OV-VO in English and the role of case-marking in word order

ABSTRACT

It is shown that the connection posited by Roberts (1997) between the loss of case marking and the shift from OV to VO in English is contradicted by the facts of English and the other Germanic languages. It is argued that this failure is not the result of an incorrect handling of the syntactic or morphological details, but of the way that the proposal straddles the syntax-morphology interface. Parallels are explored with research on verb-raising and agreement, and it is proposed that, in order to be more than a descriptive generalization, the claim that some morphological property has a syntactic effect must be couched in terms that both the morphology and the syntax can refer to in a principled way.

1 INTRODUCTION

There is a long tradition in work on historical syntax of drawing causal links between the loss of rich inflectional morphology and changes in word order. Some well-known examples are tying V-to-I movement and V2 to verbal tense/agreement morphology and the availability of scrambling and other optional order-changing operations to case-marking. Roberts (1997) brings this strategy to an analysis of the change from OV to VO in the history of English based around two innovative ideas. First, he adopts Kayne's (1994) theory of antisymmetry, according to which all phrases are underlyingly head-initial. This implies that all head-final surface orders must be derived by movement processes, including the OV orders of Old English (henceforth OE). Roberts implements this in early Minimalist terms as raising of the object past the verb to Spec-Agr_OP. Such a proposal constitutes a significant departure from previous theories of historical English phrase structure, e.g. Lightfoot (1979), van Kemenade (1987) and Pintzuk (1991), as the change from OV to VO is now analyzed as the loss of object movement, rather than a change in the directionality of the VP.

The second crucial idea put forward in Roberts (1997) is that the object movement process that derives OV orders is linked to rich case-marking. Recall that in early Minimalism, raising to Spec-Agr_OP was driven by the need to check object Case, and thus obligatory in all languages. However, it could either take place overtly, if the Case features involved are strong, or covertly if they are weak. Roberts proposes that Case-feature strength is a parameter, and it is set during the acquisition process

partly on the basis of the morphology. Like other movement-driving features, its default value is weak,[1] but the acquisition of its strong value can be triggered in two different ways: by the existence of rich morphological case, or by the exposure to sentence types with unambiguous syntactic evidence for overt movement. This implies that the the shift from OV to VO order in English depended on breakdown of the OE case system. It eliminated the morphological trigger for the acquisition of overt object raising, and subsequently the preponderance of surface VO orders resulting from verb raising and other movement processes obscured the syntactic trigger. As a result, learners began to acquire the default weak Case feature, and the remaining surface OV orders disappeared.

While Roberts' test of antisymmetry on the complex word-order patterns of OE is interesting and worth pursuing further, I will argue in this paper that the causal connection he posits between case-marking and OV word-order cannot be correct (see Biberauer and Roberts, 2005, for an antisymmetric account of OE word order that does not assume this connection). In Section 2 I will reexamine data from the history of English. I will show that the rise of VO orders and the loss of case-marking do not track each other in the way that Roberts' theory predicts, and that he is forced to posit unduly complex movement operations in order to maintain the link from case-marking to object-raising. In Section 3 I will turn to the other Germanic languages and demonstrate that this link fares even worse when considered in a comparative light. However, my real goal in this article is not just to argue against Roberts' paper. To simply abandon a theory such as his, which attempts to explain the interaction between certain morphological and syntactic facts, would be a step backward. Rather, we should investigate why it is that the theory fails, in order to get an idea of what a better theory should look like. What I will argue, in Section 4, is that Roberts' account of OV/VO word order and case-marking fails because it posits an interaction between the syntax and the morphology that is in no way constrained by a theory of the interface. I will discuss parallels from the more extensive work on V-to-I raising and verb agreement, in particular Bobaljik (2002), and what it can tell us about drawing principled connections between syntactic and morphological facts.

2 COUNTER-EVIDENCE FROM ENGLISH

There is plenty of evidence that superficially contradicts an implicational relationship between rich case-marking and OV word-order within the history of English. OV orders persist at a rate of roughly 25% in

early Middle English (Kroch & Taylor, 2000) and can be found with some regularity as late as the 15th century (van der Wurff, 1997, 1999), even though the case system was reduced to roughly its current complexity in nearly the whole of England by the mid13th century (Allen, 1995).[2] However, as van der Wurff (1999) shows, the latter sentence types are best explained in terms of leftward movement of negative and quantified objects and object topicalization in subjectless clauses. This means they are not the same phenomenon as the more general OV orders that Roberts is trying to explain. Furthermore, given the nuanced relationship between case and word-order intermediated by language acquisition that Roberts proposes, the earlier examples are not necessarily problematic either. His theory does not predict an immediate shift to VO upon the loss of case-marking, because morphological case is not the only trigger available for the acquisition of strong Case features. Given sufficient syntactic evidence, object raising can still to be acquired, and OV order can hang on, potentially indefinitely.[3]

We thus have to think more carefully about what would count as evidence for or against Roberts' theory. Since OV orders are derived due to a feature that can be acquired either morphologically or on the basis of OV orders themselves, their presence tells us nothing.[4] VO orders, however, are a different matter. Since rich morphological case triggers the acquisition of strong Case features, no case-rich language should lack overt object movement. This does not categorically rule out surface VO word orders, since it is always possible that the verb has raised past the derived position of the object in Spec-Agr_OP. However, given the predominance of OV orders in OE, at least with non-finite verbs, Roberts himself must assume that verb movement was not general in the language, thus for those sentences with surface VO order, the postulation of verb movement must be justified. What I will argue here is that while this is possible for certain OE VO sentence types, a residue remains for which a failure of overt object movement is the most attractive analysis. If this is correct, and if these orders show up at a time when the case-marking system was still intact, then we will have evidence against Roberts' theory. For clarity and concreteness I will adopt the terminology associated with Roberts' uniformly head-initial analysis, but I will not be arguing for or against it as opposed to an analysis that admits head-final structures, and my discussion could be recast in terms of the latter.[5] I will thus leave aside the word-order data that only bear on the question of underlying head directionality and restrict my attention to those that are relevant to the connection with case-marking.

As is well known, OE was a verb-second language. That is, in many clause-types, the finite verb raised to a high position in the clause, above the surface subject position, with a position above it for a topicalized phrase (see e.g. Kroch & Taylor, 1997).[6] Because this position is also higher than the landing site for objects in Spec-Agr$_O$P, finite verb-object orders tell us nothing about whether the object has raised. Non-finite verb forms, however, do not participate in V2, so in clauses with a finite auxiliary, non-finite main verb and object DP we expect the order AUX-O-V. Indeed, there are many examples of this kind:[7]

(1) a. he ne mæg his agene aberan (CP, 52.2)
he NEG can his own supported
'He cannot support his own.'

b. þæt hi mihton heora fynd oferwinnan
so-that they could their foes overcome
'...so that they could overcome their foes.'

However, as shown by Pintzuk (2002a,b), there is no shortage of sentences with a finite auxiliary where the object comes after the non-finite main verb:

(2) a. þu hafast gecoren þone wer (ApT 23.1)
you have chosen the man
'You have chosen the man.'

b. þæt he mot ehtan godra manna (WHom, 130.37-38)
that he might persecute good men
'... that he might persecute good men ...'

Roberts proposes two possible analyses for such orders. According to one, the object raises to the Specifier of an Agr$_O$P above the VP headed by the main verb, but below the auxiliary. The main verb then raises past the object and adjoins to the finite auxiliary. However, as Roberts himself points out in a footnote, we would expect head-movement of this kind to place the non-finite verb **before** the auxiliary. We could obtain the order in (2) if the main verb adjoined on the right side of the auxiliary, but right-adjunction is inconsistent with Kaynean antisymmetry. Instead, Roberts suggests that we have normal head movement with left-adjunction, and that subsequently the auxiliary excorporates from the

complex head and moves on to a higher Agr_O. The relevant aspects of this derivation (abstracting away from what the subject does), are shown in (3), where a hyphen represents adjunction:[8]

(3) ... [$_{AgroP}$ hafast$_i$-Agr_O [$_{VP}$ [gecoren$_j$-Agr_O]$_k$-t$_i$ [$_{AgroP}$ [$_{DP}$ þone wer$_l$] t$_k$ [$_{VP}$ t$_j$ t$_l$]]]]]

The complexity of this proposal is a high price to pay for avoiding the conclusion that the object has failed to raise. First and foremost, excorporation is explicitly ruled out in standard theories of head movement.[9] Second, the assumption of two Agr_OPs seems ill-justified in a mono-transitive clause. While it may be reasonable to posit an Agr_O above the auxiliary in sentences where the object raises past both verb forms, it seems odd to do so here where the object is accommodated in a lower position. The auxiliary itself is, after all, not transitive. The higher Agr_O provides not just a landing site, but also a justification for why the final instance of head-movement occurs, since for Roberts OE finite verbs always raise to Agr_O. Nonetheless, it does not provide an explanation for why the auxiliary would excorporate before undergoing this movement. Note that it cannot be because of a restriction against the entire complex head containing the main verb and the auxiliary moving together, since Roberts needs just this analysis for V-Aux-O orders like in (4):

(4) he þæs habban sceal ece edlean on Godes rice. (WHom 164.164-5)
he thus have must eternal reward in God's kingdom
'...he therefore must have eternal reward in God's kingdom.'

Roberts' analysis of Aux-V-O orders becomes even less plausible when we consider sentences with the same order and an additional auxiliary verb, like (5), from the *York-Toronto-Helsinki Parsed Corpus of Old English Prose* (Taylor et al., 2003, henceforth YCOE):[10]

(5) ðæt fremde ne scolden beon gefyllede ures mægenes (cocura,CP:36.249.25.1639)
that strangers ne should be filled our.GEN resource.GEN
'that strangers should not be filled with our resources'

If we want to claim that *ures mægenes* has raised to Spec-Agr_O, then *gefyllede* will have to have adjoined to *beon*, with the latter subsequently excorporating and raising to yet another Agr_O. On top of the further proliferation of Agr_O heads that it implies, this last movement step cannot be explained in the same way as in sentences with only one auxiliary. Roberts assumes obligatory raising of **finite** verbs to Agr_O, a version of V-to-I movement, but the auxiliary in question here is non-finite. Of course, an additional optional

movement process for non-finite verb forms could be posited in such instances, but unconstrained invocation of new movements weakens the theory considerably. It would be far simpler to allow that the object has failed to raise in these sentences.

This is in fact the other analysis that Roberts entertains. He suggests that in at least some VO sentences the object escapes the requirement to raise to Spec-Agr_O by being focused. This is presented as a reinterpretation in Kaynean terms of the idea that focus and heaviness facilitate extraposition. However, Roberts offers no ideas on how it is that focus can exempt a DP from the requirements of Case theory. One might imagine that the object actually raises to a special focus position, but to address the Case issue this would have to be located higher than Spec-Agr_OP so that the object could handle its licensing on the way. This would again put the object above the main verb though, and thus would bring us back to the situation that led Roberts to adopt the excorporation analysis.

What is more, there is reason to doubt that all post-verbal objects in OE can be analyzed in terms of extraposition or its antisymmetric equivalent. For one thing, minimally light elements like pronominal objects, verbal particles and stranded prepositions occur post-verbally at low but non-trivial rates. For example Pintzuk (2002a) finds that in the clauses in her corpus with a pronominal object and an auxiliary verb in medial position, the object comes after the non-finite main verb 10 times out of 139. She does not give information about the temporal distribution of these examples,[11] but sentences of this type can be found in the YCOE in texts written in the first half of the OE period (see also Koopman, 2005). Consider for example (6a) from Boethius' *Consolation of Philosophy* and (6b) from the *Orosius*, both written in the late 9th century and preserved in manuscripts from the early to mid 10th century:

(6) a. Gif ge wise beoð & gode, he wile folgian **eow** (coboeth,Bo:16.35.23.643)
if you wise be and good, he will follow you
'If you are wise and good, he will follow you.'

b. ... se wolde geagnian **him** þa læssan Asiam (coorosiu,Or_5:4.118.1.2472)
who wanted own himself the lesser Asia
'...who wanted to take Asia Minor for himself'

It is unlikely that all such pronouns can be analyzed as focused, and they certainly have not ended up in post-verbal position due to heaviness. Of course examples of this kind are rare, but this is in large part due

to reasons that have nothing to do with normal object raising. Most pronominal objects in OE are clitics, clearly moved leftwards, even as far as the position between the topic and the finite verb in V2 clauses, and it is impossible to tell whether they moved there from a position before the main verb or after it.

Non-pronominal objects, on the other hand, do not move leftwards as far or as frequently, and they appear in this order at a considerably higher rate. Pintzuk (2002a) reports that of the 931 examples she found with an auxiliary verb and a full NP object, 341, or 36.6%, were post-verbal. While focus most likely played a role in determining OE word order, it is implausible to assume it for one third of all non-pronominal objects. A further interesting pattern emerges if we take a diachronic perspective on these VO word orders. In texts written before 950, 144 of 524 objects, or 27.5%, were post-verbal, while in texts from after 950, 197 of 407 objects, or 48.4%, were post-verbal, according to Pintzuk (2002a). This should be considered in the light of Allen's (1995) assessment that the full system distinguishing four cases was still intact at the end of the 11th century, though a series of sound changes had led to an increase in the number of syncretic forms.[12] There are two important points to be made about these facts. First, the rate of post-verbal objects is already high in the early period, when the case system was at its most robust. This means it is not the case that the overall numbers for VO orders in OE have been skewed by data from late texts where the collapse of the case system was already advanced. Second, as discussed by Pintzuk, the comparison of the two periods gives evidence that the replacement of OV by VO did not just occur suddenly in Early Middle English after the morphological case system collapsed. Rather, if we consider these data together with Kroch & Taylor's (2000) claims about OV order in ME, the change appears to have been a gradual process that was already underway in the OE period and progressed independently of the changes in the nominal morphology.

Thus neither of the alternatives Roberts proposes for clauses with the object following both a finite auxiliary and a non-finite main verb is very convincing. The first involves a complicated derivation depending on unorthodox assumptions about head movement, all to restore the relative ordering posited as the base, while the second requires an implausible rate of occurrence of focused objects and may not even be compatible with the version of Case theory that Roberts explicitly assumes. A far simpler analysis is that object raising has failed to take place in these examples (or, in more traditional terms, that these are early head-initial VPs). Under Roberts' theory, however, this is not available, because for him

object raising should be obligatory in OE due to the richness of case-marking. That is, the hypothesis that rich case-marking implies object raising forces the adoption of a questionable analysis for a very basic OE word-order pattern. It is also out of step with the time-line for the change from OV to VO orders in English, and for these reasons it should be rejected.

3 CROSS-LINGUISTIC COUNTER-EVIDENCE

In the previous section, I argued that Roberts' (1997) connection of object raising and OV word-order to rich case-marking runs into difficulties when we consider the details of the historical English data it was meant to explain. Of course, his theory was not intended to apply solely to English, but to capture the interaction between case-morphology and the syntax more generally. In this section I will thus turn to a larger sample of languages and see how Roberts' ideas fare. Specifically, I will consider the facts of verb-object order and object raising in the other Germanic languages and argue that they militate strongly against any direct connection with rich morphological case.

To begin with, consider a comparison between Dutch, German, Icelandic, and their immediate predecessors. As shown in Table 1, Icelandic has, if anything, richer case-marking than German, with the noun itself bearing overt case markers (in boldface) in all but one paradigm slot.

[Table 1 here]

Even taking determiners into account (in italics), Icelandic beats German in distinguishing nominative from accusative in the plural and the feminine in addition to the masculine. Yet Icelandic is VO while German is OV. If the case-marking in German is rich enough to force object movement across the verb, then it must be rich enough to do so in Icelandic too. Roberts' explanation of why Icelandic is nonetheless VO is that here there is movement of the verb to I in addition to object raising. However, this leads to troubling questions for German. Analogous to the idea that rich case-marking is associated with object raising is the theory that verb-raising is tied to rich agreement morphology (see especially Roberts, 1993; as well as Holmberg & Platzack, 1995; Platzack, 1988; Rohrbacher, 1994; Vikner, 1997). Here, it is to be noted that German has agreement morphology as rich as that in Icelandic, which is shown by the present tense paradigms for *heyra* 'hear' and *tragen* 'carry' in Table 2.

[Table 2 here]

In each language there are five distinct forms for six person number slots. Thus we should expect German to have V-to-I raising as well. For this reason it is generally assumed, by those who claim that verb raising

depends on agreement, that languages like German must be head-final, at least within I, but this move is unavailable to Roberts (1997), given his adoption of the Kaynean antisymmetry. If the German IP is left-headed, then we are left predicting that the language should be VO.[13]

In other words, a unified treatment of Icelandic and German under the assumption of a direct relationship between case-marking and object shift is incompatible with a direct relationship between agreement-marking and verb raising. While it is certainly possible to assume that object movement depends on overt morphology while verb movement does not, the lack of consistency eliminates the possibility for an attractive reduction of both processes to a single abstract phenomenon, and Roberts presents no independent motivation for such a move. If anything, there seems to be more empirical support and argumentation in the literature in favor of the agreement/verb raising connection than for Roberts' case/object movement connection.

Even worse for theories tying word order to morphology is Dutch, which is OV like German. Under Roberts' analysis we must again posit object shift to Spec-Agr_OP and failure of V-to-I raising, but Dutch has no more case morphology than English. It is largely to handle these facts that Roberts proposes the potential syntactic trigger for acquisitiong of strong Case features, alongside the morphological trigger. Previous stages of Dutch did have German-like case-marking, so the idea is that, while the morphological trigger for strong Case was lost when this case-marking was lost, there was enough unambiguous syntactic evidence for it at every stage that children continued to learn it. The question then, of course, is why the development of English was different. Roberts' suggestion is that the existence of other reordering processes in older stages of English – including the ability of focused objects to stay in situ discussed above, verb raising to Agr_O, and remnant movement of verbal phrases past objects that have raised out of them to Spec-Agr_O – obscured the syntactic evidence for object movement that was presented to learners. When the morphological trigger was lost as well, there was no longer sufficient evidence to posit a strong N feature, and this reverted to its weak setting, which Roberts suggests is the default.

However, older forms of Dutch (and German) also allowed post-verbal DPs (i.e. surface VO orders). This is demonstrated by the following Middle Dutch example from Neeleman & Weerman (1999, p. 76):

(7) Hi soude dorbreken den muur.

he would through-break the wall

'He would break through the wall.'

Roberts acknowledges this fact in a footnote, and concludes that Dutch must have lost these orders before it lost its case-marking, though it is unclear why this should be. In fact, as regards the chronology of the changes, Weerman (1997) and Neeleman & Weerman (1999) have argued roughly the opposite, that the loss of these orders was actually the result of the loss of case-marking in the language.[14] So the question is why orders of this kind served to eliminate the raising of objects to Spec-Agr$_O$P in English, while in Dutch and German they simply disappeared. Furthermore, why is it that Dutch and German followed the same path of development, eliminating VO orders in favor of rigid OV, while English went off by itself in the other direction? If rich case-marking were a determining factor for word-order patterns, we should have expected Dutch and English, which have lost their older case systems, to pattern together to the exclusion of German, which has not.

Even if we can get around the problem of Icelandic being VO by assuming that the verb raises to a higher position than in OE and German, the surface position of objects still raises trouble. There is good evidence that Icelandic objects can raise out of their base positions as their rich case-marking would lead Roberts to predict:

(8) Jón las bækurnar$_i$ ekki t$_i$.

John read books-the not

'John didn't read the books.'

We see in (8), from Collins & Thráinsson (1996), that the object can surface to the left of negation. Given the assumptions that objects are first merged as complements of the verb, and negation marks the left edge of the VP (or perhaps an even higher functional projection), this order can only arise via overt object shift. Indeed, during the early stages of Minimalism, it was this movement process that was the focus of the discussion of case-driven movement of the object to Spec-Agr$_O$P. However, there are crucial differences between this and whatever derives OV orders in West Germanic languages like German. In the latter, all DP objects must appear before the non-finite verb, but in Icelandic, overt object shift is variable, depending on the definiteness, specificity and pronoun/full NP status of the object in addition to discourse

factors. Sentence (9), for example, shows that the sentence is grammatical with unshifted order:

(9) Jón las ekki bækurnar.
John read not books-the
'John didn't read the books.'

Note that OE goes along with German rather than Icelandic in this respect, especially in the early periods of its history, and certainly in Roberts' analysis. He assumes that, with the possible exception of focused objects, raising to Spec-Agr_O is obligatory in OE.

In order to handle the Icelandic data, one would have to assume that Case features are optionally strong in the language. When the strong variant is chosen, overt object shift occurs obligatorily. When the weak variant is chosen, objects wait until LF to move overtly. However, such variability is not predicted by Roberts' theory, because it would imply that the raising or non-raising of an object has nothing to do with morphological case. As discussed above, Roberts predicts that there should be no language that has rich case-morphology, yet lacks overt object raising. For the history of English one could perhaps argue that variation in the raising of the object originated as the case system of the language began to break down, and non-raised orders took over when it had fully collapsed. In Icelandic, however, there is no sign that morphological case is being lost, and thus no reason why non-raising orders should occur. Icelandic also presents a constellation of facts known as Holmberg's Generalization, which amount to overt object shift being parasitic on overt raising of the main verb. In clauses where a finite auxiliary raises and leaves the non-finite main verb in situ, the object must also remain in what would seem to be its base position (see e.g. Holmberg, 1999):

(10) Jón hefur (*hana) ekki séð (hana).
John has (*her) not seen (her)
'John has not seen her.'

It is difficult to imagine why Case features should be forced to be weak in such environments. OE and German are again very different in this respect, with objects indeed appearing before non-finite verbs, under Roberts' analysis moving across them. Such attempts to derive both OE and German style OV orders and Icelandic object shift orders via Case-driven object raising would seem to be misguided.

The historical development of OV and VO orders and object shift within the Scandinavian

languages presents additional difficulties for theories that tie word-order directly to morphological factors. To begin with, even though it has undergone essentially no changes in its morphological system, Icelandic has lost OV orders over the course of its recorded history (Hróarsdóttir, 2000). Recall that, under Roberts' analysis, Icelandic is currently VO because it has both object raising and V-to-I raising. The loss of older OV orders could thus only be due to the acquisition of V-to-I in the language. It is hard to see what could have caused this, however, if movement is tied to morphology. It is certainly not the case that Icelandic verbal agreement has become richer over the relevant period.

What is more, the loss of OV orders is actually just one component of a unified change in the ordering of verbal complements. That is, pre-verbal complements of all types – DPs, PPs and clauses – were lost at the same time, as shown by Hróarsdóttir (2000). Sundquist (2002), in a large-scale study of the historical development of the Mainland Scandinavian languages, presents similar arguments, supported with strong statistical evidence. Looking at Middle Norwegian in the period from 1250 to 1525, he performs a logistic regression analysis on the changes in the frequencies with which the various complement types appear pre-verbally. The resulting slopes are nearly identical for pronominal objects, non-pronominal objects and PPs (-.024, -.028 and -.024 logit units per year respectively). Sundquist thus argues, adopting the Constant Rate Hypothesis (Kroch, 1989), that the loss of pre-verbal PPs and the loss of pre-verbal objects represent a single change. If this is correct, then it is difficult to maintain that the loss of case-marking was the driving force in the change, because this should not have had any effect on PPs, and because pronouns did not actually lose their case-marking in Norwegian.[15] Sundquist further presents evidence that the loss of pre-verbal objects in Middle Norweigan (as in English) does not track the loss of case-marking. The most relevant syncretisms in the nominal paradigms appear a full two centuries before the start of the major decline in OV orders.

Matters do not get any better when we turn to the historical development of overt object shift. In all of the modern Scandinavian languages, the pronouns have retained at least a nominative/oblique distinction and undergo overt object shift. Full DPs, on the other hand, have retained their case-marking in Icelandic but not in Mainland Scandinavian, and undergo object shift only in the former. This is the basis for theories like that of Holmberg & Platzack (1995), which make object shift depend formally on rich case-marking. However, the historical facts militate strongly against this analysis.

Sundquist (2002: ch. 4) shows first of all that the frequency of pronominal object shift in Norwegian has **increased** over time. In his corpus, spanning from 1275 to 1525, only 77.5% of non-indefinite pronouns shift overtly, while in the modern standard language overt object shift of such pronouns is obligatory. This is surprising for case-based accounts, since the number of case distinctions in pronouns has actually decreased over the period. The data on non-pronominal object shift, however, are even more surprising. The available evidence indicates that, unlike Modern Icelandic, the older Scandinavian languages simply did not allow it, even though they all had the rich case-marking system that has survived in Icelandic. Sundquist finds that, of the 135 non-pronominal objects that appear with a diagnostic adverbial element in his corpus of Middle Norwegian, **zero** undergo overt object shift. No examples of non-pronominal object shift turn up in Haugan's (1998) examination of the entire Old Norse saga corpus either. Overt shift of non-pronominal objects is lacking in modern Faroese as well, even though it has case-marking nearly as rich as that in Icelandic (Holmberg, 1999; Vikner, 1994). Oddly enough, this movement is thus apparently an innovation of Modern Icelandic. But if this is so, then we can conclude nothing about a potential relationship to overt case-marking. Given the data from Old Norse, Middle Norwegian and Modern Faroese, rich case-marking does not imply the availability of non-pronominal object shift. It could still be that case-marking is necessary for object shift to be possible, but this is not what Roberts' theory predicts. Again, his claim is that rich case triggers the acquisition of the strong Case feature that drives overt object raising, and thus it should imply overt object shift.

4 CASE AND INTERACTIONS BETWEEN SYNTAX AND MORPHOLOGY

As an interim summary, the empirical evidence from the history of English and the other Germanic languages leads us to doubt the sort of connection that Roberts (1997) draws between case-marking and the ordering of objects and verbs. The hypothesis is interesting because it proposes to relate two sets of facts, raising the prospect of a unified account that minimizes stipulations, but it appears to be at odds with the facts and leads to serious complications in areas of the theory related to movement. At this point we could simply stop at this negative conclusion, but that would not constitute much of a step forward. Instead, in this section I will explore why Roberts' theory and several other theories that posit a dependency of word-order properties on rich morphology have failed. If we can understand this, then we can make progress toward theories linking syntactic properties to morphological ones that work.

In order to make strong, testable predictions, a theory like that put forward by Roberts needs to incorporate a precise definition of 'rich' case-marking. Roberts does discuss two such possibilities, but neither is really workable. His first idea is that having rich case-marking means distinguishing nominative from accusative, but he rejects this because "only two out of seven declensions ever distinguished nominative from accusative in OE, and those only in the singular" (p. 425). He suggests instead that what is crucial is having this distinction in the determiners, rather than in the nouns. This would seem to be more plausible, since all masculine and feminine singular DPs would then show distinct nominative and accusative forms in OE, and because we need evidence from the article to find similar distinctions in modern German. However, here again this distinction has a limited distribution (to masculine singulars). Furthermore, there are any number of case-marking OV languages that lack overt determiners entirely, including Latin, which Roberts mentions elsewhere in the article for showing just this set of properties. To be fair, the discussion of this issue in Roberts (1997) is very brief, limited almost entirely to one footnote. The implication seems to be that the morphological details can and will be worked out at a later time.

However, this never actually seems to happen. Other authors like Kiparsky (1997) and Neeleman & Weerman (1999), who also propose a crucial role for rich case-marking in the determination of word order, fail to give a proper definition of rich case-marking as well. We must ask how one would go about proposing such a definition in the first place. For comparison, let us consider the phenomenon of V-to-I raising mentioned above, where theories drawing connections to the richness of agreement morphology have been more explicit about richness. Consider for example a proposal from Rohrbacher (1994):

> (11) A language has V to I raising if and only if in at least one number of one tense of the regular verbs, the person features [1st] and [2nd] are both distinctively marked (p.108).

We could follow this lead and propose something like the following for the richness of case-marking:

> (12) If a language distinguishes subject from object case in at least one number of at least one productive noun inflection class, then it has strong Case features.

Definitions of this kind, however, have never succeeded for V-to-I raising. Throughout the 1980s and 1990s, a series of ever more precise formulations were proposed (again, see Holmberg & Platzack, 1995;

Platzack, 1988; Roberts, 1993; Rohrbacher, 1994; Vikner, 1997), but at each stage, another language or dialect was found that contradicted the most recent formulation. Rohrbacher's formulation, for example, is contradicted by Tromsø Norwegian and Kronoby Swedish, which show no person distinctions, yet still have overt verb raising (Bobaljik, 2002).

Indeed, Bobaljik (2002) has demonstrated that the variation in V-to-I raising attested within the Germanic languages alone is such that definitions of this kind by their very nature cannot capture it.[16] He argues instead that the correlation supported by the empirical evidence is not a bi-conditional, but a one-way implication. Rich agreement does imply verb-raising, but lack of rich agreement does not imply the lack thereof. That is, the true counterexamples to the correlation all involve languages with limited verbal morphology that nonetheless have V-to-I. In fact, in this respect Bobaljik's approach is strikingly similar to Roberts' proposal on case-marking and object raising. Both have rich morphology trigger the acquisition of a movement process, which however can also be acquired in the absence of morphology given sufficient syntactic evidence.

However, there is a difference between the two theories that I will argue is crucial. Bobaljik defines rich verbal inflection not in terms of contrasts in morphological paradigms, but rather in terms of the identification of segmentable affixes that correspond to syntactic heads. The approach can be demonstrated with a comparison of the conjugation of weak verbs in Old and Modern English. In Table 3 we see that OE had segmentable endings for tense and agreement. That is, in a form like *hierdest,* we can identify *-d-* as the past tense marker and *-est* as the 2nd singular marker.

[Table 3 here]

This is not so for Modern English, as shown in Table 4. While there is a clear past tense marker, and what appears to be a 3rd singular ending *-s*, the two do not co-occur in any single form because the latter is restricted to the present.

[Table 4 here]

Why should this matter? Bobaljik argues that morphological patterns like that in OE lead language acquirers to posit distinct syntactic heads in the structure, in this instance both T and Agr. He combines this with a theory where the existence of multiple heads in the Infl complex forces raising of the verb, and the implication from rich morphology to movement is derived.[17] In a language like Modern English that

does not have have segmentable affixes, the learner can posit a single I head which can host whatever inflection is appropriate. If there is sufficient syntactic evidence for the verb raising to I, learners will still acquire it, as in Tromsø Norwegian and Kronoby Swedish, but it will not be forced by virtue of the structure.

Some aspects of Bobaljik's analysis could be debated, and it may turn out to be incorrect in the details (though it does appear to be an improvement over those that came before in terms of empirical coverage). For our purposes, however, the details of verb raising and the inventory of functional heads are irrelevant. What is crucial is the fact that Bobaljik provides a simple and explicit account of the point where the richness of morphology interacts with the syntax. Rich agreement has syntactic implications because it can provide unambiguous evidence for certain syntactic structures. Proposals of the Rohrbacher kind fail here, because they provide no justification for why one particular paradigmatic distinction should force verb-raising while others do not. Indeed, there is a very good reason for this. Richness of agreement and case-marking are morphological issues, while object and verb raising are syntactic ones. For the most part, they deal in different terms and distinctions which are not directly translatable. Defining the conditions on a particular syntactic movement in terms of morphological facts is a slippery business, because we have no a priori reason to choose from the various imaginable combinations. We can propose on the basis of a given sample of languages that verb raising depends on morphological property X. However, if there is no basis for that proposal independent of our observation that the two go together in the sample, then all we have is a descriptive generalization. We have little reason to expect that the pattern will hold out when the sample of languages is widened. A proposal of this sort should instead be embedded in a theory of the syntax-morphology interface which morphological property X is related in some principled way to a syntactic property that is involved in verb raising. That is, we need a theory which makes available a set of terms that the syntax and morphology share. A statement in the morphology involving these terms will then translate directly into the syntax with clear consequences. Bobaljik, e.g., adopts the hypothesis — associated with Distributed Morphology (Embick & Noyer, to appear; Halle & Marantz, 1993, 1994) and other realizational theories — that morphology spells out the pieces put together by the syntax. Since both syntax and morphology deal with the same pieces, statements about pieces identified in the morphology will of necessity have relevance for the syntax, and vice versa.

Roberts (1997) adopts no such explicit hypothesis regarding the structure of the interface, and thus the connection he posits between object raising and the richness of morphological case is essentially a stipulation rather than a principled statement about relationships that follow from the interaction between syntax and morphology. What sort of theory would we need in order to actually derive such a dependency, and can that theory be supported? At the minimum, we need some form of isomorphism between morphological case forms and movement operations or landing sites. Of course, something in this direction can be achieved under certain versions of Case-theory, if we assume that the Case features that trigger A-movement of DPs are the same features that are spelled out as morphological case-markers. However, a considerable body of work over the past two decades has argued against the direct connection between morphological case and the positional licensing of DPs (see e.g. Harley, 1995; Marantz, 1991; McFadden, 2004; Schütze, 1997; Sigurðsson, 2001; Yip, Maling & Jackendoff, 1987). The argument comes in three parts, which I will lay out here in abbreviated form, referring the reader to the cited works for the full details.

First, and most importantly, the relationship between structural cases and the structural positions of the DPs that bear them is not one-to-one, but many-to-many. In most languages, the primary structural cases are assigned not to specific syntactic positions, but according to a sequence, after the assignment of oblique cases.[18] So in nominative-accusative languages, the highest argument which does not receive an oblique case is assigned nominative, the next highest accusative. In ergative-absolutive languages, the lowest non-oblique argument gets absolutive and the next lowest gets ergative. This accounts for why underlying objects are marked nominative in passives, unaccusatives and oblique subject constructions like the Icelandic example (13), independently of whether they raise to subject position or not (see Marantz, 1991; Smith, 1996; Yip et al., 1987):

(13) Henni hefur alltaf þótt Ólafur leiðinlegur.
her.DAT has always thought Olaf.NOM boring
'She has always found Olaf boring.'

It also provides an explanation for why the assignment of the structural cases is nearly identical in German and Icelandic, in spite of the fact that the two differ considerably in terms of A-movement and subject determination. For example, the theme argument of dative experiencer verbs like German *gefallen* and

Icelandic *líka* (both roughly 'be pleasing to' or 'like') gets nominative in both languages, in spite of behaving like a subject in the former and an object in the latter.

Second, morphological case can be assigned to a position in which no overt DP is licensed. In (14), the agreement on the predicate adjective *einum* 'alone' shows that the subject position of the infinitive is assigned dative (*batna* 'recover' takes a quirky dative subject) in spite of being phonologically null (Sigurðsson, 1991; see vanden Wyngaerd (1994) for similar data from Latin and Ancient Greek.):

(14) Að PRO batna veikin einum er erfitt.

to PRO recover the-disease alone.DAT.MASC is difficult

'To recover from the disease alone is difficult.'

Note that the issue raised here cannot be avoided even if we adopt the null Case theory of PRO (Chomsky & Lasnik, 1993; Martin, 2001). Whether we say that PRO gets null Case or no Case at all, it must be treated differently in terms of positional licensing than overt finite subjects, yet the assignment of morphological case is the same for both.

Third, if a DP surfaces in a position which does not satisfy the conditions for any specific morphological case, it does not necessarily fail to be licensed, but may instead surface with a default case form. A cross-linguistically common environment for this is left-dislocation, as in the following German sentence. Here a default nominative shows up instead of the dative that we might have expected by concord:

(15) Der/*Dem Hans, mit dem spreche ich nicht mehr.

the.NOM/*DAT Hans with him.DAT speak I not more

'Hans, I don't speak with him anymore.'

As Schütze (2001) argues, if the licensing of DPs were tied to the assignment of morphological case, then it could always be satisfied by this default rule, and the equivalent of the old Case Filter would be rendered vacuous. That is, whatever assigns morphological case in examples like this must be independent of syntactic Case, otherwise Case theory would never rule out any sentences (see Schütze, 2001 for further development of this argument and evidence for default case in a number of other languages).

In fact, the role of Case in driving movement has been greatly reduced in developments of Minimalist theory since Roberts (1997) was written. Chomsky (2000, 2001) argues that it is in fact some

version of the EPP that drives DP movement, while the needs of Case-checking merely constrain that movement. Specifically, when a DP has its Case checked, it is rendered invisible to further A-movement-triggering processes. This connection is retained to block things like subject-to-subject raising out of finite clauses as in example (16):

(16) *John seems is sick.

To a large extent, this transfer of responsibilities to the EPP is an attempt to handle facts like those just discussed, where structural position and morphological case do not match up. Chomsky wants to maintain the idea that syntactic Case features are what trigger the spell-out of morphological case, and thus he is forced to admit Case-checking without movement in examples like (13).

A tight connection between morphological case and DP movement in the form of Case theory thus does not seem possible. In fact, even if it were, we would still be a long ways from showing that rich case-marking derives object raising. At no stage in its development has Case theory claimed, for example, that languages like OE and German have Case while languages like modern English do not. Rather, it has always been asserted that DPs require licensing in all languages, and that this licensing has morphological side-effects in some but not others. Recall that within the early Minimalism of Roberts (1997), DPs must raise to specifier positions to check their Case features, but depending on the strength of those features, this movement can either be early and overt or late and covert. Roberts' proposal is that the former option is forced by rich morphology, but this follows in no way from the structure of the theory. There is nothing to prevent us from claiming just the opposite, that rich morphology is what allows DPs to wait and undergo covert movement at LF. Indeed, we could even construct a functional argument for this position, whereby the morphology serves to identify the checking relationship so that overt movement is not necessary.

This situation is crucially different than what we found with V-to-I movement. There it was reasonable to posit a direct mapping between morphological and syntactic elements. However, while it has sometimes been claimed that morphological case spells out a syntactic head (see e.g. Lamontagne & Travis, 1984; Neeleman & Weerman, 1999), the idea has never really caught on, in part because—for the structural cases at least—it is hard to map this head to any consistent contribution to the sentential semantics in the way that is standard for elements like Tense on verbs and Number on nouns.

Furthermore, even those who do posit a K(ase) head locate it somewhere above D, crucially not in the extended verbal projection. That is, the need to combine with K could at most drive movements within the extended nominal projection, not movement of the entire nominal complex to some other position in the clause.

5 SUMMARY AND CONCLUSION

In this paper I have argued that the basis for a connection between rich morphological case and overt object placement is lacking. Barring the development of a theory that provides a non-stipulative mapping between morphological case categories and syntactic DP positions (analogous to the mapping between verbal suffixes and heads in the extended verbal projection), all imaginable versions of such a connection are equally statable. From this perspective, it is unsurprising that the theory proposed by Roberts (1997), tying the change from OV to VO word order in the history of English to the loss of morphological case, turns out to not quite fit the facts from English, and is contradicted by facts from the other Germanic languages. I have not offered an alternative explanation for this change, but I have suggested a way to think about connections between syntax and morphology that may help in the formulation and evaluation of hypotheses on this sort of change in future research.

REFERENCES

Allen, C. (1995). Case marking and reanalysis: grammatical relations from Old to Early Modern English. Oxford: Oxford University Press.

Biberauer, T. & I. Roberts. (2005). Changing EPP parameters in the history of English: Accounting for variation and change. This issue.

Bobaljik, J. D. (2002). Realizing Germanic inflection: why morphology does not drive syntax. Journal of Comparative Germanic Linguistics 6: 129-167.

Chomsky, N. (2000). Minimalist inquiries: the framework. In Martin, R., D. Michaels & J. Uriagereka (eds.) Step by step: essays on Minimalist syntax in honor of Howard Lasnik. Cambridge, Mass.: MIT Press. 89-155.

Chomsky, N. (2001). Derivation by phase. In Kenstowicz, M. (ed.) Ken Hale: A life in language. Cambridge, Mass.: MIT Press. 1-52.

Chomsky, N. & H. Lasnik. (1993). The theory of principles and parameters. In Jacobs, J. et al (eds.) Syntax: an international handbook of contemporary research.

Collins, C. & H. Thráinsson. (1996). VP-internal structure and object shift in Icelandic. Linguistic Inquiry 27: 391-444.

Embick, D. & R. Noyer. (to appear). Distributed Morphology and the syntax/morphology interface. In Ramchand, G. & C. Reiss (eds.) The Oxford handbook of linguistic interfaces. Oxford: Oxford University Press.

Halle, M. & A. Marantz. (1993). Distributed Morphology and the pieces of inflection. In Hale, K. & S. J. Keyser (eds.) The view from building 20: essays in linguistics in honor of Sylvain Bromberger. Cambridge, Mass.: MIT Press. 111-176

Halle, M. & A. Marantz. (1994). Some key features of Distributed Morphology. MIT Working Papers in Linguistics 21:275-288.

Harley, H. (1995). Abstracting away from abstract Case. Proceedings of NELS 25: 207-221.

Haugan, J. (1998). Old Norse word order and information structure. PhD. dissertation, NTNU Trondheim.

Holmberg, A. & C. Platzack. (1995). The role of inflection in Scandinavian syntax. Oxford: Oxford

University Press.

Holmberg, A. (1999). Remarks on Holmberg's Generalization. Studia Linguistica 53: 1-39.

Hróarsdóttir, Þ. (2000). Interacting movements in the history of Icelandic. In Pintzuk, Tsoulas & Warnder (eds.) 296-321

Kayne, R. (1994). The antisymmetry of syntax. Cambridge, Mass.: MIT Press.

Kemenade, A. van & N. Vincent (eds.) (1997). Parameters of morphosyntactic change. Cambridge: Cambridge University Press.

Kiparsky, P. (1997). The rise of positional licensing. In Kemenade, A. van & N. Vincent (eds.) Parameters of morphosyntactic change. Cambridge: Cambridge University Press.

Koopman, W. (2005). Transitional syntax: postverbal pronouns and particles in Old English. This issue.

Kroch, A. (1989). Reflexes of grammar in patterns of language change. Language Variation and Change 1: 199-244.

Kroch, A. & A. Taylor. (1997). Verb movement in Old and Middle English: dialect variation and language contact. In van Kemenade & Vincent (eds.) 297-325.

Kroch, A. & A. Taylor. (2000). Verb-complement order in Middle English. In Pintzuk, Tsoulas & Warner (eds.) 132-63.

Lamontagne, G. & L. Travis. (1987). The syntax of adjacency. Proceedings of WCCFL 6: 173-186.

Lightfoot, D. (1979). Principles of diachronic syntax. Cambridge: Cambridge University Press.

Marantz, A. (1991). Case and licensing. Proceedings of ESCOL. Republished in Reuland, E. (ed.) (2000). Arguments and case: explaining Burzio's Generalization. Philadelphia: John Benjamins.

Martin, R. (2001). Null Case and the distribution of PRO. Linguistic Inquiry 32: 141-166.

McFadden, T. (2004). The position of morphological case in the derivation: a study on the syntax-morphology interface. PhD. dissertation, University of Pennsylvania.

Neeleman, A. & F. Weerman. (1999). Flexible syntax: a theory of case and arguments. Dordrecht: Kluwer.

Pintzuk, S. (1991). Phrase structures in competition: variation and change in Old English word order. PhD. dissertation, University of Pennsylvania.

Pintzuk, S. (2002a). Verb-object order in Old English: variation as grammatical competition. In Lightfoot, D. (ed.) Syntactic effects of morphological change. Oxford: Oxford University Press.

Pintzuk, S. (2002b). Morphological case and word order in Old English. Language Sciences 24: 381-395.

Pintzuk, S., G. Tsoulas & A. Warner (eds.) (2000). Diachronic syntax: models and mechanisms. Oxford: Oxford University Press.

Platzack, C. (1988). The emergence of a word order difference in Scandinavian subordinate clauses. McGill Working Papers in Linguistics: 215-238.

Roberts, I. (1993). Verbs and diachronic syntax. Dordrecht: Kluwer Academic Publishers.

Roberts, I. (1997). Directionality and word order change in the history of English. In van Kemenade & Vincent (eds.) 397-426.

Rohrbacher, B. (1994). The Germanic VO languages and the full paradigm: a theory of V to I raising. PhD. dissertation, University of Massachusetts.

Schütze, C. (1997). INFL in child and adult language: agreement, Case and licensing. PhD. dissertation, MIT.

Schütze, C. (2001). On the nature of default case. Syntax 4: 205-238.

Sigurðsson, H. (1991). Icelandic Case-marked PRO and the licensing of lexical arguments. Natural Language and Linguistic Theory 9: 327-65.

Sigurðsson, H. (2001). Case: abstract vs. morphological. Working Papers in Scandinavian Syntax 67.

Smith, H. (1996). Restrictiveness in case theory. Cambridge: Cambridge University Press.

Sprouse, R. (1998). Some notes on the relationship between inflectional morphology and parameter setting in first and second language acquisition. In Beck, M. (ed.) Morphology and its interfaces in second language acquisition. Philadelphia: John Benjamins.

Sundquist, J. (2002). Morphosyntactic change in the history of the Mainland Scandinavian Languages. PhD dissertation, Indiana University.

Taylor, A., A. Warner, S. Pintzuk & F. Beths. (2003). The York-Toronto-Helsinki parsed corpus of Old English prose. Department of Language and Linguistic Science. University of York.

Vikner, S. (1994). Scandinavian object shift and West Germanic scrambling. In Corver, N. & van Riemsdijk, H. (eds.) Studies on scrambling: movement and non-movement approaches to word-order phenomena. Berlin: Mouton.

Vikner, S. (1997). V-to-I movement and inflection for person in all tenses. In Haegeman, L. (ed.) The new comparative syntax. London: Longman.

Weerman, F. (1997). On the relation between morphological and syntactic case. In van Kemenade & Vincent (eds.) 427-59.

Wurff, W. van der. (1997). Deriving object-verb order in Late Middle English. Journal of Linguistics 33: 485-509.

Wurff, W. van der. (1999). Objects and verbs in modern Icelandic and fifteenth-century English: a word order parallel and ist causes. Lingua 109: 237-65.

Wyngaerd, G. vanden (1994). PRO-legomena: distribution and reference of infinitival subjects. New York: Mouton de Gruyter.

Yip, M., J. Maling & R. Jackendoff. (1987). Case in tiers. Language 63:217-250.

	German		Icelandic	
	sg	Pl	sg	pl
N	*der* Mann	*die* Männer	hest-**ur**-*inn*	hest-**ar**-*nir*
G	*des* Mann-**es**	*der* Männer	hest-**s**-*ins*	hest-**a**-*nna*
D	*dem* Mann	*den* Männer-**n**	hest-**i**-*num*	hest-**u**-*num*
A	*den* Mann	*die* Männer	hest-*inn*	hest-**a**-*na*

Table 1: Def. masc. strong nouns in German (*Mann* 'man') and Icelandic (*hestur* 'horse')

	Icelandic	**German**		**Icelandic**	**German**
1s	heyr-i	trag-e	1p	heyr-um	trag-en
2s	heyr-ir	träg-st	2p	heyr-ið	trag-t
3s	heyr-ir	träg-t	3p	heyr-a	trag-en

Table 2: Present inflection in Icelandic (*heyra* 'hear') and German (*tragen* 'carry')

	Present	**Past**
1s	hier-e	hier-d-e
2s	hier-st	hier-d-est
3s	hier-þ	hier-d-e
pl	hier-aþ	hier-d-on

Table 3: Old English (Weak) Conjugation

	Present	**Past**
3s	hear-s	hear-d
Elsewhere	hear	hear-d

Table 4: Modern English (Regular) Conjugation

[1]This is an expression of the idea that learners will assume the simplest derivation with the least amount of displacement that is consistent with the primary linguistic data (Clark and Roberts, 1993).

[2]The situation is of course rather complex, but a quote from Allen (1995) can serve as a good summary. "By the beginning of the fourteenth century, the category distinction between dative and accusative case had been gone in most of the Midlands for about a century, and for considerably longer further north. In Kent, however, evidence from the *Ayenbite of Inwyt*, written by Dan Michel in 1340, shows that this distinction is still alive nearly half-way into the century" (p. 206).

[3] As we will see in the next section, this is crucial to Roberts' account of languages like Dutch, where just this possibility is exemplified.

[4]The disjunction in Roberts' formulation of the triggering conditions for strong Case features could be regarded as a weakness in his theory, precisely because it yields weaker predictions and makes it more difficult to test.

[5]That is, my claim that certain sentences are derived without object raising is equivalent to the claim that they sentences are underlyingly VO rather than OV.

[6]I follow the standard literature on V2 here in referring to the pre-verbal constituent as a "topic", without taking a position on whether it is a topic in the technical sense or something slightly different.

[7]Except where otherwise indicated, the Old English examples are taken from Pintzuk (2002a).

[8]Roberts does not discuss the interaction of the main verb with the lower Agr_O, but even under his assumptions about head movement the former should not be able to just skip over the latter. He could assume another instance of excorporation, i.e. the main verb essentially moving through Agr_O, but here there is no word-evidence in support. I have thus made the more standard assumption in (3) that the Agr_O moves along in a complex head with the main verb to adjoin to the auxiliary. As far as I can tell this makes no difference one way or the other for Roberts' analysis.

[9]Roberts argued previously (1991) that excorporation is necessary to handle West Germanic verb raising. However, the excorporation analysis proposed in that paper for a word order found in Dutch actually depended on the assumption of head-final underlying word-order and a particular analysis of verb particles. Under different assumptions (closer to what Roberts, 1997 adopts), the argument for excorporation no longer holds.

[10]In the examples from the YCOE, I reproduce the label giving the abbreviated name of the text with page and line numbers exactly as it appears in the corpus.

[11]She does give one such sentence as an example, which is from Ælfric's mid 11th century First Letter to Wulfstan.

[12]That is, while many inflectional classes had ceased to show separate forms for nominative and accusative, the distinction was maintained in other classes (especially in the pronouns and determiners), and the forms that do show the distinction reliably show the proper distribution in the texts. This contrasts with later periods, where archaizing scribes continued to use historically accusative forms of pronouns alongside historically dative ones, but failed to get their distribution right. See Allen (1995) for extensive documentation and discussion.

[13]In fact, we might ask the same questions for Old English itself. It had verb agreement nearly as rich as that in German and Icelandic, with four distinct forms for the six slots. However, it did not make any person distinctions in the plural, a fact which could perhaps be claimed to make the crucial difference.

[14]They propose a theory where rich case-marking allows DPs to appear in non-canonical positions by facilitating theta-role assignment. When a language loses its case-marking, it loses this flexibility, and DPs are restricted to appear only in positions where they are governed, which in Dutch means before the verb.

[15]Roberts may in fact intend raising to Spec-Agr_O to apply to PPs and other non-DP complements. He certainly assumes that they undergo a raising process to get them past the non-finite verb, and notes that "given that we assume small-clause predicates and non-finite complement clauses are also subject to a checking requirement the notion of 'case' here is much more abstract than in GB theory" (p. 413). However, this would not seem to be in line with the rather concrete proposal that rich morphological case triggers the acquisition of strong Case features. If this case-marking were also interpreted abstractly, then his theory would lose all predictive power.

[16] See also Sprouse (1998) and Sundquist (2002) for criticism of this approach to explaining verb-raising.

[17]The idea is that a checking relationship must be established between the verb and any heads located higher in the extended verbal projection. This can be done by Agree in situ, but by minimality this will only work with the first head above V. If there is only one such head, all is well, but if there are more, V

can only get into a checking relationship with them by raising.

[18]I use the term 'oblique cases' informally to refer to all cases that do not participate in the case-alternations characteristic of structural cases. In languages like Old English, Icelandic and German, this thus includes the dative on indirect objects as well as so-called lexical cases and semantic cases.