edited by Gisella Ferraresi and Maria Goldbach, was published in late 2008. Its title is *Principles of Syntactic Reconstruction*, but the book reveals little consensus on any such principles, or even on a general methodology. None of the contributions adequately meets the challenge laid down by Lightfoot (2002b:625): ‘If somebody thinks that they can reconstruct grammars more successfully and in more widespread fashion, let them tell us their methods and show us their results. Then we’ll eat the pudding.’

The present contribution sketches a method for meeting Lightfoot’s challenge, illustrating it with a few case studies from the Germanic languages. My aim is to show that syntactic reconstruction, although more difficult than some of its proponents acknowledge, is possible within a method compatible with both modern syntactic theory and the comparative method as applied in phonological reconstruction.

The dissertation is structured as follows. Section 1 demonstrates the need for a new approach to syntactic reconstruction by showing that all previous approaches are either insufficiently powerful or suffer from methodological flaws, but also critically evaluates the potential obstacles to syntactic reconstruction mentioned in the literature, arguing that most of them are no more problematic in syntax than they are in phonology. Section 2 presents an overview of phonological reconstruction, with the emphasis on reaching an explicit definition of the comparative method such that it can be applied, *mutatis mutandis*, to syntax; I go on to present my approach and the theoretical considerations underpinning it. Section 3 is an application of the method to three case studies from Germanic. Section 4 concludes.

Considerations of space mean that the dissertation is deficient in a number of respects: for instance, the topic of directionality, which can fill whole journal issues (cf. *Language Sciences* 23), is barely touched upon. I hope to pursue these matters in more detail in future research.

1. Attitudes to syntactic reconstruction

A wide range of views are held by linguists as to the feasibility and profitability of the reconstruction of the syntax of protolanguages. At one end of the spectrum, Lightfoot (1979a; 1980; 2002a,b) has repeatedly asserted that syntactic reconstruction is impossible or impractical. Others (e.g. Lehmann 1974, Friedrich 1975) have written whole books attempting it. In this chapter I present a critical overview of what I consider to be the five main approaches to syntactic reconstruction advanced in the literature, in roughly chronological order (1.1). This is followed in 1.2 by a discussion of the key sceptical arguments put forward by critics such as Lightfoot. Section 1.3 concludes this literature review with a summary of the prospects for syntactic reconstruction given the state of the art.

1.1 Approaches to syntactic reconstruction

1.1.1 ‘Traditional’ reconstruction and syntax

As has been noted by many authors (e.g. Brugmann 1904:viii; Watkins 1964:1035; Clackson 2007:157), comparative linguistics as practised in the nineteenth and twentieth centuries has not always accorded syntax a central place: Beekes (1995), for instance, omits syntax entirely, and Meillet’s (1917) volume on the Germanic languages devotes a mere 6 pages to syntax out of 222, concluding with regard to Proto-Germanic word order that ‘l’ordre des mots était souple et n’avait pas de valeur grammaticale’ (1917:187).¹ Some work on syntax was done within this tradition, however, with the most frequently cited (and celebrated) example being Berthold Delbrück’s three-volume ‘Vergleichende Syntax der indogermanischen Sprachen’ (1893-1900). In the second half of the twentieth century, the work of Calvert Watkins (1964, 1976) can be considered a continuation of this tradition. I will focus on these two scholars as practitioners of syntactic reconstruction using traditional assumptions.

Although frequently described as the best available treatment of PIE syntax (e.g. by Lehmann 1974:3), Delbrück himself viewed his work as deficient in a number of respects (1893:vi): for example, he does not make reference to Armenian, Albanian or the Celtic languages (1893:88). Delbrück saw himself as attempting to use the same scientific method that had led to successes in lexical and phonological

¹ ‘Word order was flexible and had no grammatical value.’

reconstruction (1893:1), but he draws the reader's attention to some important differences in procedure. For example, he states that in syntax it is not appropriate to reconstruct forms for PIE as is done in phonology, and that therefore his work should be seen as 'eine in konvergierenden Linien ausgeführte Darstellung der ältesten Schichten der hier herangezogenen indogermanischen Sprachen' (1900:vi)² rather than as a presentation of the syntax of the protolanguage.

Watkins (1976:306) refers to 'the confirmation by Hittite of virtually every assertion about Indo-European word order patterns made by Berthold Delbrück', but few such assertions can be found, and those that can are rarely based on systematic comparison. Delbrück's most famous claim is that PIE was basically verb-final (1900:83), which he bases largely on evidence that in Sanskrit the verb is habitually final but occurs in initial position 'wenn es als wichtig hervorgehoben werden soll' (1900:81).³ This is later raised to the status of a universal 'Grundgesetz' (1900:110), which therefore dictates his analysis of PIE. He suggests that the formerly 'occasional' pattern has become 'habitual' in the verb-initial Celtic languages and Albanian, but he provides no explanation for why this should be the case and not the opposite; the archaic attestation of the oldest Sanskrit is assumed to speak for itself. Furthermore, his use of prosody as evidence can be called into question: Delbrück argues against Wackernagel's suggestion that the neutral position of the verb in PIE was second on the grounds that elements in this position had to be unstressed whereas the verb was only weakly stressed (1900:82). Here he uses intonation to make a point about word order. However, he uses word order itself to argue that intonation in Sanskrit subordinate clauses was the same as in main clauses (1900:84). Since the only evidence we have for non-standard intonation patterns or topicalisation in ancient languages such as Sanskrit is the word order, we cannot use these features to 'explain' word order in the way that Delbrück does without leaving ourselves open to the charge of circularity. Delbrück's distinction between 'habitual' and 'occasional' orders can also be challenged, as he provides no explicit definition of 'occasional'.

These criticisms of Delbrück's reconstruction are not intended to detract from the impressive nature of his achievement or to dispute the assertion that his work has yet to be surpassed. The point is solely to illustrate that the 'mastery of the important Indo-European dialects' (Lehmann 1974:1) achieved by Delbrück and his

² 'a presentation, laid out in converging lines, of the oldest stages of the Indo-European languages referred to.'

³ 'if it is intended to be emphasised as important'.

contemporaries should not lead us to accept their conclusions about protolanguages without question; rather, their reconstructive methods should be evaluated on their own merits.

The main contention made by Watkins is that ‘the “tractability” of the syntactic system for historical investigations is only in degree different from that of the phonological or the morphological’ (1964:1035). Most of his own work is based on the hypothesis that ‘recurrent similarities of syntactic patterning may result from genetic filiation’ (1964:1036). His 1964 paper aims to reconstruct certain patterns for PIE. Using only the elements N (sentence connectives), E (enclitic pronouns), P (preverbs) and V (finite verb forms), he suggests that from five of the older IE languages we can establish that PIE had the orders #V(E)...# and #.(E)...V#, with the latter being ‘normal’ or unmarked (1964:1041). In his 1976 paper he gives four examples of sentences about athletic contests, from Hittite, Vedic and early Greek, and concludes that ‘the syntactic agreements are so striking and so precise, that we have little choice but to assume the way you said that sort of thing in Indo-European could not have been very different’ (1976:315).

Some of the criticisms made of Delbrück’s work also apply to Watkins (1964), especially his distinction between marked and unmarked orders. No explicit statement of what it means to be ‘marked’, or of what triggers the ‘marked’ order, is given, making the notion essentially vacuous. Watkins’s interpretation of the evidence can also be called into question: he states that for Hittite ‘the normal position of V is sentence-final, but initial position exists here also as a stylistically marked variant’ (1964:1038) without explaining how he reaches this conclusion, and treats the other four languages in his sample equally briefly.

With regard to Watkins (1976), Lightfoot (2002a) accepts the claim that striking similarities may signify genetic relatedness, but argues that ‘problems arise when the most archaic patterns in the daughter languages are not alike’, and that therefore ‘one can reconstruct syntactic patterns [only] where the daughter languages show identity’ (2002a:120). It is indeed difficult to see how Watkins’s (or Delbrück’s) methods could deal with differences between the languages under comparison.

To sum up, ‘traditional’ syntactic reconstruction as practised by Delbrück and Watkins has led to some plausible results. However, these are few, and generally reliable only where strong similarity or identity can be found. This is a crucial departure from the phonological comparative method, in which systematic

correspondences, not structural similarity, are diagnostic for cognacy. This discrepancy may explain why the reconstruction of syntax lagged behind that of phonology in traditional scholarship: the methods used do not provide a firm basis for far-reaching inferences about the syntactic structure of protolanguages.

1.1.2 Typological syntactic reconstruction

In contrast to the above, the typological tradition of syntactic reconstruction as practised in the 1970s targeted exactly such far-reaching generalisations. Of the frameworks discussed in this section, this one has been most widely applied to actual reconstruction: Lehmann's (1974) 'Proto-Indo-European Syntax' is the only book-length treatment of the syntax of PIE to date (Clackson 2007:185). Although some of the results of this method may be correct, the method itself is unsound and has been extensively criticised since the 1980s (e.g. by Lightfoot 1980, Smith 1981, and McMahon 1994). In this section I will discuss the origins of the approach and how it is applied by Lehmann (1974) and Friedrich (1975), as well as considering the utility of a typological approach such as that advocated more recently by von Mengden (2008).

The study of syntactic typology dates back to Greenberg (1963), who presented a number of typological universals of word order based on a small sample of languages. Although these were primarily observational, he did tentatively posit a notion of 'harmony' in which VO order was harmonic with VS, NGen, NA and prepositions while OV was harmonic with the opposite orderings (1963:98-100). This work inspired a rich field of investigation, spearheaded by Lehmann (1973), who elevated Greenberg's notion of harmony to the status of a 'fundamental principle' of placement of modifiers (1973:48), and Vennemann (1974). Both these authors assume that typological 'inconsistency' with regard to this principle is a sign that a language is undergoing change from one type to the other (e.g. Lehmann 1973:55). Problematically, this assumption requires consistency as a force to be weak enough to allow an initial violation for whatever reason and yet strong enough to reassert itself once this has happened (Hawkins 1979:641). Lehmann and Vennemann have also been criticised for insisting on the rigidity of universals that Greenberg originally posited cautiously as statistical: Watkins (1976:306) accuses them of turning these universals into 'an intellectual straitjacket'.

Lehmann (1974) attempts to use these typological principles to reconstruct the syntax of PIE. Many of his conclusions are in line with Delbrück's, e.g. that PIE

was basically verb-final (1974:31). The OV structure of PIE is simultaneously the book's main conclusion and its main assumption: although Lehmann denies that he assumes this (1974:26), the third chapter of the book states that it aims to 'demonstrate how the nominal modifying constructions of PIE can be accounted for by assuming that it was an OV language' (1974:61). The evidence is treated in a similarly circular way. Examples of standard-pivot-comparative and of comparative-pivot-standard orders from Sanskrit are given (1974:31); Lehmann interprets the former as unmarked, as it correlates with OV in his theory, and the latter as innovative; he then claims that this is evidence for the OV structure of PIE. Lehmann also makes some assertions about typological correlations that are demonstrably false. He considers that a middle inflection characterised by verbal suffixes correlates with OV (1974:19), yet Old Norse, which if anything was in transition to VO, developed a middle inflection from reflexive pronouns (Faarlund 2004:123-7). Likewise, he asserts that 'sentence connectives seem to be characteristic of VSO languages' (1974:215). He is therefore forced to explain the ubiquity of the connectives *nu*, *šu* and *ta* in the largely OV Hittite through contact with Akkadian, and *sá* and *tád* in largely OV Vedic through 'outside influence' (1974:215), making no mention of the extremely common connective *jah* in largely OV Gothic. For neither of these correlations does Lehmann provide references or cross-linguistic evidence.

The majority of Lehmann's examples (53 out of 90 in his chapter on the syntax of simple sentences) are drawn from Vedic Sanskrit, for which he is criticised by Friedrich (1975:5-7). Some languages, such as Tocharian, Albanian, Armenian, Baltic, Celtic and Slavic, are not mentioned at all. Lehmann also goes further than the previously mentioned authors in ignoring 'marked' structures: Watkins (1976:316) accuses him of implying that textual materials exhibiting marked order should be excluded from consideration when he states that 'we must be careful in our choice of typical material when carrying out linguistic analysis' (1974:21). Due to these methodological shortcomings, then, Lehmann (1974) cannot be relied upon as a description of the syntax of PIE, and the evidence he adduces in support of his claim that PIE was SOV is certainly no stronger than Delbrück's.

Along with Lehmann's (1974) proposal of SOV and Miller's (1975) proposal of VSO, Friedrich (1975) is most frequently cited for claiming that PIE was SVO (e.g. by Watkins 1976 and Lightfoot 1980). However, this claim must be seen in the light of Friedrich's aim: to cast Lehmann's position and methodology into doubt by advocating an alternative basic ordering equally compatible with the data. Like

Watkins (1976), who writes derisively of the typologists' focus on 'the magic letters S, V and O' (1976:305), Friedrich was critical of the notion that word order could be reduced to VO/OV, arguing that 'the network of variables ... cannot be mapped onto a two-dimensional space' (1975:3). For instance, he questions the idea (accepted by Lehmann) that AN order correlates with OV: 'clearly, the ordering of adjectival dyads and their harmony vis-a-vis the dominant system differs significantly from the harmony relations between other variables' (1975:fn5). It has subsequently been established that no such correlation can be found across the world's languages (e.g. Dryer 1992:95).

Friedrich's suggestion that SVO was the basic order of PIE is made extremely tentatively: PIE 'could have been a loose type II [SVO - GW] - although a weak type III [SOV] and even some sort of type I [VSO] cannot be precluded either' (1975:66). Furthermore, he claims that he has 'stated the antithetical VO hypothesis as part of a more general hypothesis regarding the indeterminacy of the evidence' (1975:69). In any case, the influence of Ross (1970), which Friedrich refers to as 'deservedly celebrated', should be taken into account: Ross proposed, on the basis of evidence from gapping, that a language can only be verb-final in deep structure if it contains no leftward verb-movement rules, and that PIE was therefore underlyingly SVO (1970:258). This proposal can be seen as an antecedent of the antisymmetry theory proposed by Kayne (1994); in Kayne's approach, SVO order is basic to human language and non-SVO orders must be derived by movement, as they must in many cases in Ross's approach. Friedrich's proposal that PIE was *underlyingly* SVO is therefore different from the SOV proposal of Lehmann, for whom deep and surface structure were less divergent.

Despite these mitigating factors, Friedrich's study still suffers from a number of the same problems as Lehmann's. Although he justly criticises Lehmann (and Delbrück) for putting too much emphasis on Vedic Sanskrit (1975:5), Friedrich's whole study is based on Homeric Greek. Furthermore, although finding fault with the way Lehmann applied the typological method, Friedrich continues to use the method itself. Although he questions the validity of the AN ~ OV correlation, he continues to use it as a diagnostic for word order type (1975:11-13). Also, like Lehmann, Friedrich is quick to label data that does not fit with consistent patterns as 'marked', without explicitly defining the notion: for example, he notes that the speech of Achilles in Homeric Greek is inconsistent with typological predictions, and puts this down to Achilles's 'aberrant personality' (1975:12-13).

It is clear, then, that typological syntactic reconstruction as practised in the 1970s suffered from a number of methodological flaws. Despite the reservations expressed about Lehmann and Friedrich's methods, however, it is clearly not the case that typology has nothing to offer reconstruction. Typology is frequently used as a heuristic in phonological reconstruction: for example, Jakobson's (1958) observation that the traditionally reconstructed phoneme inventory of PIE, with voiced aspirated but not voiceless aspirated stops, was unattested in living languages has caused the nature of the PIE system to be reconsidered. Underlying this is a 'uniformitarian' or 'actualist' approach to history, borrowed into linguistics from geology (Lass 1997:25-8): 'Nothing that is now impossible *in principle* was ever the case in the past'. Following this, we should be wary of reconstructing states of affairs that are totally unattested in the languages of the world today. As Jakobson (1958:528-9) puts it, 'a conflict between the reconstructed state of a language and the general laws which typology reveals makes the reconstruction questionable'. It is undoubtedly true that some synchronic implicational universals can be established in the field of syntax. For example, if a language has prepositions, then if the adjective follows the noun then relative clauses will follow the noun (Hawkins 1983:74). Where such syntactic universals can be established, it is reasonable to apply them to reconstructions. This principle was recognised as early as Dressler (1971:7). It is also the approach advocated by Ferraresi & Goldbach (2008:13), although their discussion is couched in terms of (macro)parameters: if an implicational relationship between grammatical phenomena A and B is identified such that $A \supset B$, 'we could predict - should we identify phenomenon A in the historical texts from a dead language, but find no evidence of B - that B must have been present as a grammatical phenomenon at the corresponding linguistic stage. Syntactic reconstruction could be carried out this way.' Von Mengden (2008) is also optimistic about the possibilities offered by this method.

This form of typological approach is not the solution to all our problems, however. As can be seen from the quotation above, Ferraresi & Goldbach's method requires historical texts in order to identify one half of an implicational universal to carry out 'reconstruction'. But this postulation of unattested forms in a language for which we have independent evidence - essentially filling in the gaps in a synchronic grammar - is not what is usually referred to as reconstruction; in a language such as PIE or Proto-Germanic, for which no texts exist, we have (by definition) no evidence for either A or B, and so no starting point for this method. At the very least we would need a reconstruction reached in some other way, of the kind that is provided for phonology by the comparative method, before we could begin to make inferences

from implicational universals. Von Mengden (2008:109) acknowledges this major drawback to his method.

Using typology alone, then, is not a sound or fruitful way to approach syntactic reconstruction. The benefits of syntactic typology in reconstruction could potentially be enormous, but in order to reap them we need a starting point; in other words, we need another method of rolling back linguistic history.

1.1.3 Grammaticalisation and palaeosyntax

One contender for such a method is provided by the work of Givón (1971; 1999), building on his aphorism that ‘today’s morphology is yesterday’s syntax’ (1971:413). The key idea is that the synchronic morphology of a language may provide clues as to the history of its syntax, on the basis that independent syntactic items become fossilised as morphology without changing in position. Givón (1999) and the papers in Gildea (1999) are examples of attempted reconstruction using principles of grammaticalisation. Unlike in the typological paradigm, relatively little actual reconstruction has been carried out in this way, although see Balles (2008) for discussion of a few Indo-European examples.

Grammaticalisation has become a controversial subject within the last decade (cf. Newmeyer 1998:ch.5; Campbell 2001). However, the controversy is largely centred on matters that are tangential to its application in reconstruction, namely whether the notion is explanatory, whether it has an independent status as a type of change, whether there can be such a thing as a grammaticalisation ‘research framework’ (Hopper & Traugott 2003:1), and whether the claim of unidirectionality (Hopper & Traugott 2003:16-17) is justified. Only the latter has any bearing on reconstruction, but regardless of the meaning and status of unidirectionality, ‘even those who are critical of grammaticalization recognize that instances of change from less grammatical to more grammatical are the expected norm and far outnumber changes in the opposite direction’ (Campbell 2001:133). Directionality of syntactic change is a fact, and ‘grammaticalization is a real phenomenon’ (Lightfoot 2006:177). If we accept Dressler’s view of reconstruction as a ‘Wahrscheinlichkeitsschätzung’⁴ (1971:6), as I will suggest in section 2 of this dissertation that we must, rather than as a matter of mechanical certainty, then a minority of counterexamples to the prevailing tendency should not concern us much when carrying out reconstruction.

⁴ ‘estimation of probability’.

Grammaticalisation, then, is a powerful tool in any reconstructive toolbox for syntax. Nevertheless, some cautionary remarks must be made. First, hypothesising that a morpheme once existed in a language as an independent syntactic item does not provide us with any clue to *when* that might have been the case, as emphasised by Lightfoot (1980:38-9): ‘each of the individual forms reconstructed may be accurate, but there is no reason to suppose that they all reflect the same earlier stage - they may each reflect the syntax of 500, 1000, or 2000 years ago’. This contrasts with the results of the phonological comparative method, in which all reconstructed forms are by definition dated to the point of the split of the individual languages, since changes prior to this time cannot be reconstructed by comparison. Givón (1999) reconstructs features of historical Tolowa syntax based on the language’s present day morphology, but we cannot tell from his exposition or methods whether his results reflect the state of affairs in earlier Tolowa, in Proto-Athabaskan or in Proto-World. In grammaticalisation as in diachronic linguistics more generally, ‘particular changes do not have to occur, nor do they have to go through to completion’ (Hopper & Traugott 2003:39). Impressive though hypotheses like Givón’s may be, internal reconstruction based on grammaticalisation is not enough alone to make inferences about protolanguages: it is necessary to embed such hypotheses within a comparative perspective. This is the angle taken by Vincent (1980), who compares the Italian noun *casa* and the Spanish noun *casa* with the French noun *chez* and argues that the latter must be the innovation on the basis of a common grammaticalisation chain (1980:60-61; cf. also Longobardi 2003).⁵

Second, grammaticalisation is just one type of change: other morphosyntactic changes (e.g. lexicalisation, and other kinds of reanalysis) exist, and it is not always possible to establish directionality in these changes. Word order is a prime example. Like typology, then, grammaticalisation has the potential to be highly useful in reconstruction, but to apply it in a meaningful way we must first have a comparative framework within which to work.

1.1.4 Harris & Campbell

The most vocal proponents of syntactic reconstruction in recent years, responding to the scepticism of critics such as Lightfoot (1979a, 2002a) and Winter (1984), have

⁵ Of course, there is a danger of circularity if the transition from a reconstructed to an attested form is used as evidence for the grammaticalisation chain itself; evidence for such chains should therefore always be taken from languages with attested histories.

been Alice Harris and Lyle Campbell (Harris & Campbell 1995:ch.12; Campbell & Harris 2002; cf. also Harris 2008).

Harris and Campbell argue that ‘it is both possible and appropriate to use the methods of comparative and internal reconstruction to reconstruct syntax’ (1995:344). In addition, they suggest, as I have above, that both synchronic typology and diachronic directionality are helpful tools in reconstruction (1995:361-7). Their main contribution, however, is to argue that correspondences can be established in syntax in much the same way as they can in phonology. While I am sympathetic to this general approach (cf. section 2 of this dissertation), I nevertheless feel that their method as presented is too unconstrained and ill-defined to serve as a firm basis for syntactic reconstruction. In this section I will briefly present their method and then critically discuss it.

In contrast to Campbell & Mithun (1980:19-20), who assert that syntactic change has ‘no direct analogue’ of the regularity of sound change (cf. also Campbell 1990), Harris & Campbell (1995) argue that ‘syntactic change is indeed regular, in the sense that it is rule-governed, non-random’ (1995:347). For Harris & Campbell, change is established in syntactic patterns.⁶ The examples they cite are from Harris (1985:52-3), from the languages Mingrelian (1a) and Laz (1b):

- (1) (a) zaza oškviduans nodar-s
 Zaza.NOM he.drown.him Nodar-DAT
- (b) zaza-k oškvidaps nodari
 Zaza.NAR he.drown.him Nodar.NOM
- ‘Zaza drowns Nodar’

These are said to be ‘in an intuitively clear sense ... cognate sentences’. Campbell & Harris (2002:606) clarify that ‘cognate sentences cannot, of course, be descended from a shared sentence ... ; they are examples of shared patterns descended from a pattern in the proto-language.’ No way of determining sentence cognacy is provided, but a number of ‘safeguards’ are presented (1995:349): (i) the verbs, with which the case marking patterns are associated, are entirely cognate, (ii) the tense, aspect and verb class are the same in each pair, (iii) the individual verbal morphemes are cognate, (iv) the meaning is the same, (v) cases occur regularly in

⁶ In a footnote to Campbell & Harris (2002:fn4), it is stated that the term ‘pattern’ is used in the sense of Jackendoff (1994). They clearly view these patterns as transmitted from one generation to another: ‘it is those aspects of language that can be stored in memory that are also potentially reconstructible’ (2002:fn4).

the context of other cases and in the context of particular verb morphology, and (vi) these examples are representative of all regular verbs. There is therefore ‘no basis on which the sentences do *not* correspond’ (1995:349). For their method, it is, however, not necessary that the lexical materials in the example sentences be cognate (1995:350).

The first problem with this method is their use of the term ‘cognate’, which Lightfoot (2002a:123) calls into question. As von Mengden (2008:103) notes, for two items to be cognate in the traditional comparative method requires there to be a diachronic identity between those items and a single item in the protolanguage; Harris & Campbell’s use of the term in ‘cognate sentences’ is therefore very different from its traditional meaning. It is noteworthy that Harris (2008) makes no mention of cognate sentences.

If cognacy of patterns is what is meant, then a second problem emerges, because it is not clear whether patterns are in fact transmitted across generations or even stored in memory. Principles-and-Parameters theory (e.g. Chomsky 1981) has held that the locus of syntactic variation is a finite set of binary parameters; the dominant idea within Minimalism (Chomsky 1995; Baker 2008a) is that all syntactic variation is attributable to the features of lexical items. Neither of these approaches posits anything like the traditional grammatical notion of syntactic ‘pattern’ or ‘construction’.⁷ While this is of course not conclusive evidence for the nonexistence of such entities, it is clear that they are far more controversial units than words or sounds. If, and to the extent to which, patterns have no psychological reality, then no cognacy of any kind can be established and Harris & Campbell’s method collapses.

Even if we allow that some or all of syntax can be profitably analysed in terms of patterns, we encounter a third problem, namely that the method is not sufficiently restricted. The correspondence set in (1) is clearly in terms of function rather than form, as the individual case marking morphemes are not diachronically identical to one another. But how far can this notion of correspondence be taken? In modern spoken German south of the river Main, the perfect tense is used to fulfil the function of the simple past in other varieties of German and English (König

⁷ Although work exploring the possibility of ‘tracking the development of constructions’ has been carried out within frameworks such as Construction Grammar (e.g. Trousdale 2008).

2005:163). We could therefore argue that (2) was a ‘correspondence set’ in Harris & Campbell’s terms:

- (2) (a) Er ist gekommen (Southern German)
 he is come
 ‘He came’
 (b) He came (English)

These correspond in the same intuitive sense as Harris & Campbell’s case pattern examples in (1), and meet safeguards (i), (iv), (v) and (vi) at the very least. However, what we are dealing with here is an example where one pattern - namely the traditional ‘perfect tense’ - has been extended to fulfil the role of another; there is clearly no diachronic identity between the tense forms, one of which is analytic and the other synthetic. Instead we are dealing with ‘pattern replacement’ in the sense of Jeffers (1976:4).

In phonology, neither form nor function alone is sufficient to determine cognacy; instead, distribution is key, and systematic correspondences must be established, as Campbell and Harris (2002:136) recognise. But to judge by the examples in (1), functional similarity is all that is required. If patterns are viewed as cognate in the same sense that words are, then this is equivalent to arguing that English *dog* and German *Hund* are cognates because they have come to fulfil the same role, independent of their form. The only other way to view patterns as cognate would be to consider them analogous to sounds. But sounds can only be established as ‘cognate’ if regular correspondences can be found in the context of multiple distinct word or morpheme pairings: this is the Neogrammarian regularity hypothesis. It is hard to know what this context would be for syntactic patterns, since patterns do not occur in context as such; rather, they *are* the context for the lexical items within them. At any rate, no notion of context is suggested by Harris & Campbell, since their definition of ‘regularity’ makes no reference to this, unlike that of Osthoff & Brugmann (1878:xiii) whom they claim to follow.⁸

While I agree with Harris & Campbell on many points, their rationale for stating that certain items ‘correspond’ to one another does not stand up to careful scrutiny,

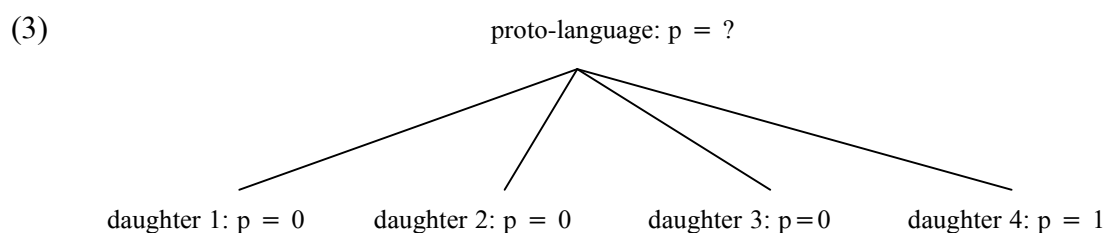
⁸ Their definition of regularity states that ‘alle wörter, in denen der der lautbewegung unterworfenen laut unter gleichen verhältnissen erscheint, werden ohne ausnahme von der änderung ergriffen’ (1878:xiii) (‘all words in which the sound subject to the sound change appears in the same context are affected by the change without exception’); the notion of phonological environment is thus clearly already present in this work.

and their interpretation of the terms ‘cognate’ and ‘regularity’ is out of step with the general use of these terms in phonological reconstruction.

1.1.5 Parametric reconstruction

One further angle to consider is that afforded by the Principles and Parameters research program, in particular the approaches suggested by Roberts (1998, 2007) and Longobardi (2008; Longobardi & Guardiano 2009). I here evaluate these two approaches in turn.

The approach of Roberts (1998, 2007) observes that if UG provides a finite set of binary parameters defining all syntactic variation then we already have a set of points of correspondence between grammars. Given this, we can schematically represent the history of the languages involved as in (3):



(Roberts 2007:363)

One can then use majority rule, or considerations of directionality, to reconstruct a value for the protolanguage. Roberts illustrates by way of the OV/VO parameter with regard to PIE: Latin, Sanskrit and Gothic are all said to be OV, while Old Irish is VO. This suggests a reconstruction of OV because of majority rule as well as the facts that (i) Old Irish is younger than the other three, (ii) Archaic Old Irish shows some evidence of OV orders and (iii) ‘OV > VO seems to be a more natural change than VO > OV’ (Roberts 2007:365).

Leaving aside (iii), which could be disputed,⁹ these criteria are reasonable. For completeness’ sake it should be mentioned that, like archaic Old Irish, none of the three languages given as OV is consistently so. Both Sanskrit (Delbrück 1900:81)

⁹ Claims similar to (iii) are frequently made in the literature (e.g. by Kiparsky 1996; Newmeyer 2000); however, these are often based on the assumption that PIE and its early daughters were fundamentally OV (e.g. Kiparsky 1996:140), giving rise to circularity. To my knowledge, no reliable statistical evidence for such a claim has so far been presented in the literature.

and Gothic (Eythórsson 1995:ch.1) frequently display VO orderings, and the word order variation in Latin is notorious. Other early IE languages with more variable or VO word orders, such as Albanian and Homeric Greek, are also left out of consideration, and so this sample reconstruction is perhaps not as straightforward as suggested.

Binarity is a problem for this approach, since, all else being equal, the probability that two languages will share a value for a given parameter is 0.5. Chance resemblance is thus very difficult to rule out, and even a random distribution of parameter settings in an odd number of languages will yield a reconstruction if majority rule is the deciding factor.

This aside, the logic behind this form of reconstruction is sound; however, as von Mengden (2008:116) notes, ‘it is not the empirical linguistic data which provide precise comparanda but the axiomatic model designed by the theory’. In other words, to reap the benefits of this approach one must accept a specific model of syntactic variation revolving around a finite set of binary parameters. Furthermore, this model has been challenged by many linguists in recent years. Newmeyer (2004) and Haspelmath (2008), for example, both argue that the clusterings of properties theorised to occur as a result of parameters (cf. Ferraresi & Goldbach 2008:13) have not materialised, and that a vast (perhaps infinite) number of parameters would be required to capture all possible syntactic variation. Within Minimalism, Boeckx (2008) suggests that ‘empirically the expectations of the traditional Principles and Parameters model have not been met’, and even Baker, a supporter of macroparametric approaches, admits that few accepted results have been achieved in the field (2008a:352), despite its twenty-year history. Newmeyer (2004:191-3) also argues that there is no *a priori* reason to suppose that parameters should be binary, and also no clear empirical evidence for this.

Losing the assumption of binarity merely makes applying this method less straightforward. However, losing the assumption that there is a fixed universal set of parameters is more of a problem. If instead the Minimalist view that parametric variation is variation in the formal features of functional lexical items (Baker 2008a:353) is adopted, then we lose the conceptual neatness of parametric correspondences, since before postulating correspondence we must verify that the lexical items themselves are cognate; for instance, there is no reason to expect that exponents of C in one language are cognate in any sense with exponents of C in another. Although logically sound, then, Roberts’s (2007) parametric reconstruction

only provides a neat solution to the problem of comparison if we accept a theory of syntactic variation that has little empirical support.

Longobardi's work (2008, Longobardi & Guardiano 2009) does not directly target syntactic reconstruction. Instead these works, although supportive of the possibility of syntactic reconstruction in general, 'take a further step' (2008:xiii) and suggest that parametric linguistics may serve as a tool for establishing phylogenetic relations among languages. I will not discuss these methods in detail, but one point arises that is interesting from the perspective of reconstruction: comparing parameter values as abstract universals provides no way of differentiating between common ancestry, chance typological resemblance, and language contact. As regards language contact, Grico, a dialect of Greek spoken in southern Italy, is analysed by Longobardi & Guardiano's method as belonging to the Romance subgrouping of IE, and English and Norwegian are analysed as more closely related than English and German, which may be due to 'the Scandinavian influence on English' (2009:15). As regards chance resemblance, the unrelated languages Wolof and Old English are assigned a higher likelihood of being related than the Germanic languages Gothic and English (2009:22). The comparative method in phonology, because of its focus on systematic correspondences, is able to identify and exclude such chance resemblances. The danger for syntactic reconstruction, however, is that features that these languages have in common may influence our choice of what to reconstruct such that our reconstructed protolanguage may reflect parallel innovations or similarities due to language contact rather than shared inheritance.

1.2 Obstacles to syntactic reconstruction

1.2.1 The correspondence problem

Watkins (1976:312) states that 'the first law of comparative grammar is that you've got to know what to compare'. The difficulty of finding comparanda in syntax is the most often emphasised problem for syntactic reconstruction. As Lightfoot puts it, 'it is hard to know what a corresponding form could be in syntax, hard to know how one could define a sentence of French which corresponds to some sentence of English, and therefore hard to see how the comparative method could have anything to work with' (2002a:119). It is clear that sentences, in the vast majority of cases, cannot be cognate in the traditional sense: 'For sentences ... acquisition by learning is most unusual ... Sentences are formed, not learned; morphemes and simple

lexemes are learned, not formed' (Winter 1984:622-3). It is rare that diachronic identity in the sense of transmission can be established for sentences.

Two solutions to this problem have been proposed in recent years, taking patterns (Harris & Campbell 1995) and parameters (Roberts 1998) as the units of correspondence; their attendant problems are discussed in 1.1.4-1.1.5. Most problematically, in neither case is there a clear basis for distinguishing structural similarity caused by language contact or parallel innovation from structural similarity caused by shared inheritance in a manner that is comparable to the Neogrammarian regularity hypothesis. Harris & Campbell acknowledge that 'grammatical borrowing may complicate reconstruction of syntax just as it does the reconstruction of phonology, morphology, and the lexicon' (1995:372), but lexical and morphological borrowings are easy to identify and exclude in the comparative method because they do not show regular sound correspondences. All Harris and Campbell can offer is the suggestion that 'syntactic deviations in some daughter language from an otherwise common pattern shared by the other daughters of a family may similarly suggest possible borrowing and urge us to investigate further for possible sources' (1995:372) - but no diagnostic for the more problematic opposite case, in which the daughter languages become more homogeneous due to contact, is provided (or in my view possible). This criticism applies, *mutatis mutandis*, to parametric approaches: 'structural similarity may mislead a historian' (Lightfoot 2002a:117).

The correspondence problem, then, is a major problem for syntactic reconstruction, and no comprehensive solution has yet been presented. The majority of section 2 of this dissertation is devoted to sketching such a solution, insofar as is possible.

1.2.2 The 'pool of variants' problem

The 'pool of variants' problem is mentioned by Roberts (2007, following Vincent & Roberts 1999). It is illustrated on the basis of forms used for the future tense of the verb 'to sing' in Romance: French *chanterai*, Italian *canterò*, Spanish *cantaré*, Rumanian *voi cânta*, Sardinian *appo a cantare*, while Calabrese and Salentino have no form. 'How are we to decide what the original form might have been on this basis?' (2007:362).

A partial answer to this question has already been given: these tense forms correspond in function only, and function is not a reliable basis for determining

correspondence. As discussed in 1.1.4, the problem has an analogue in lexical/phonological reconstruction: English *dog* and German *Hund* are words fulfilling the same function, but diachronic identity can be excluded on the basis that regularity shows that they are not cognates. The ‘pool of variants’ problem thus reduces to the correspondence problem: if a reliable way of reinforcing hypotheses about correspondence can be stated, we would have an independent criterion for stating that the Romance forms listed above do not correspond.

1.2.3 The discontinuity of syntactic change

Lightfoot has repeatedly emphasised that the lack of continuity between grammars, and the reanalytic nature of syntactic change, is an obstacle to reconstruction: ‘grammars are not transmitted historically, but must be created afresh by each new language learner ... If this is correct, one can deduce very little about the form of a proto-grammar merely through an examination of the formal properties of the daughter grammars’ (1980:37).

Two points should be made here. First, although it is true that grammars are created afresh by each generation, it is also true that language acquisition is incredibly successful most of the time; indeed, this was, and remains, one of the key motivations of generative theory, in the form of the poverty-of-the-stimulus argument. Given a finite array of data there are infinitely many theories consistent with it but inconsistent with one another (cf. Hauser, Chomsky & Fitch 2002:1577), but it should follow from UG and/or general learning principles that grammars actually acquired do not vary substantially from one another, i.e. that L1 acquisition is deterministic. Hale (1998:9) explicitly assumes that convergence on the target grammar is successful in the default case, and a similar notion is implicit in the principle of Inertia (Keenan 2002:2, Longobardi 2001:278). This vitiates Lightfoot’s criticism, as under these assumptions there is no reason to assume that the grammars of successive generations will be drastically different; in fact, quite the contrary.

A second relevant point is that, ‘if Lightfoot’s objection were valid, it would presumably apply equally to that portion of the grammar that handles phonology. This would equally mean that phonological reconstruction were impossible’ (Harris & Campbell 1995:372). Therefore, if one accepts the validity of phonological reconstructions in some sense, this objection to syntactic reconstruction has no purchase. The fact that change occurs by way of discrete reanalyses is also unproblematic, as there is evidence that the same is true of phonology: Ohala (1981)

has shown that reanalyses of the speech signal on the part of the listener commonly lead to phonologically abrupt sound changes, e.g. of the type $VN > \tilde{V}N > \tilde{V}$. Yet these changes are just as reconstructible as any other. The comparative method in phonology does not require sound change to be gradual.

Discontinuity and reanalysis are thus no more problematic for syntactic reconstruction than they are for phonological reconstruction.

1.2.4 Directionality

In phonological reconstruction, statements about the predictable direction of sound changes help us to reconstruct proto-sounds: for instance, $b > p / V_V$ is a highly unlikely change, whereas $p > b / V_V$ is natural and often found (Harris & Campbell 1995:361). Lightfoot (2002a) claims that ‘we have no well-founded basis for claiming that languages or grammars change in one direction but not in another’ (2002a:126), and that ‘there is no theory of change to be had independent of theories of grammar and acquisition’ (2002a:127). In response, Campbell & Harris (2002:612) argue that ‘directionality in many changes is a fact of linguistic life’ and that ‘appeal to directionality is a valid and fundamental criterion in reconstruction within the comparative method’. As discussed in 1.1.3, tendencies of directionality do exist and can be used in change. To cite just one example, ‘there is a verifiable directionality by which certain lexical verbs with broad semantic content (e.g. “want”, “go”, “come”, “have”, “possess”, etc.) have become modals in many independent languages around the world, while few, if any, modals have changed to become full lexical verbs with these meanings’ (Campbell & Harris 2002:613).

While it may be the case that statements about directionality in syntactic change are scarcer and less reliable than those found in sound change, then, they do exist, and can be used in much the same manner.

1.3 Interim conclusion

As I have tried to show in section 1.1, while syntactic reconstruction as it has been practised in the past has led to some interesting results, none of the methods so far proposed is entirely reliable, which may explain why none has found wide acceptance. This is largely due to the apparent intractability of the correspondence problem in syntax, which, as section 1.2 demonstrated, is the only major obstacle to a successful approach to syntactic reconstruction: all other purported obstacles are

either overblown or reduce to the correspondence problem. Solving this problem is the aim of the next section.

2. The comparative method in phonological and syntactic reconstruction

In this section I will first attempt to describe and define the comparative method as practised in phonology (2.1). Section 2.2 will outline my syntactic framework and assumptions, and section 2.3 explores the isomorphism between phonological and syntactic reconstruction when using this framework.

2.1 The comparative method in phonological reconstruction

The comparative method has long been considered one of the greatest successes of linguistics. Campbell (1998:108), for example, writes: ‘The comparative method is central to historical linguistics, the most important of the various methods and techniques we use to recover linguistic history’.

Despite the great importance accorded to it, however, the comparative method has always eluded definition. In Trask’s *Dictionary of Historical and Comparative Linguistics* (2000:64-5), the entry on the comparative method begins by classing it as ‘the central method in comparative linguistics’, only later acknowledging that ‘oddly, in spite of the great success and the universal acceptance of the method, there were hardly any attempts at characterizing it explicitly’. This vagueness is nothing new: Meillet (1954), in a chapter entitled *Définition de la méthode comparative*, presents no more explicit definition, instead relying purely on ostension by giving examples of reconstruction. Fox (1995:57) suggests that to the nineteenth-century linguists who developed it, the comparative method was ‘evidently regarded as ... a matter of improvisation, of personal insight, rather than of strict application of mechanical principles’.¹⁰

This lack of an explicit definition is problematic if we wish to find out whether an isomorphism between phonology and syntax exists that will permit application of the comparative method in both fields, although it has not stopped historical linguists from claiming that it is possible or impossible. Jeffers (1976:5) argues that ‘a straightforward transfer of the principles of the comparative method to the

¹⁰ In fact, it is likely that nineteenth-century linguists did not view themselves as following a specific method at all. Baxter (2002) suggests that the term was popularised in its modern, specifically linguistic sense by Meillet in the twentieth century. Furthermore, the term (or its translation) was never widely adopted in the German tradition, with ‘vergleichende Rekonstruktion’ (‘comparative reconstruction’) being a more common term for the endeavour (Anthony Fox, p.c.)

reconstruction of syntax seems totally inappropriate’, while in the same year Watkins (1976:306) claims that syntactic reconstruction is possible ‘within the comparative method’, and Fox (1995:105) states that ‘syntactic reconstruction has been undertaken since the end of the nineteenth century, using the Comparative Method’. Such claims are difficult to interpret without a clearly agreed definition of the traditional methodology. In the remainder of this section, therefore, I work towards a clear statement of the method, starting with its goals.

2.1.1 Role and reality of reconstructions

There is some disagreement among linguists even regarding the purpose of the method. Campbell, as we have seen, states that its key role is to recover linguistic history, i.e. to find out something about the protolanguage. Others, such as Harrison (2003:215), argue that ‘[t]he primary role of the comparative method is in developing and testing hypotheses regarding genetic relatedness’, with reconstruction of the proto-language a secondary goal. I will follow Longobardi (2008:xiv), who argues that, although it is important to recognise these two goals as distinct, the method provides us with both. My focus is on the reconstruction of linguistic history, although, as we will see (2.1.3), the demonstration of phylogenetic relatedness actually comes ‘for free’ where reconstruction can be successfully carried out.

An important question concerns the status of our reconstructions, i.e. how to interpret the results of the method. The argument is often framed in terms of ‘formalism’ vs. ‘realism’ (e.g. Fox 1995:9): where realists see their reconstructions as representing historical forms of a once-spoken language, formalists view them as abstract formulae representing relationships within the data. However, this division is not as straightforward as it may seem. Pulgram (1959:424) argues that we must distinguish ‘Real PIE’, the unattested language spoken at some point in the distant past, from ‘Reconstructed PIE’, the result of the comparative method applied to its daughters. Although the distinction is undoubtedly an important one, it does not follow that Reconstructed PIE bears no relation to Real PIE; rather, the former is an approximation to, and a hypothesis about, the latter (cf. Campbell & Harris 2002:600).

Moreover, few historical linguists have self-identified as formalists. The position is generally attributed to Meillet, on the basis of a reading of the following passage:

‘la seule réalité à laquelle elle ait affaire, ce sont les correspondances entre les langues attestées. Les correspondances supposent une réalité commune; mais de cette réalité on ne peut se faire une idée que par des hypothèses, et ces hypothèses sont invérifiables : la correspondance seule est donc objet de science ... ce que fournit la méthode de la grammaire comparée n'est pas une restitution de l'indo-européen, tel qu'il a été parlé : *c'est un système défini de correspondances entre des langues historiquement attestées.*’ (Meillet 1934:41, 47)¹¹

However, as Campbell and Harris (2002:601) emphasise, this view is not the same as the abstract formulist position sketched above. This becomes clear on a reading of some of Meillet’s other work, e.g.:

‘La communauté d’origine se reconnaît à ce que ces langues concordent à beaucoup d’égards; et c’est en observant les concordances qu’on arrive à restaurer, par hypothèse, mais de manière sûre, l’original commun, non attestée, des diverses langues indo-européennes.’ (Meillet 1917:1)¹²

Although Meillet refers to reconstructions as hypotheses, he states that it is possible to restore a common original and that the method for doing so is reliable.

The ‘pure’ formulist view also suffers from a number of other defects. As Lass (1993:169) observes, if IE */p/ and */t/ are simply abstract cover symbols and could as well be represented by */♠/ and */♣/, we have no principled explanation for why their reflexes are [p, f] and [t, θ] respectively. Lass points out that without phonetic assumptions it would have been impossible to posit laryngeals for PIE. Furthermore, as observed by Trask (1996a:240), if we view the product of the comparative method as merely a set of correspondences rather than as a language, then certain tools that we could otherwise use as heuristics to evaluate the correspondences we have posited are unavailable to us, e.g. typological generalisations about the plausibility of sound systems and generalisations about the directionality of change,

¹¹ ‘the only reality it had to do with were correspondences between attested languages. These correspondences suppose a common reality; but of this reality one can only get an idea through hypotheses, and these hypotheses are unverifiable: only the correspondence is the object of science ... what the method of comparative grammar provides is not a restoration of Indo-European as it was spoken: *it is a system of correspondences between the historically attested languages.*’

¹² ‘Their common origin can be recognised in that these languages agree in many respects; and it is through observing these agreements that one comes to restore, by hypothesis, but through a reliable method, the unattested common ancestor of the diverse Indo-European languages.’

as to apply such heuristics is to assume that the object of study is a natural language.

I therefore take the ‘realist’ (hypothesist) position to be the only tenable one. Although a decent dose of scepticism is only healthy, ‘[t]he reconstructing historian is making claims about substance whether he thinks he is or not’ (Lass 1993:169), and to adopt a formulist stance is to ignore some of the evidence at our disposal, both for establishing phylogenetic relations and for reconstructing linguistic history.

2.1.2 Falsifiability

The issue of proof is one that is often raised by those concerned about the status of reconstructions. I will frame my discussion here in terms of falsifiability as defined by Popper (1935), since this is commonly considered a necessary condition for a theory to be scientific.

Meillet, as we have seen, stated that our hypotheses about reconstructions are unverifiable (1934:41). More recently, the issue has been raised explicitly by von Mengden (2008) as follows:

‘If we have come to a reasonable reconstruction ... it will be extremely difficult to find supporting data ... to verify our results. Reconstructions ... therefore potentially lack an essential prerequisite of scientific validity: the possibility to falsify the results.’ (2008:116)¹³

There are two separate issues here: adequacy of methods, and adequacy of results. It is possible to apply our reconstructive methods to families for which the ancestor language is attested and thus to assess the degree of success of our reconstruction, as has been done for the comparative method in Romance by Hall (1950). The hypothesis that our reconstructive methods are sound, then, is not itself unfalsifiable.

The results of a given application of the method are a different matter. With Proto-Germanic or PIE, for example, we have nothing at present to compare our results against. This is the case for almost all reconstructions, since the object of reconstruction is to recover linguistic history and this is only necessary and

¹³ Verifiability is not the same as falsifiability; cf. Popper (1935). Equating the two, as von Mengden does, is a common error.

worthwhile when that linguistic history is not already evident. This does not mean that our hypotheses are unfalsifiable in principle, however. To borrow a favoured analogy of Lightfoot's (e.g. 2002a:135), if language is like the weather, then reconstructions are like a weather forecast. When we make predictions about the weather, e.g. 'It will rain tomorrow', it is not immediately possible to judge whether or not they are correct; on the other hand, we know exactly what it would take to confirm or deny them, i.e. whether or not it rains tomorrow (Popper 1935:13). It is the same with reconstructions: although we do not have the wherewithal to assess our hypotheses at the time that we make them, we know that the discovery of texts in the ancestor language would provide what we need to confirm or deny them.¹⁴

Of course, it is questionable whether texts could be demonstrated to be written in a given protolanguage. Furthermore, as Pulgram (1959:424) reminds us, it is probable that speakers of languages as early as PIE could not write, and consequently that no texts exist. These two problems reduce the likelihood of falsification, but have no bearing on the logical issue of falsifiability, since we do not know whether they will pose real problems at all. Although it is highly unlikely that any given reconstruction will be falsified empirically, then, reconstructions are not inherently unfalsifiable, and therefore, if we follow Popper, not unscientific.

2.1.3 Defining the method

I will take as my starting point the definition given by Ross & Durie (1996:6-7), who attempt to summarise CM as a set of instructions:

1. Determine on the strength of diagnostic evidence that a set of languages are genetically related, that is, that they constitute a 'family';
2. Collect putative cognate sets for the family (both morphological paradigms and lexical items).
3. Work out the sound correspondences from the cognate sets, putting 'irregular' cognate sets on one side;
4. Reconstruct the protolanguage of the family as follows:
 - a. Reconstruct the protophonology from the sound correspondences worked out in (3), using conventional wisdom regarding the directions of sound changes.

¹⁴ Discovery of another daughter language would of course test the reconstruction, as happened with Anatolian and Tocharian for PIE. Loans from Proto-Germanic into Finnish have also been used to test reconstructions.

- b. Reconstruct protophonemes (both morphological paradigms and lexical items) from the cognate sets collected in (2), using the protophonology reconstructed in (4a).
5. Establish innovations (phonological, lexical, semantic, morphological, morphosyntactic) shared by groups of languages within the family relative to the reconstructed protolanguage.
6. Tabulate the innovations established in (5) to arrive at an internal classification of the family, a 'family tree'.
7. Construct an etymological dictionary, tracing borrowings, semantic change, and so forth, for the lexicon of the family (or of one language of the family).

Ross and Durie take determination of genetic relatedness to be a prerequisite for the application of the core of the method rather than as a result of its application, following Nichols (1996), who argues that the comparative method 'can only describe and extend relatedness but cannot establish it' (1996:64). Rather than using the method to establish relatedness, '[o]nce relatedness is assumed, then the labor-intensive processes of working out the correspondences and cognate sets begins' (1996:41). Nichols's claim is that diagnostic evidence is probabilistic rather than absolute, and that it comes from morphological systems rather than sound correspondences (1996:64). I disagree with this claim for three reasons. Firstly, it is necessary to use the comparative method to confirm the relatedness of morphological paradigms: mere surface similarity is simply 'type-identifying' evidence and not 'individual-identifying', to use Nichols's (1996:48) terms. Secondly, Nichols equates assuming relatedness (1996:41) with establishing it (1996:64). Thirdly, the specific probabilities Nichols adduces for her calculations of likelihood are shockingly *ad hoc*. For instance, she claims that the chance of finding a dental as second consonant in a language's word for water is 0.15 (three in twenty), since 'the worldwide average number of consonants in inventories is twenty' and 'there are three dentals in the basic inventory' (1996:67). This assumes that all consonants are equally likely to appear in any given position, that the language in question is 'average' as regards its number of consonants and distribution of dentals, that it has only one word for water, and that that word has at least two consonants (cf. French *eau*).

Even if we disagree with Nichols's conclusions, an important point about the nature of the method is raised: the initial step of discerning possible relatedness is a matter of a creative leap rather than mechanical application of any method. (Greenberg

(1999:170) makes the same point.) The comparative method should be seen as a process of hypothesis testing rather than hypothesis formation, and its role is to *establish* suspected relatedness.

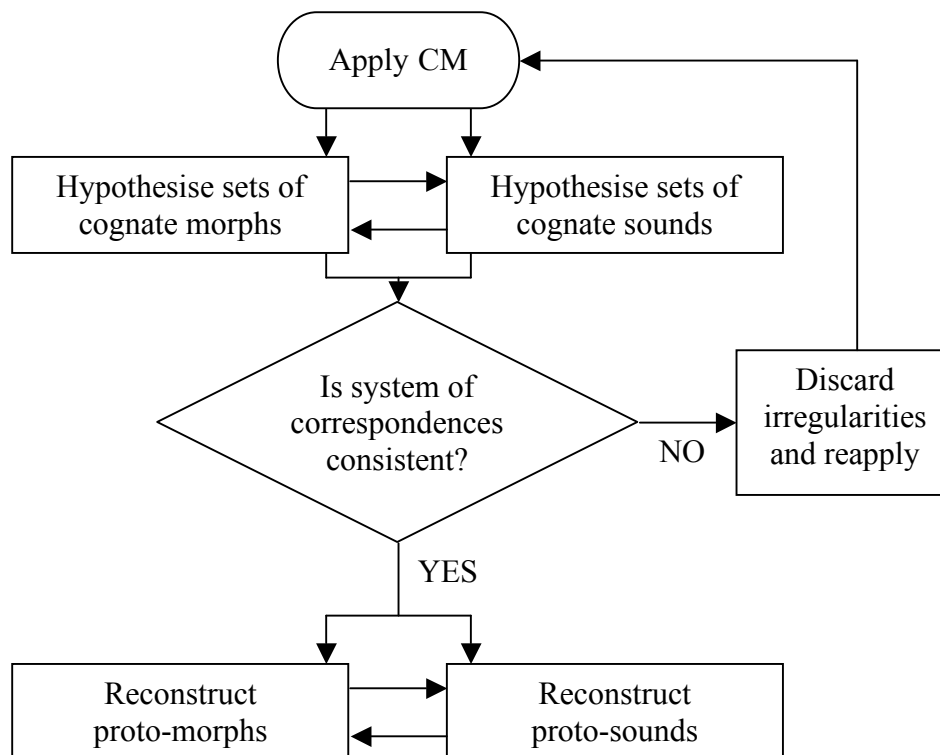
I therefore reject step 1 of Ross & Durie's algorithm. This opens up the possibility of carrying out the method on unrelated languages. In fact, this presents us with no problem: if the compared languages are unrelated then step 4 will simply fail, since no correspondences will be established in step 3 and we will therefore not be able to reconstruct. In addition, I will leave aside steps 5-7, since subgrouping is not my concern. This leaves us with steps 2-4, the core of the method.

The first point to note is the inductive nature of step 2, which Ross & Durie capture using the word 'putative'. The initial discovery of cognate sets, like the discovery of potential relatedness, is simply reached by observance of similarities between morphs, phonological and/or semantic.¹⁵ Since it is these cognate sets that allow the establishment of sound correspondences in step 3 of Ross & Durie's algorithm, the sound correspondences themselves cannot be used to determine cognacy at this stage. An alternative way to proceed would be to reverse the order of steps 2 and 3, i.e. starting by hypothesising sound correspondences and proceeding from these hypotheses to work out cognate sets. A circularity thus emerges: in the comparative method, the cognacy of morphs is demonstrated by the cognacy of the sounds within them, which itself is demonstrated by the cognacy of the morphs in which they occur. This circularity is acceptable, however, to the extent that alternative explanations (chance similarity, or massive borrowing) are less plausible in accounting for the data. The account is justified by its internal coherence, which goes some way towards defending against the charge of circularity.

Neither 2 nor 3 need be prior to the other, then. This allows us to represent the method as in (1), reworked from Ross & Durie's algorithm:

¹⁵ Semantic similarity is useful for making this creative leap, but not required for the comparative method proper. To see why, consider French *bureau* 'office' and Spanish *buriel* 'a coarse cloth', which are cognate despite having completely different meanings.

(1)



If no consistent system of correspondences can be reached, then a) the languages under comparison are not shown to be related and b) no reconstruction (step 4 of Ross & Durie's algorithm) can be carried out.

2.1.4 Cognacy

The notion of 'cognacy' needs further clarification. As hinted above, two distinct classes of item can be cognate in phonological reconstruction: sounds, and the environments in which they occur, morphs.¹⁶ Ross & Durie's algorithm requires correspondences to be drawn from cognate sets. I will state this assumption as in (2):

(2) **Double Cognacy Condition:**

In order to form a correspondence set, the contexts in which postulated cognate sounds occur must themselves be cognate.

¹⁶ Although the term is usually applied only to words (cf. the definition in Trask 1996b: 78), I use the term to apply to sounds in the intuitively clear sense mentioned by Harris & Campbell: 'sounds which are related to each other ... by virtue of descent from a common ancestral pronunciation' (1995: 345).

However, it is not clear that (2) is necessary to establish phonological correspondences. Consider the following example of a potential correspondence set (from Harris & Campbell 1995:367):

(3)	English	German	Gothic	Old Norse	
	<i>adder</i>	<i>Natter</i>	<i>naðr-</i>	<i>naðra</i>	‘adder’

The ‘deviant’ English form could be argued to be no longer cognate with the others, since reanalysis has robbed it of its initial *n*. Indeed, the set does not illustrate correspondence with regard to *n*, since the change that caused its loss in English was not a regular sound change. However, it does illustrate correspondence with regard to its medial consonant: /d/ : /t/ : /ð/ : /d̥/, a cognate sound and a correspondence amply attested elsewhere.

If our interest is on the level of proto-phonemes (as it is for Ross & Durie, as their algorithm makes no reference to the reconstruction of the forms of lexical items), we can even go a step further and reject (2) altogether. The Neogrammarian regularity hypothesis entails that what determines the reflexes of cognate sounds is *not* the lexical item in which they occur but the phonological environment they find themselves in. With this in mind, we could construct correspondence sets of a sort in which the lexical items used were not themselves cognate. For example:

(4)	English	German
	<i>trap</i>	<i>straff</i> ‘taut’
	<i>prig</i>	<i>Pfriem</i> ‘awl’

These words, which have no cognates in the other language, could be said to illustrate the correspondences between English /p/ and German /pf/ in initial position, /f/ in final position. The sounds are cognate, but the morphs in which they are presented are not. The purpose of such an example would be to illustrate the sound changes that we hypothesise. As with the traditional method, more occurrences of the predicted sound in the predicted position provide evidence for the hypothesis. Such a method could come in useful in cases where insufficient cognate words survive into the daughter languages.

What are the consequences of this rejection of (2)? Most importantly, if we do not use cognate morphs we cannot demonstrate that our hypothesised sound correspondences are correct; instead the positional alternation found in German

must be taken as evidence in itself. If we assume (2), then our hypothesised sets of cognate morphs and our hypothesised sets of cognate sounds provide mutually reinforcing evidence. Although this reasoning is circular, as demonstrated in 2.1.3, rejecting (2) means that there is no method of reinforcing our initial hypotheses, which must then stand or fall on their own merit.

It is advantageous, then, to assume (2). As I show in the rest of this section, this has consequences for syntactic reconstruction, since because of the nature of syntactic variation this assumption cannot generally be made.

2.2 Modelling syntactic variation

A key problem in comparative syntax is that it has not always been clear how to represent cross-linguistic syntactic variation. As we have seen above (1.1), some authors (e.g. Harris & Campbell 1995) model syntactic change in terms of syntactic patterns; others (e.g. Longobardi & Guardiano 2009) represent it in terms of the values of a universal set of binary parameters, some of which may be dependent on others; and still others (e.g. Newmeyer 2004) prefer to represent it in terms of a set of language-specific rules.

The approach I will focus on here is that stated in (5):

(5) The Borer-Chomsky Conjecture

All parameters of variation are attributable to the features of particular items (e.g., the functional heads) in the lexicon.

(Baker 2008a:353)

This approach is associated with Minimalism, due in no small part to Chomsky's support in the Minimalist Program (e.g. 1995:131), but has its origins in a considerably earlier stage of the Principles & Parameters paradigm. Borer (1984) points out the explanatory advantages of such a system:

‘The inventory of inflectional rules and of grammatical formatives in any given language is idiosyncratic and learned on the basis of input data. If all interlanguage variation is attributable to that system, the burden of learning is placed exactly on that component of grammar for which there is strong

evidence of learning: the vocabulary and its idiosyncratic properties.’ (Borer 1984:29)¹⁷

As Baker (2008a) emphasises, (5) is not an established result but a conjecture. Empirical falsification could come from at least two sources. Firstly, linguistic data impossible to represent in such a model could be found. Secondly, since (5) is a conjecture about psychological reality, psycho- or neurolinguistic evidence could be adduced to show that this is not the way the human brain operates. Since no such falsifying evidence has yet been found, to the best of my knowledge, I will assume (5) in what follows.¹⁸

I am not the first to suggest that (5) should be investigated as the basis for syntactic reconstruction: Pires & Thomason (2008:47) and especially Bowerman (2008:195) mention the possibility. However, its implications for reconstruction have not been explored in detail, and this will be the main focus of section 2.3 below.

My claim is essentially that, if (5) is correct, then all of syntax can in principle be reconstructed along the lines laid out in 2.3. It does not, of course, follow that if (5) is false then none of syntax can be reconstructed along these lines. Rather, any syntactic phenomenon that *can* be realistically described in terms of featural variation on items in the lexicon is amenable to reconstruction using the approach presented in 2.3.

The framework I will use to present my analyses is that of the Minimalist Program (Chomsky 1995); further details will be given with the analyses in section 3. Many aspects of this framework will likely be superseded, and the analyses themselves will almost certainly be. Like phonological reconstruction, syntactic reconstruction is dependent on correct analyses of the daughter languages, and the relative difficulty of syntactic analysis of a given string compared to phonological analysis cannot be taken as an argument against the validity of the approach presented in 2.3. A correct analysis surely exists in both cases, and can be found or approximated.

¹⁷ ‘Inflectional rules’ in her framework are associated with items drawn from the lexicon; cf. Borer (1984: 20).

¹⁸ Note that the existence of macroparameters such as those suggested by Baker (2008b), even if clearly demonstrated, would not *falsify* (5); if it is still possible to describe the data using clusters of microparameters, then the accretion of these parameters might find an alternative explanation, e.g. in terms of third-factor considerations such as acquisition strategies.

I take it as given that grammars are constructed anew by each generation as repeatedly emphasised by Lightfoot (e.g. 1979a, 1999). I also assume that convergence on the target grammar of the previous generation is successful in the default case, and that change only occurs to the extent that the PLD require it to; this is a variant on the inertia hypotheses of Keenan (2002) and Longobardi (2001).

2.3 The comparative method in syntactic reconstruction

As the figure in (1) shows, there are two key steps to reconstruction: finding systematic correspondences, and postulating protoforms (cf. Harris & Campbell 1995:344). I will discuss each of these in turn.

2.3.1 Solving the correspondence problem

As we saw in section 2.1, a crucial component of the comparative method in phonology is the notion of context: sounds develop regularly according to the phonological environment they find themselves in. This is the Neogrammarian regularity hypothesis (Osthoff & Brugmann 1878). As demonstrated in section 1.2.1, this notion of context is not present in the approaches to syntactic reconstruction of Harris & Campbell (1995) or Roberts (1998), and it cannot therefore be said that these approaches employ the comparative method. This section is an attempt to find an isomorphism between phonology and syntax such that the comparative method can be employed in the latter.

The obvious starting point, given our approach to syntactic variation in 2.2, is to equate phonemes with lexemes: both are in some sense minimal units of composition, and both are stored in some sort of inventory.¹⁹ Furthermore, these units are not atomic in major models of phonology and syntax but can be decomposed into feature matrices. Two possible analyses are given below:

¹⁹ This raises the interesting possibility that the same notions of symmetry, balance and differentiation employed by structuralist phonologists (e.g. Trubetzkoy 1962: 60) could be applied to the generative lexicon. Something like this may be implicit in Chomsky's (1995: 235) suggestion that the lexicon is not merely a list of exceptions but 'provides an "optimal coding" of such idiosyncrasies'.

$$(6) \quad (a) \quad /t/ = \left(\begin{array}{l} + \text{coronal} \\ - \text{voice} \\ - \text{-cont} \\ + \text{ant} \\ + \text{dist} \end{array} \right) \quad (b) \quad \text{'dog'} = \left(\begin{array}{l} + \text{animate} \\ + \text{count} \\ + \text{N} \end{array} \right)$$

If some or all of phonological change can be represented as the change of features of phonemes, then it seems reasonable to assume that some or all of syntactic change can be represented as the change of features of lexical items.

But we can go a step further. Although unconditioned changes in the featural composition of phonemes may occur, many changes are represented in terms of conditioning environments, as in (7):

$$(7) \quad r > \emptyset / V _ [C, + \text{coronal}] \#$$

(a change that happened in English around 1300, according to Lass (1997:284-5)).
Do environmentally-conditioned syntactic changes occur?

There is evidence that they do. Longobardi (2001) provides an example from the history of French, where the Latin noun *casa(m)* 'hut, house' developed in two different ways: into Old French *chiese*, a noun that was later lost except in a few fixed expressions, and into Old French *chies*, which became the Modern French preposition *chez* (2001:276). Using a variety of evidence from the Romance languages he demonstrates that a 'construct state' construction is present in some of these languages, in which common nouns move leftward to a phonetically empty D(eterminer) position under certain conditions, and that French *chez* shared enough of the properties of this construction to be plausibly derived from it. The phonological alternation is then explained on the grounds of differing stress patterns (2001:293). Importantly for our purposes, the single lexical item *casa(m)* develops in two different ways in different contexts: where it moves to D, it becomes the preposition *chez* (presumably through string reanalysis of a D head as a P head at some point during the history of French), and elsewhere it remains syntactically the same. This type of change, where the 'new' and 'old' items coexist in the same grammar, is referred to as 'layering' in the literature on grammaticalisation (e.g. Hopper & Traugott 2003:124), and is analogous to a phonemic split. Furthermore, there is no reason to consider this type of syntactic change a rarity: a similar example from Germanic will be presented in section 3.1.

We can thus see that an effect akin to the Neogrammarian regularity hypothesis is at work. Syntactic change of a given lexical item may occur within a correctly defined context, and may well be exceptionless. This in fact follows from the reanalytic nature of syntactic change: if a speaker reanalyses an item in a certain context, e.g. a noun as a preposition, that speaker will not also continue to postulate the original ('correct') analysis of that item in that context. String-adjacency and other configurations of linear order may be insufficient to define syntactic environment, and appeals to hierarchical structure may be needed to describe specific changes, but this is not a problem, since phonological environments too must sometimes be defined in terms of suprasegmental features not related to the linear ordering of the phonemes involved (e.g. Verner's Law).

Pires & Thomason (2008) challenge the idea that there can be regularity in syntactic change, arguing that the analogical spread of animacy through Slavic noun declension paradigms is 'not regular in the sense of regular sound change', although they admit that 'the analogic changes that led to the current states of [Russian and Čakavian Serbo-Croatian] were regular in that they affected all nouns in the relevant class, case, and number categories' (2008:53). This appears to be a misunderstanding of the nature of regularity, since, as the above quotation shows, their example in fact provides evidence for it. Evidence *against* regularity would be provided by a speaker sporadically using the old and the new forms of the same noun interchangeably in different sentences involving the same syntactic and interpretive context; as mentioned above, the reanalytic nature of syntactic change means that this is unlikely to happen.

In a footnote, Pires & Thomason (2008:fn17) also cite personal communication from Longobardi, stating that he has never argued for regularity of syntactic change, 'considering, for instance, that change of syntactic features may spread regularly [sic] and incompletely through similar lexical items'. Again, this is not a problem if we wish to maintain that syntactic change is regular. The High German Second Consonant Shift occurred to different extents in different geographical areas: /t/ > /ts/ in initial position occurred in all High German dialects, whereas /p/ > /pf/ in initial position took place only in Bavarian, Alemannic and East Franconian, and /k/ > /kx/ in initial position only in Bavarian and Alemannic (König 2005:63). Although the voiceless stops could be said to constitute a natural class, they did not always pattern together. Crucially, though, the changes in each individual phoneme

occurred regularly in each dialect in the relevant contexts.²⁰ There is no need for ‘similar’ lexical items to pattern together in regular change, just as there is no need for ‘similar’ phonemes to pattern together.

This regularity gives us part of the wherewithal to establish correspondences in syntax, but not all of it. In section 2.1 I argued that it was desirable to accept the condition repeated here as (8):

(8) **Double Cognacy Condition:**

In order to form a correspondence set, the contexts in which postulated cognate sounds occur must themselves be cognate.

We have seen that sounds can be compared to lexical items. Drawing the analogy further, if we apply (8) to syntax then the contexts in which these lexical items occur - i.e. sentences - must be cognate. As argued in 1.2.1, however, sentences are characteristically *not* cognate, since they are formed rather than learned. We thus reach a point where phonological and syntactic change fail to be isomorphic.

There are two ways round this problem. Firstly, we could restrict our reconstruction to those sentences that *are* learned, such as formulae in verse traditions; however, identifying these is not an uncontroversial process, and the results we could achieve by this method would be limited. Secondly, and more promisingly, we could abandon (8). As with phonology, this would leave us without the mutually reinforcing evidence to demonstrate that the correspondences we hypothesise are correct; instead, positional and structural alternations must be taken as evidence in themselves. As with phonology, this is an undesirable move, but one that is in this case necessitated by the nature of syntactic transmission.

A further problem for this approach is presented by contact, particularly transfer²¹ between the languages under comparison. Lexical borrowing is known to be very common (Winford 2005:377), and in the comparative method can be ruled out by the fact that borrowed items typically fail to fit the system of regular sound correspondences established for the recipient language. Phonological transfer is perhaps rarer, although does occur: Dravidian influence caused Indic to develop retroflex consonants (Bowern 2008:204), and Armenian may have developed

²⁰ Dialect borrowings, of course, later obscured this clear picture.

²¹ I use the term ‘transfer’ here in a neutral sense to refer to any kind of cross-linguistic influence regardless of agentivity, following Winford (2005: 376).

ejectives as a result of influence from other Caucasian languages. The comparative method does not contain any way of identifying such transfer. But under the analogy we have been pursuing, the equivalent of phonological transfer in syntactic reconstruction is lexical transfer (involving functional items), and so our syntactic comparative method is unable to identify this. Independent methods do exist for identifying transfer: Bowerman (2008:208-10) discusses some. For example, ‘exotic’ constructions that are counterexamples to strong typological principles or exceptional within the language itself may be cases of transfer if a source can be identified (2008:209). This alleviates the problem, but we cannot be sure to identify all cases of transfer in this way. Like phonological transfer in traditional reconstruction, then, lexical transfer is an unavoidable confounding factor in a syntactic application of the comparative method, and may obscure the history of the languages involved, leading us to incorrectly reconstruct retention of a feature rather than innovation.

Thus far, difficulties in identifying genuine correspondences in the sense of cognacy have been discussed. How, then, do we establish such correspondences? As with phonology, surface formal similarity is not enough, although it is a useful criterion. Environmental alternations brought about by the regularity of syntactic change are key, where they exist; as in phonological reconstruction, these help to differentiate between similarity caused by genetic relationship and similarity due to other causes. Where overt phonetic material is present, as in the *casa(m)/chez* example, this may itself provide clues as to cognacy. A third heuristic, as in phonological reconstruction, is semantic similarity.

To sum up this section, it has been argued that the comparative method can indeed be used in syntactic reconstruction, up to a point, and that correspondences between (functional) lexical items can be established. The fact that the Double Cognacy Condition (8) cannot hold in syntax is a significant problem, as it means that our hypotheses about relatedness are less firm than those we can make in phonology. On the other hand, the regularity of syntactic change as argued for in this section allows us to use a large portion of the comparative method if we accept that it can be applied without the need for cognate sentences.

2.3.2 Postulating protoforms

Once correspondences have been established, the next stage is to postulate forms for the protolanguage. Three criteria are generally used within the comparative method:

directionality, synchronic typology, and majority rule. I will argue below that all are as applicable to syntax as they are to phonology.

The issue of directionality has been covered already (1.1.3, 1.2.4). To recapitulate: while our knowledge of common directions of change in syntax is less than in phonology, some have been established, especially within grammaticalisation. For example, if an item is an independent word in one language and an affix in another, we would generally reconstruct the item as an independent word for the protolanguage, since the progression from independent word to affix is more common than from affix to independent word. This sort of background knowledge can be used to guide reconstruction.

The same applies to synchronic typology (1.1.2). For instance, complementisers in VO languages are universally initial (Dryer 1992:102; Hawkins 1990:225). Extending this observation, it appears to be a robust typological generalisation that head-initial phrases must be dominated by head-initial phrases if their heads are non-distinct in categorial features (Biberauer, Newton & Sheehan 2008:19). It follows that we should be wary of positing a system that violates such apparent universals at any point during a language's history.

Majority rule is the most controversial of the three guiding criteria. Lightfoot (2002a) criticises the method of reconstruction presented in Roberts (1998), stating that 'traditional methods do not depend on majority rule' (2002a:117). Campbell and Harris (2002), however, defend Roberts's reconstruction. If we accept that all the languages under investigation are related to the protolanguage without intermediate stages, i.e. that they are not subgrouped in any way, then, all other things being equal,²² it is more desirable to posit change in a single language than in multiple languages, as parallel innovation is less likely than parallel retention (Campbell & Harris 2002:615). As Hale (2007:240) points out, the principle behind majority rule is 'adopt the hypothesis which posits the minimal number of changes to get the attested data'. Hale argues that it is dangerous to apply majority rule, since the change under investigation may itself be evidence for subgrouping: if three related languages show one variant and another related language shows another, it would be sufficient grounds for subgrouping the three languages together as against the fourth (2007:240-2). In cases where subgroupings have already been safely

²² Hale (2007: fn32) points out that all other things are not always equal, in the sense that majority rule should be viewed as a 'last resort', with directionality taking precedence where relevant.

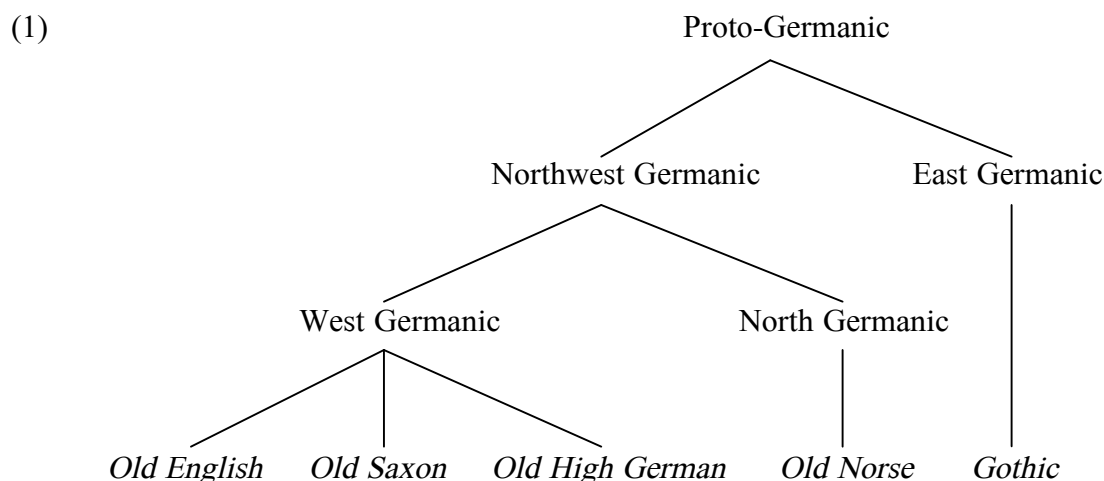
established, however, this is less of a problem. In Germanic, for example (see section 3), if Gothic, Old English and Old High German exhibit one form and Old Saxon and Old Norse exhibit another, we would reconstruct the form found in the first three, all other things being equal, since we have good evidence that these languages are not closely related to the exclusion of the other two.

Majority rule, then, alongside considerations of directionality and synchronic typology, is a tool we can use to reconstruct protoforms.

3. Case studies from Germanic

In this section I apply the method developed in the previous section to some concrete case studies from the Germanic languages, attempting to make inferences about the syntax of Proto-Germanic. The choice of a small language family with attested texts dated relatively close to the presumed age of the protolanguage is intentional: as Alice Harris (2008:90) observes, starting out with a language such as Proto-Indo-European, as many syntactic reconstructionists have done in the past, makes reconstruction considerably more difficult and speculative due to the time depth and relative separation of the major branches of the family.

Germanic, a sub-branch of Indo-European, is widely assumed to have an internal structure as in (1):



In the above, adapted from Harbert (2007:8), only the earliest stages of the family are shown, with attested languages in italics. The assumption of a Northwest Germanic node is not universally accepted; Krahe & Meid (1969:37-8), for example, prefer to assume an early Gotho-Nordic unity, as these languages share a number of features that set them apart from West Germanic. However, in line with most recent scholarship (e.g. Harbert 2007:7; Nielsen 2000:23), I take the tree given in (1) to be the most plausible; in any case, since the early Germanic varieties were geographically contiguous (Harbert 2007:8), it is likely that East Germanic and West Germanic both individually shared innovations with North Germanic, and that the binary branching tree above is essentially an artefact of the method.

With the exception of a small number of runic inscriptions, mostly attributed directly to Northwest Germanic (Antonsen 1975), Gothic is the earliest attested Germanic language, preserved mainly in sixth-century manuscripts of a fourth-century Bible translation. The majority of texts in the older West Germanic languages date from the last few centuries of the first millennium AD, while the extant Old Norse texts are mostly dated to the twelfth-thirteenth centuries AD. The fact that our earliest textual records are not contemporaneous is a confounding factor when carrying out direct comparisons; a further problem is the fact that many early texts are written in verse and/or are translations, which may have an effect on the syntax. For the method set out in section 2, which makes existential rather than universal claims about the features of protolanguages, this is less of a problem than for the typological methods of e.g. Lehmann (1974); as Lass (1997:68) puts it, ‘it is unlikely in principle ... that any device used in verse will be an absolute violation of the norms of non-verse language’, i.e. fully ungrammatical, and the same can be said of translations. These and other problems will therefore be left aside in the following sections, although this doubtless represents an oversimplification.

The sample reconstructions I present in 3.1-3.3 concern reflexive pronouns, infinitives and verb position, respectively.

3.1 Directionality in action: the Old Norse middle voice

In Old Norse texts a ‘middle voice’ verbal ending can be found, with reflexive, reciprocal and passive functions (Barnes 2004:146; Faarlund 2004:123-7). It mainly surfaces as *-sk*, although *-mk* is found in the first person and *-zk* in the second person plural (Eythórsson 1995:234).

- (2) Úlfrinn gapði ákafliga ok **fekksk** um mjök ok vildi bíta þá
 wolf.DEF gaped greatly and got.REFL about much and would bite them
 ‘The wolf gaped terribly and thrashed around and wanted to bite them’
 (Prose Edda, 34)

This ending has no obvious parallels in the other old Germanic languages: although Gothic has mediopassive verb forms, none of the phonological material is cognate. Furthermore, the Gothic forms only occur in the present indicative and subjunctive, with periphrastic constructions elsewhere (Wright 1910:191), while the Old Norse forms in *-sk* are also found in the past tense.

Synchronically it is sometimes analysed as a clitic (e.g. by Faarlund 2004, Eythórsson 1995) and sometimes as an affix (e.g. by Ottósson 2008). One argument for the clitic analysis is that *-sk* is best viewed as an exponent of a head of VoiceP along the lines of similar endings in languages like Lithuanian (Eythórsson 1995:238), but that VoiceP is thought to be below TP and yet *-sk* invariably occurs outside tense and subject agreement morphemes:

- (3) *kalla-ð-i-sk*
 V + T + AgrS // Voice (Eythórsson 1995:241)

If suffixal, this ending would violate the Mirror Principle (Baker 1985), according to which morphological derivations directly reflect syntactic derivations. Against this, however, it can be noted that *-sk* fails to meet at least three of the six criteria for clitichood proposed by Zwicky & Pullum (1983:503-5): it is highly selective in that it can only follow verbs, it triggers stem allomorphy as exemplified by the contrast between *kalla* ‘I call’ and *køllumk* ‘I am called/I call myself’, and it is treated as a unit with the verb with respect to syntactic operations in that it moves along with the verb to C (cf. section 3.3). I therefore analyse *-sk* as a suffix, but with the worrying proviso that it violates the Mirror Principle.

The other early Germanic languages had a third person reflexive pronoun with a phonologically similar shape, e.g. Gothic *sik*, Old High German *sih* (Wright 1910:123).

- (4) Jah gawandida **sik** Iēsus in mahtái ahmins in Galeilaian
 and turned REFL Jesus in power.DAT spirit.GEN to Galilee.ACC
 ‘Jesus returned in the power of the Spirit to Galilee’
 (Gothic Bible, Luke 4:14)

On the basis of both phonological and semantic criteria we can posit that the Old Norse *-sk* ending is cognate with this pronoun. The alternation between *-sk* and *-mk*, in particular, is indicative of this. Old Norse itself retains the pronoun:

- (5) Grettir lá kyrr ok hrærði **sik** hvergi
 Grettir lay quiet and moved REFL nowhere
 ‘Grettir lay quietly and did not move an inch’
 (Grettis Saga, 35)

Like the case of *casa(m)/chez* discussed in 2.3.1, we are dealing here with lexical split, or ‘layering’ in grammaticalisation terms (Hopper & Traugott 2003:124). Considerations of directionality lead us to posit a split, with the independent pronoun being the original form. The alternative would be to posit that the Old Norse *-sk* endings existed in Proto-Germanic, which would require their independent loss in both East and West Germanic. Furthermore, synchronic considerations help us rule out this option: since Gothic exhibits traces of a morphological passive with phonologically cognate forms in other Indo-European languages, we are led to reconstruct this for Proto-Germanic, and so reconstructing *-sk* endings for Proto-Germanic would require us to postulate that this language had two distinct morphological passives.

Formally the change can be characterised as a pronoun first Merged as the complement of VP being reanalysed as an affix first Merged higher in the clausal structure to which the verb then moves and left-adjoins (perhaps via a clitic stage). The environment for the reanalysis is simply string-adjacency; elsewhere, in all other positions, such an analysis is impossible and so the pronoun is retained.

In (5) the pronoun is directly postverbal, which would, of course, still be possible in a grammar that had retained both the pronoun and the new affix. Aside from orthographic differences (presence vs. absence of <i>), differences in interpretation rule out the possibility that *-sk*, *-zk* and *-mk* are simply enclitic pronouns: *þeir hittusk* ‘they met’ is possible with a reciprocal reading, whereas **þeir hittu sik* is never found, with *hverr annan* ‘each other’ being the only possibility if the *-sk* ending is not used (Eythórsson 1995:236).

This simple example of reconstruction, concerning the origin of *-sk*, has been accepted for over a hundred years (e.g. by Nygaard 1905). Since we are dealing with an example for which clear phonological evidence exists, a charge of triviality could be raised. However, it does weigh heavily against Lightfoot’s (2002a:120) contention that reconstruction of syntax is possible only in cases of identity, since in this example two different items descend from a single source. Furthermore, as noted earlier, the method sketched in 2.3 makes existential rather than universal claims, and as in phonology we are unlikely to be able to recover every aspect of the syntax of an unattested language. This case study shows, then, that reconstruction of some aspects of syntax can be straightforward as well as successful and widely accepted.

3.2 *The inflected infinitive*

Old English infinitives can appear in two forms: either as a ‘bare’ infinitive ending in -an, e.g. *unlustian* ‘to loathe’, or as the ‘inflected’ infinitive (Mitchell & Robinson 2007:36). The latter is found almost exclusively after *tō* (Callaway 1913:2), as in the example in (6):

- (6) þe forbēad Petre mid wǣpnum **tō winnenne** wið þā wæłhrēowan Iūdēiscan
 who forbade Peter with weapons to fight with the bloodthirsty Jews
 ‘who forbade Peter to fight the bloodthirsty Jews with weapons’
 (Ælfric’s Life of St Edmund, lines 204-5)

The inflected infinitive is also found in Old High German and Old Saxon, as in (7) and (8):

- (7) Óba iz ward iowánne \ in not **zi fehtanne**
 if it became ever \ him need to fight
 ‘If he were ever required to fight’
 (Old High German, Otfrid’s Ad Ludowicum, line 21)

- (8) Thô uuârun thea uuíson man \ fûsa **te faranne**
 then were the wise men \ ready to travel
 ‘then the wise men were ready to travel’
 (Old Saxon, Heliand, fitt 8, lines 649-50)

Only one form of the infinitive is found in Gothic (Wright 1910:134) and Old Norse (Faarlund 2004:122-3), which corresponds in both cases to the Old English bare infinitive:

- (9) jah þata **du frijōn** nehundjan swē sik silban
 and that to love neighbour.ACC as REFL self
 ‘and to love your neighbour as yourself’
 (Gothic Bible, Mark 12:33)
- (10) Hafi þit hér illan vanða upp tekit **at berja** saklausa menn beinum
 have you.DUAL here bad custom up taken to strike innocent men bone.DAT
 ‘You two have taken up a bad habit, hitting innocent men with bones.’
 (Old Norse, Hrólfs saga kraka, chapter 34, line 52)

The existence of both inflected and uninflected infinitive is thus restricted to West Germanic. The following discussion will concentrate on Old English, since this language has been most widely discussed in the literature and the patterns found in Old High German and Old Saxon are essentially identical.

The synchronic analysis of the inflected infinitive construction is disputed, with radically different analyses being proposed by different authors.²³ The key insight is captured by Anderson's (1993:14) remark that 'morphologically and syntactically, the Old English inflected infinitive is clearly more nominal than the uninflected'. On the other hand, Fischer et al. claim that 'the nominal character of Old English infinitives has been overestimated' (2000:62). A key indication of the nominal nature of the *tō* + inflected infinitive construction is that the *-enne* ending on the inflected infinitive is identical to the dative singular ending of neuter *ja*-stem nouns in Old English, e.g. *wēstene* 'waste, desert (dat.)'. Since the preposition *tō* is otherwise always followed by a noun in the dative case, a link between the two constructions is straightforwardly visible. The question, then, is how far the parallel can be extended.

Los (1998) argues that *tō* + inflected infinitive is a clause-like element, based on parallels with *that*-clauses. Like Old English *that*-clauses, *tō* + inflected infinitive always follows the finite verb (297 examples in the Toronto Corpus with only one preceding), whereas the bare infinitive may follow or precede, and 'this "heaviness" suggests that *to*-infinitives should be analysed as representing a higher clausal level than the bare infinitive' (1998:12-13). Furthermore, she claims that *to*-infinitives 'refer to a durative, interruptable event that can be temporally segmented' (1998:16), like *that*-clauses and unlike bare infinitive complements. Following this, Roberts & Roussou (2003:105) suggest that *tō* + inflected infinitive in Old English was selected by a verb as a clause (CP) with a null C, with *tō* occupying the head of a functional projection MP between CP and TP.

Jarad (2003) argues against a clausal analysis of *to*-infinitives, his main observation being that they can occur in coordination with ordinary PPs, at least where the preposition is *tō*:

²³ Much of the literature has attempted to account for the distributional differences between the inflected infinitive and the uninflected infinitive (e.g. Callaway 1913) or *that*-clauses (Los 1998); I largely leave this important question aside here.

- (11) ut eode to his gebede oððe to leornianne mid his geferum
 out went to his prayer or to learn with his comrades
 ‘He went out to prayer or to study with his comrades’
 (Bede’s Ecclesiastical History, book 3:3) (Jarad 2003)

It does not seem plausible to assume that a CP and a PP could be conjoined in this way. Such an analysis also explains the *-enne* ending on the infinitive, which in the accounts of Roberts & Roussou and Los is simply a relic with no synchronic function: ‘the *-ne* ending was no longer a true dative inflection’ (Roberts & Roussou 2003:109). It is clearly desirable to account for this ending within the synchronic system of Old English, as was recognised by Lightfoot (1979a). Los’s argument that *to*-infinitives pattern with *that*-clauses in terms of position relative to the verb is not a strong one, since principles of heaviness are likely to exist independently, rooted in performance (cf. Newmeyer 1998:ch.4 and Hawkins 2004 for discussion) or third factor considerations, and it is a trivial observation that *to*-infinitives are ‘heavier’ than bare infinitives. I will therefore accept the analysis of *tō* as a head of PP, while recognising that some problems still exist for this analysis.²⁴

The relevant relationship of cognacy we can postulate is between Old English *tō* in this construction and its equivalents in the other Germanic languages (e.g. Gothic *du*, Old Saxon *te*, Old High German *zi*), if we assume that the status of this element is responsible for the presence or absence of the *-enne* ending on the infinitive. Since, as we have seen, the ‘prepositional infinitive’ construction exists in all the older Germanic languages, we can reasonably posit some version of this for Proto-Germanic. Furthermore, the inflected infinitive itself can be reconstructed for Proto-West-Germanic, since it occurs in all these languages. The remaining unanswered question is whether the infinitive was inflected in Proto-Germanic in this construction.

Under traditional assumptions, infinitives in Proto-Indo-European were ‘petrified’ nouns of action (Callaway 1913:1). The uninflected infinitive, in this view, was derived from the nominative/accusative form of a neuter verbal noun, and the inflected infinitive from the dative form. If we take this stance, the split between inflected and uninflected infinitive can be reconstructed for Proto-Germanic much

²⁴ For instance, extraction from PPs is generally impossible in Old English (Jarad 2003: fn17), but objects of *to*-infinitives may precede *tō*. Similar problems apply, however, to an approach that equates *to*-infinitives with *that*-clauses, since leftward movement of complements from *that*-clauses is also impossible; cf. Los (1998: fn9); Roberts & Roussou (2003: fn12).

as it is in Old English. This leads us to posit the loss of the infinitive inflection, perhaps by analogy with the uninflected infinitive, in both North and East Germanic. The loss of such endings is not an uncommon type of change; however, one argument against this is that Gothic has otherwise been extremely conservative in retaining its inflectional endings, including the *ja*-stem nominal endings with which the prepositional infinitive inflection patterns in West Germanic.

An alternative view, which to my knowledge has not been expressed in the literature, is that the inflected infinitive in West Germanic is an innovation by analogy with *ja*-stem nouns. This would require the postulation of a single change: a surface reanalysis in West Germanic as in (12). I follow Pesetsky (1995) and Marantz (1997) here in assuming that the infinitival root is itself acategorial.²⁵ **to* is the reconstructed phonological form for Proto-West-Germanic.

$$(12) \quad [{}_{\text{TOP}} *to [{}_{\text{VP}} v [\sqrt{\text{ROOT}}]]] > [{}_{\text{PP}} *to [{}_{\text{DP}} D [{}_{\text{NP}} n [\sqrt{\text{ROOT}}]]]]$$

If we follow Distributed Morphology analyses such as that of Embick & Noyer (2006:307) in assuming that the declension of a noun is a feature inserted at PF (since it is irrelevant to the syntax), we can posit that the reanalysis included the assignment of *ja*-stem status to the Vocabulary Items associated with the relevant roots.

In this case, then, considerations of directionality cannot help us, and majority rule is of little use. We must therefore continue to entertain both of the two options for Proto-Germanic, just as we would have to in a similar situation in phonological reconstruction.

3.3 Word order: *V-to-C movement in early Germanic*

The previous two case studies have involved elements with some phonological realisation. In this final case study I will examine a word-order phenomenon that, under the theory I am adopting, must be triggered by phonologically null functional items.

²⁵ I leave the exact nature of ToP open: it may have been the same as Roberts & Roussou's (2003) MP, above TP, or it may itself have been TP or CP. This analysis does not preclude a reanalysis of PP as ToP as in Roberts & Roussou's (56) (2003:105) having taken place at an earlier stage, which may be necessary for independent reasons to explain the emergence of the prepositional infinitive construction from verbal nouns between Proto-Indo-European and Proto-Germanic.

Since the 1970s, under the influence of Greenberg's (1963) notion of basic-order typology, it has commonly been held (e.g. by Hopper 1975:38; Lehmann 1972:243; Kiparsky 1995:152, 1996:140) that Proto-Germanic was essentially verb-final. While there is likely to be some truth to this claim, I argue, on the basis of recent work by Eythórsson (1995, 1996), Ferraresi (2005) and Axel (2007), that this concentration on 'basic' or 'unmarked' word order has led to the neglect of some interesting contextually grounded word-order alternations, some of which can be reconstructed for Proto-Germanic.²⁶ In particular, all the early Germanic languages exhibited V-to-C movement to some degree (Eythórsson 1995:333), and I explore the extent to which this can be reconstructed for Proto-Germanic. In the following I briefly describe the situation in each of the early Germanic languages before presenting an analysis and discussion of the possibilities for the protolanguage. I do not discuss Old Saxon separately from Old High German due to the lack of secondary literature on the former.

3.3.1 Runic

The early Germanic runic inscriptions are generally held to be OV, largely on the basis of the famous and oft-cited Gallehus inscription:

- (13) ek hlewagastiz holtijaz horna tawido
 I Hlewagastiz Holtijaz horn made
 'I, Hlewagastiz Holtijaz, made this horn.'

However, they are not uniformly so. Eythórsson (1995) found several examples of verb-second and verb-initial orders:

- (14) ek hagustadaz hlaaiwido magu minino
 I Hagustadaz buried son my
 'I, Hagustadaz, buried my son.'
 (Kjølevik stone) (Eythórsson 1995:182)

²⁶ The same criticism cannot be made of earlier scholars, who were well aware of the variation existing in early Germanic. Kiparsky (1995: 152) cites Delbrück (1878) as authority for his suggestion that Proto-Germanic was verb-final, yet Delbrück later changed his view to admit the possibility of Wackernagel's law holding in the protolanguage; cf. Hopper (1975: 15-16).

- (15) wate hali hino horna
 wet stone this horn
 ‘Let the horn wet this stone!’
 (Strøm whetstone) (Eythórsson 1995:183)

(14) seems to be an example where a constituent has been fronted, and (15) an imperative. Instances of what appear to be V-to-C movement on a parallel with modern Germanic languages such as German can thus already be seen in Runic.

3.3.2 Old Norse

Of all the older Germanic languages, Old Norse is the most robustly V2; with the exception of the earliest verse texts, the verb immediately follows the first constituent in both main and subordinate clauses (Nygaard 1905; Eythórsson 1995:189; Faarlund 2004:191).

- (16) Nú skaltu drekka blóð dýrsins
 now shall.2SG drink blood beast.DEF.GEN
 ‘Now you shall drink the beast’s blood.’
 (Hrólfs saga kraka, chapter 34, line 101)

The only exception to this pattern are: clauses with negated initial verbs (Eythórsson 1995:258-261); conditional clauses lacking an overt complementiser (Faarlund 2004:252), in which the verb is also initial; and other (main or subordinate) clauses in which the verb is initial, a construction which Eythórsson (1995:249-50) dubs Narrative Inversion.

3.3.3 Gothic

The picture in Gothic is not as straightforward. Kiparsky (1995:162) asserts that V-to-C movement is absent from Gothic, something which Eythórsson (1995) has shown to be false under standard assumptions, since the verb is in second position in *wh*-questions²⁷ (1995:25) and is in initial position in negative clauses and imperatives (1995:22-24):

²⁷ Fuß (2003: 199) argues that the few exceptions to this, in which pronouns intervene between the *wh*-element and the finite verb, are word-for-word translations of the Greek *Vorlage* and ‘do not tell us anything about the syntax of Gothic’. This is problematic in that we must assume that these examples are fully ungrammatical in Gothic if we do not wish to posit this pattern as a native one. Furthermore, Axel (2007: 244-5) has shown that

- (17) wairþ hrains
 become clean
 ‘be cleansed’
 (Gothic Bible, Matthew 8:3)

V1 narrative inversion may also occur in Gothic:

- (18) Usgeisnodedun þan allai þai hausjandans is ana frodein
 were.amazed then all they hearers his in wisdom
 ‘Everyone who heard him was amazed at his wisdom’
 (Gothic Bible, Luke 2:47)

V2 was certainly not generalised in Gothic, however, and it is unclear whether there was an asymmetry in verb-placement between main and subordinate clauses: Eythórsson (1996:112-3) observes that verbs tend to precede verbal particles such as *inn* ‘in’ and *ut* ‘out’ in main clauses and follow them in subordinate clauses, but that there are exceptions both ways.

A further fact about verb movement in Gothic is that an asymmetry exists between clause-initial definite subjects and other clause-initial constituents (Eythórsson 1995:55-67). In clauses introduced by *iþ* ‘but’, the particle *-uh* ‘and’ is enclitic to the (second position) verb when a proper name or overt pronoun is clause-initial, as in (19), but when another constituent is clause-initial, as in (20), *-uh* cliticises to the head of that phrase.

- (19) iþ Iesus iddj-uh miþ im
 but Jesus went-uh with him
 ‘but Jesus went with him’
 (Gothic Bible, Luke 7:6)
- (20) sumai-h þan ize wildedun fahan ina
 some-uh þan of.them wanted seize him
 ‘and some of them wanted to seize him’
 (Gothic Bible, John 7:44)

parallel examples exist in Old High German where the order diverges from the Latin original. We can still posit some kind of V-to-C movement in such cases, however, as I will discuss in 3.3.6.

Eythórsson analyses *-uh* as first Merged in C, and hence takes this as evidence that the verb only moves to C when the fronted constituent is a definite subject.²⁸

3.3.4 Old English

Since van Kemenade (1987) first approached the topic, verb movement in Old English has garnered more attention in the generative literature than verb movement in all the other Germanic languages put together. In this language, as in Gothic and Old Norse, the verb was found in initial position in imperatives, direct questions and narrative inversion (Fischer et al. 2000:106-7). In *wh*-questions and with fronted negation, the verb is always in second position (Fuß 2003:208). With regard to other fronted constituents, the position of the verb is more variable. Full subject DPs usually follow the verb when another constituent is fronted, but V3 orders are often found with a subject pronoun intervening between the fronted constituent and the verb:

- (21) *Ēasteward hit mæg bion syxtig mila brād*
 eastward it may be sixty miles broad
 (Orosius, Voyage of Ohthere) (Harbert 2007:408)

However, there are exceptions to both of these patterns: subject pronouns may follow the verb, and full subject DPs may precede it (Fischer et al. 2000:108). Furthermore, object pronouns are also found between fronted constituents and the verb:

- (22) *on þæt stēorbord him bið ærest Īraland*
 on the starboard to-him will-be first Ireland
 (Orosius, Voyage of Ohthere) (Harbert 2007:408)

In general, the finite verb occurs in second or third position in main clauses about 95% of the time according to Fischer et al. (2000:109). Verb-final main clauses are also possible in Old English. An asymmetry between main and subordinate clauses can be observed, since non-verb-final orders only occur about 35% of the time in subordinate clauses; furthermore, in these clauses the nominative subject always precedes the finite verb except in ‘special constructions such as passives’ (Fischer et

²⁸ *-uh* has no cognates within Germanic, but is cognate with Latin *que* (Ferraresi 2005: 155).

al. 2000:109), which indicates that V-to-C movement did not take place in subordinate clauses.

The relevant distinction may in fact not be between main and subordinate clauses but between matrix and ‘bound’ clauses, since coordinated main clauses often pattern with subordinate clauses with regard to verb movement: clauses beginning with *ond* or *ac* ‘and’ are often verb-final (Mitchell 1985:694). I will return to this issue, which may have greater validity across the older Germanic languages, in 3.3.6.

3.3.5 Old High German

Axel (2007) shows for Old High German that direct interrogatives were verb-initial and *wh*-interrogatives were verb-second, except when preceded by an interrogative particle²⁹ (2007:52-4), that imperatives were verb-initial (2007:56-61) and that negated clauses were negated-verb-initial (2007:62). Furthermore, V2 is the norm in all matrix declarative clauses, as in (23).

- (23) Chindh uirdit uns chiboran
 child becomes us.DAT born
 ‘a child will be born to us’
 (Isidor translation, line 385) (Axel 2007:63)

Verb-initial declaratives (Narrative Inversion) are also found:

- (24) gieng thô zuo ther costari
 went *thô* to the.NOM tempter
 ‘The tempter went to him’
 (Tatian, 113.28) (Axel 2007:113)

As in Old English, V3 orders are also found, albeit marginally and only in older texts (Axel 2007:201). Many can be argued to be parallel to other surface violations of V2 such as German Left Dislocation, where a left dislocated element is followed by a resumptive pronoun before the verb:

²⁹Axel (2007: 208) suggests that in a framework assuming Rizzi’s (1997) articulated CP proposal these clause-type particles could be viewed as exponents of the head of ForceP.

- (25) thie morganelihho tág / ther bisuorg sih selbo
 the tomorrow-ADJ day-NOM / that-NOM worries REFL self
 ‘for tomorrow will look after itself’
 (Tatian, 157.14) (Axel 2007:204)

Examples parallel to (21), in which a pronoun intervenes between the fronted constituent and the finite verb, also exist:

- (26) Erino portun ih firchnussu
 iron portals I destroy
 ‘I will destroy iron portals’
 (Isidor, line 157) (Axel 2007:237)

Axel argues that such pronouns, which can be either subject or object pronouns, cannot be viewed as syntactic clitics (2007:254-277). No ‘full’ DPs are attested in second position before verbs (2007:287), but, since the corpus of Old High German is considerably smaller than that of Old English, it is possible that this could be a coincidental non-attestation rather than ungrammaticality.

3.3.6 Analysis

The results of the above sections with regard to the occurrence or non-occurrence of V-to-C movement in different contexts are presented in the table below.

	Direct questions	<i>wh</i> -questions	Neg-initial	Imperatives	Narrative Inversion	XP-fronting	Matrix declar.	Subord. clauses
Runic	?	?	?	yes	?	yes	no	?
ON	yes	yes	yes	yes	yes	yes	yes	yes
Gothic	yes	yes	yes	yes	yes	yes*	no	no
OE	yes	yes	yes	yes	yes	yes	yes	no
OHG	yes	yes	yes	yes	yes	yes	yes	no

In Gothic, as discussed in 3.3.3, V-to-C movement in cases of XP-fronting only occurs where a definite subject has been fronted.

For my analysis of early Germanic I will adopt Rizzi’s (1997) split CP proposal, according to which the cartography of the clausal left periphery is as in (27):

(27) ForceP > TopP* > FocP > TopP* > FinP (Rizzi 1997:297)

In contrast to earlier work (e.g. Eythórsson 1995), Rizzi draws attention to a difference between fronting involving focus, presenting new information, and topic, expressing old, shared information (1997:285), which are sharply distinguished in many languages. The relevance of this will become clear when we look at Old English.

The first observation to be made from the table is that, setting aside Runic for which our data is limited, the first five columns show identity between all the early Germanic languages. The first four of these contexts - yes/no and *wh*-questions, negative-initial clauses and commands - are referred to by Fuß (2003:196) and Axel (2007:52) as the ‘historical core’ of verb-movement. They can be analysed in a similar way to these constructions in Modern English, with a phonetically null head of the C-system (potentially of FocP) bearing a feature attracting the verb to adjoin with it via head-movement, among other things. The fifth context, Narrative Inversion, is similar: it can be analysed as a phonetically null declarative Foc exponent bearing a feature attracting the verb, with the interpretive effect of focalising the action. I hypothesise - uncontroversially, I think - that these heads, which display similar behaviour in all early Germanic languages, are cognate; therefore, under diachronic minimalist assumptions (cf. 2.2), we are led to reconstruct V-to-Foc movement in these five constructions for Proto-Germanic.

It can also be argued that V-to-C movement with XP-fronting is a common Germanic pattern. Parallel to the cases above, I suggest that the relevant type of XP-fronting is focalisation. The restriction to definite subjects in Gothic can either be seen as a development specific to Gothic or as the original Proto-Germanic pattern.

Old Norse and the West Germanic languages display generalised V-to-C movement in matrix declaratives.³⁰ However, this is not true of Gothic and Runic, and so, if we view Runic as representing an early stage of Northwest Germanic and hypothesise that the declarative exponents of the head of FinP in all these languages are cognate, the simplest option is to assume that this phonetically null head of Fin developed a feature requiring the finite verb to move and adjoin to it in late Northwest

³⁰ As the discussion in 3.3.1-3.3.5 should have made clear, this is an oversimplification, as V-final orders are also attested at least in Old English. I leave these unexplained here, and it remains to be seen whether a principled account for these can be found, perhaps in terms of formal optionality (c.f. Biberauer & Richards 2006; Biberauer & Roberts 2005).

Germanic, perhaps by analogy with other heads of the C-system and/or through reanalysis of clauses with focalised subjects as ordinary declaratives. This gives us the asymmetry between matrix and subordinate clauses ‘for free’ if we assume that the relevant complementisers were also exponents of Fin. Old Norse is the only language with consistent surface V2 in subordinate clauses: this may have arisen through reanalysis of complementisers as exponents of Force, or through an independent change triggering generalised V-to-T movement, depending on one’s analysis of the data.

What about the behaviour of coordinate clauses in Old English? Campbell (1970:fn4) claims that ‘even co-ordinating conjunctions are syntactically subordinating’. Kiparsky (1995:148-9) argues against this, since other orders, such as verb-initial order and even V2 with *wh*-phrases, can also be found in coordinate clauses. I propose that, like Modern English *and*, which is able to coordinate a variety of phrase types, Old English *ond* and *ac* were able to coordinate TPs as well as full CPs. There is in fact evidence that this property of *ond* can be reconstructed to an earlier stage of Germanic, as suggested by Behaghel (1932). Old High German *unti* shows the same behaviour; although Axel (2007:77-9) argues that the pattern is rare, Kuhn (1933) gives a number of examples. Kuhn also shows that in the early Old Norse *dróttkvætt* a similar pattern of verb-finality obtains among coordinate clauses (1933:54), although there is no conjunction cognate with *ond*. I therefore suggest that the ability of this and similar conjunctions to coordinate TPs as well as CPs can be reconstructed at least to Proto-Northwest-Germanic.

The remaining problem that needs addressing is the possibility of V3 in Old English and Old High German. Different authors have proposed different solutions to this problem. One common approach is to posit generalised V-to-T rather than V-to-C movement in matrix clauses (e.g. Fuß 2003). This allows for the possibility of fronting one constituent to SpecCP and another to SpecTP over the finite verb. The problem is then to explain why movement to SpecTP sometimes occurs but does not always, and in particular why object pronouns can move to SpecTP. Fuß has to consider clauses with full nominal subjects intervening between an XP and the verb to be the product of a different grammar (2003:220), and essentially has to stipulate that object pronouns can also move to this position (2003:fn22).

An alternative approach is to attempt to derive the facts from a split CP analysis (cf. Roberts 1996 for one such, which differs slightly from what I adopt here). Fuß (2003:220) rejects this solution as ‘not very enlightening’, but in fact the relevant

facts fall out neatly from the assumption that a TopP could intervene between FocP and FinP. Given that topics ‘normally express old information, somehow available and salient in previous discourse’ (Rizzi 1997), such an analysis would predict that intervening elements in V3 constructions would all be of this nature.³¹ Pronouns, which are the element most often found in this position, fit this description almost by definition. At a first glance, it appears that this prediction also holds for full nominals when they are found in this position in Old English:

- (28) Nu **ealle** **ðas** **ðing** synd mid anum naman genemnode, gesceaft
 now all these things are with one.DAT name named creature
 ‘Now all these things are called by one name: creature.’
 (Ælfric’s Homilies I, 20. 276.10) (Fischer et al. 2000:108)

This pattern could have arisen in West Germanic through reanalysis of the first constituent of V2 matrix clauses as a topic; a similar change is posited in Middle Welsh by Willis (1998). Further research is needed to test this hypothesis, however.

Summing up, I have suggested that Proto-Germanic exhibited V-to-Foc movement in direct questions, *wh*-questions, imperatives, negative-initial clauses and Narrative Inversion declaratives, and with at least one type of XP-fronting. Generalised V-to-Fin movement, I have hypothesised, was an innovation in late Northwest Germanic.

³¹ This interpretive distinction, if correct, means that this is not merely a notational variant of the SpecTP analysis.

4. Conclusion

In section 1 I offered a critical review of the literature on syntactic reconstruction, surveying traditional, typological, grammaticalisation-based and parametric approaches. I argued that none of these provided as firm a footing for reconstruction as the comparative method in phonology, but proceeded to argue that most purported obstacles to syntactic reconstruction are overblown.

Section 2 considered the comparative method in detail and explored the isomorphism between syntactic and phonological change in a framework which takes the features of exponents of functional heads to be the locus of syntactic variation following Borer (1984) and Chomsky (1995). In particular, syntactic change was argued to be regular in a comparable sense to sound change. It was concluded that the isomorphism holds up relatively well under close scrutiny, although the non-transmission of sentences is a significant point where the parallel cannot be maintained.

Finally, in section 3 I applied the approach developed in the previous section, which can be considered an application of the comparative method to syntax as far as is possible, to three case studies in Germanic. Although the postulated Proto-Germanic lexical items are hypothetical, like any reconstruction, I hope to have shown that there are principled arguments to be made in favour of establishing correspondences and reconstructing certain ‘forms’ in these cases. In particular, I argued, with Eythórsson (1995), that some form of V-to-C movement must be assumed even for the earliest stages of Germanic, and hence for Proto-Germanic. This goes against the common view that V-to-C movement was a late, language-specific innovation (as expressed e.g. by Kiparsky (1995:159-62)).

The main contribution of this dissertation has been to offer a ‘deconstruction of reconstruction’: a detailed discussion of the parallels, or lack thereof, between syntactic and phonological reconstruction, given traditional assumptions about sound change and a plausible model of syntactic variation. It is difficult to know whether my findings should lead one to be more optimistic about syntactic reconstruction or less. Certainly it is the case that the non-transmission of sentences means that it is difficult to create a coherent system of correspondences in the same way as in phonology, which is potentially an argument against the view, expressed by Watkins (1964:1035), that ‘the “tractability” of the syntactic system for

historical investigations is only in degree different from that of the phonological'. On the other hand, I hope to have shown that extreme pessimism of the kind represented by Lightfoot (2002a) is unwarranted: hypotheses about diachronic identity and about protoforms can be made in syntax much as in phonology, although on a less safe footing. In essence, I agree with the spirit, though not the implementation, of Harris & Campbell (1995) and Harris (2008).

Much work remains to be done in this area, largely in applying the methods sketched above to different cases and proceeding gradually towards the reconstruction of more archaic languages such as Proto-Indo-European. Furthermore, our inventory of working hypotheses about directionality in syntactic change is much emptier than that for sound change, and this is another angle for future research. Although I believe these methods are sound, it may be some time before Lightfoot can eat his pudding.

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