THE RICH AGREEMENT HYPOTHESIS IN REVIEW*

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Abstract. This paper examines and evaluates what may be called the "Rich Agreement Hypothesis" (RAH) in the domain of verb movement asymmetries. The most prominent current accounts (e.g., Rohrbacher 1999) require inspection of the internal make-up of paradigms and take overt morphological variation to be the cause of syntactic variation. These proposals are shown here to be empirically untenable on the basis of evidence from dialect comparisons, diachronic changes, speaker variation, language-internal differences among constructions and acquisition data. In all cases, we find syntactic variation attested in the absence of corresponding morphological variation. Proposed weakenings of the RAH (drawing only a one-way implication from rich morphology to verb movement) are shown to be inconsistent with the morphology-driven approaches they claim to instantiate, or at best, incomplete. It is also noted that approaches relying on the overt marking of contrasts within a paradigm stand in conflict with results of current morphological theory. An alternative is considered that overcomes these problems. It is recognized that there are correlations between syntactic variation and morphological variation which do stand to empirical verification, and are thus to be explained. The particular correlations that are attested (and the attested domain of indeterminacy) follow from a theory which takes morphology to be not the cause, but rather a reflection, of syntactic structure, in line with standard theorizing in morphology.

Keywords: Paradigms, Morphology-Syntax Interface, (Rich) Agreement, (Anti-) Lexicalist Hypothesis, Verb Movement

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1. INTRODUCTION

In certain Germanic languages including Icelandic, the finite verb in a subordinate clause obligatorily precedes a class of elements that includes sentential negation, floating quantifiers and certain other adverbs (1a). In the related (standard) Mainland Scandinavian languages, represented here by Danish, the opposite order is generally required in the same environments (1b).

(1) a. … að hann **keypti** *ekki* bókina. (Icelandic)
that he bought not the.book
'… that he did not buy the book.'

b. … at han *ikke* **købte** bogen. (Danish)
that he not bought the.book
'… that he did not buy the book.' Platzack 1986:209

The standard analysis of this contrast, first proposed by Travis (1984:144f), is that this variation should be accounted for in terms of verb movement to Infl in (1a) and the absence of such movement in (1b). This account assimilates the contrast in (1) to the familiar contrast between French and English analysed in essentially these terms by Emonds (1978) and subsequently by Pollock (1989) and Chomsky (1991).

Another difference between Icelandic and Danish concerns the inventory of distinct elements in these languages' inflectional paradigms. In Icelandic, verbs are marked for a range of distinctions for person and number of the subject and for tense (2a). In Danish the only distinction marked is between preterite (i.e., past) and non-preterite tense (2b).

(2)

	a. Icelandic: *heyra* 'hear'		b. Danish: *høre* 'hear'[1]	
	Present	Preterite	Present	Preterite
1 psn sg	heyr **-i**	heyr **-ði**	hør **-er**	hør **-te**
2 psn sg	heyr **-ir**	heyr **-ði-r**	hør **-er**	hør **-te**
3 psn sg	heyr **-ir**	heyr **-ði**	hør **-er**	hør **-te**
1 psn pl	heyr **-um**	heyr **-ðu-m**	hør **-er**	hør **-te**
2 psn pl	heyr **-ið**	heyr **-ðu-ð**	hør **-er**	hør **-te**
3 psn pl	heyr **-a**	heyr **-ðu**	hør **-er**	hør **-te**

A little more than a decade ago, the suggestion was put forward that the two observed differences (syntactic in (1) and morphological in (2)) were related, and causally so. Thus arose what we may call the Rich Agreement Hypothesis (3).

[1] Paradigms, though not segmentation, from Vikner 1997:191, 197.

(3) *The Rich Agreement Hypothesis* (RAH)

"Rich" agreement causes V-to-I movement.

From the earliest presentations of this hypothesis, it has typically been interpreted in its strong form, i.e., "Rich agreement *and only rich agreement* causes V-to-I movement." From this stronger form it follows as a corollary that, if a language loses rich agreement, it will also lose V-to-I movement. Thus, Platzack 1988 (p216) states that the loss of verb raising to Infl "is an immediate consequence of the loss of agreement…". Diachronic evidence was offered to argue that this corollary was, in fact, correct. For example, Modern Danish is the descendant of a language which looked very much like Icelandic on both counts—Old Danish had the word order in (1a) and an inflectional paradigm very much like (2a) (Vikner 1997:205ff).

Versions of the RAH, most notably Rohrbacher 1999, have become very prominent not only in comparative Germanic syntax, but in syntactic theory more generally as well as in diachronic work and the acquisition literature.

One goal of this article is to present the evidence that (3) is empirically untenable, even within the restricted domain of the Germanic languages. Importantly, though, the failure of the strong version of (3) does not justify the pessimistic view that there are no correlations to be observed and ultimately explained. Indeed there are such correlations, and, by shifting the perspective away from counting contrasts in paradigms and more into line with current morphological theorizing, we find that exactly those correlations which are empirically sustainable follow from a theory which takes morphology to reflect, rather than drive, the syntax. Such an alternative was first sketched in Johnson 1990 and elaborated most recently in Bobaljik & Thráinsson 1998. Thus, a second goal of this article is to compare and contrast the two families of theories, with particular reference to the data implicated by the RAH. It is worth noting at this point that the issue here goes beyond the proper characterization of an empirical generalization and the choice among competing accounts. The question on which this discussion bears is the nature of the paradigm as a linguistic entity. As Rohrbacher 1999 (p. 7) notes, the reliance on overt distinctions runs against a general (though not exceptionless) trend in the morphological literature to treat paradigms as derived, and to seek accounts of syncretism within paradigms, rather than to take the paradigm as a given. The third and final goal of this article is thus to situate the discussions of the verbal inflectional paradigms and their relationship to syntax in the broader context of the morphology-syntax connection.

This paper is organized as follows. Section 2 provides a quick review of certain empirical points establishing the basis for the discussion to follow. In particular, various controls and caveats are noted here. This section presents nothing new for those familiar with the discussion, and can easily be skipped. Section 3 presents a brief review of the competing proposals, surveying the development of the RAH in Germanic, from its roots in Roberts 1985 and Kosmeijer 1986 through the most articulated proposals in Rohrbacher 1999 and Koeneman 2000 and then setting out the bare bones of the alternative account of Johnson 1990 and Bobaljik & Thráinsson 1998. Section 4 offers a direct comparison of various aspects of the paradigm-based theories with the paradigm-free alternative. I demonstrate that theories which take overt morphological variation to be the sole determinant of syntactic variation (such as Rohrbacher 1999) are empirically untenable on the basis of evidence from dialect comparisons, diachronic changes, speaker variation, and language-internal differences among constructions. I show also (section 4.2) that existing weaker theories in which the morphology is one (of potentially many) causes of syntactic variation are insufficient in key respects. On all these counts, the alternative theory makes the correct predictions. Section 5 presents a refutation of Rohrbacher 1999's claim that acquisition data supports the morphology-driven approaches. While the first two goals identified above are thus interleaved throughout sections 4 and 5, the third goal is addressed in section 6. I present considerations from the theoretical morphological literature, which, as noted, militate against theories which take overt morphological (especially paradigmatic) contrasts to be causally related to syntactic variation.

2. EMPIRICAL PRELIMINARIES

Before we begin the discussion in any detail, certain factual considerations stand to be established. Here, no new arguments or data will be presented and the reader familiar with the debate is invited to skip ahead to the next section.

The word-order contrast in (1) is taken to be emblematic of a true source of parametric variation among the Germanic languages and beyond (the paradigm example being, as noted, the much discussed English versus French contrast first discussed by Emonds (1978) and brought back to centre stage in Pollock 1989 and Chomsky 1991). Various qualifications, however, must be made, which chip away at the robustness of the contrast, and for the record, these are noted here. At the end of the day, the conclusion will still be that Icelandic, some varieties of Swedish and Faroese (and possibly Yiddish), as well as Middle English and the full gamut of Older Scandinavian languages, have (or had) verb movement to Infl, while such movement is lacking in the standard varieties of the modern Mainland Scandinavian languages and English. As most of the data is readily accessible (see especially Rohrbacher 1999 and references there) I will not recapitulate the evidence, but simply indicate various important caveats which are useful in understanding the relevant literature.

First, we note that discussion of word order in Germanic languages other than English is complicated by the Verb-Second (V2) effect. In most main clauses (and in some subordinate clauses) the verb immediately and obligatorily follows exactly one constituent, which may, but need not be the subject. Since den Besten 1983, this is standardly analysed as movement of some XP to Spec,CP with concomitant movement of the verb to C°, though a debate remains as to whether or not subject-initial clauses may be simple IPs as argued for by Travis (1984).

Because of this, the kind of difference which (1) is taken to represent, and for which the literature discussed here seeks an explanation, is restricted to subordinate clauses. In addition, work in this vein has been concentrated mostly on the Scandinavian languages and English. This is largely because the head-final word-order (below CP) in most of the other Germanic languages (German, Dutch, Afrikaans etc.) makes the position of the verb notoriously difficult to determine with any measure of certainty. There are arguments both for and against verb-raising, none of them particularly persuasive. Though Yiddish is head-initial within the VP (Diesing 1997) additional complications have made it difficult to determine whether or not it has verb raising to Infl outside of V2 environments. By and large, Yiddish appears to pattern with Icelandic regarding verb movement, though significant questions remain.[2]

Finally, it was noted in the course of the investigation of Icelandic and Yiddish syntax, that these two languages permit V2-like topicalization relatively freely in embedded clauses and even under an overt complementizer. This was noted at least as early as Thráinsson 1986, see Vikner 1994, 1995 for discussion. In this regard, Icelandic and Yiddish differ from the other Germanic languages, in which the distribution of V2 in embedded clauses is quite limited, dependent in

[2] Vikner 1995:138 claims that Icelandic and Yiddish "apparently behave completely alike" regarding embedded V2 and movement of the finite verb. This is not quite true, see fn. 4 below; see also Diesing 1990, Rohrbacher 1999:80ff and Bobaljik and Thráinsson 1998:49f for discussion.

large part upon properties of the matrix verb. The possibility of embedded V2 under an overt complementizer in Icelandic means that examples such as (1a) can not straightforwardly be taken as reliably indicative of verb movement to Infl.[3]

Exploring deeper into the syntax of Icelandic[4], Magnussón 1990, Vikner 1990 and others noted that V2-like topicalization is not available in all embedded clauses. For example, embedded constituent questions (i.e., as opposed to yes-no questions introduced by the complementizer *hvort* 'whether') resist topicalization as illustrated by the impossibility of fronting the adverbial *í gær* 'yesterday' in (4b).

(4) a. Ég veit ekki [hvar kýrin **hefur** staðið í gær]. (Icelandic)
I know not where the.cow has stood yesterday
'I don't know where the cow stood yesterday.'

b. *Ég veit ekki [hvar í gær **hefur** kýrin staðið].
I know not where yesterday has the.cow stood
'I don't know where the cow stood yesterday.' Bobaljik and Thráinsson 1998:48

Since embedded constituent questions are not V2 environments in Icelandic, they therefore provide the necessary environment for investigating whether or not the verb raises to Infl. As Vikner has demonstrated, examples such as the following show that Icelandic displays the French pattern with the verb obligatorily VP-external, i.e., in Infl, in (5a) vs. the ungrammatical *in situ* (5b) and V2 (5c) orders.[5]

(5) Ég spurði ... (Icelandic)
I asked

a. ... [$_{CP}$ af hverju [$_{IP}$ Helgi **hefði** [$_{VP}$ *oft* lesið þessa bók]]].
why H. had often read this book

b. *... [$_{CP}$ af hverju [$_{IP}$ Helgi [$_{VP}$ *oft* **hefði** lesið þessa bók]]].
why H. often had read this book

c. * ... [$_{CP}$ af hverju **hefði** [$_{IP}$ Helgi [$_{VP}$ *oft* lesið þessa bók]]].
why had H. often read this book

'I asked why Helgi had often read this book.' Vikner 1995:139

[3] This of course does not hold if (neutral) subject-initial clauses are IPs not CPs, as first proposed for Germanic generally by Travis 1984, and for Icelandic by Rögnvaldsson 1984. If Travis and Rögnvaldsson are correct, then (1a) does constitute an example of verb movement to Infl.

[4] For Yiddish, the facts are less straightforward. Travis 1984:119 claims that embedded topicalization is not possible with a [+wh] comp, and Vikner 1995 claims that topicalization in embedded questions is "only possible if the *wh*-element immediately following the complementizer *az* is *far vos* 'why'" (p. 74). Diesing 1990 and Ellen Prince (personal communication, 1997) argue that these descriptions are too restrictive and have provided examples with, e.g., embedded topicalization under the wh-word *vos* 'what' (Bobaljik and Thráinsson 1998:49). I have no intention of trying to reconcile the conflicting descriptions of Yiddish here.

[5] Certain types of embedded clauses do show an apparently M.Sc.-like V3 word order, with material intervening between the subject and the finite verb, as originally noted by Maling 1980. See also Sigur›sson 1986, Thráinsson 1986. See Thráinsson 1984, Bobaljik and Thráinsson 1998:64ff for discussion of such examples in the context of verb raising to Infl.

This contrasts with the M.Sc. languages, which do not allow raising of the finite verb to Infl in non-V2 environments. The example in (1b) demonstrates this sufficiently; for the sake of completeness, Danish sentences parallel to (5a-b) are given in (6).

(6) Jeg spurgte ... (Danish)
I asked

a. ... *[$_{CP}$ hvorfor [$_{IP}$ Peter **havde** [$_{VP}$ *ofte* læst den]]].
why P. had often read it

b. ... [$_{CP}$ hvorfor [$_{IP}$ Peter [$_{VP}$ *ofte* **havde** læst den]]].
why H. often had read it

'I asked why Peter had often read this book.' Vikner 1995:145

The conclusion, then, is that Icelandic does have verb movement to Infl independently of the effects of V2, and that the M.Sc. languages do not. The characterization underlying the postulation of (3) was correct, even though demonstrating this turned out to be significantly more complicated than originally assumed.

With these caveats out of the way, we may proceed to the discussion of the RAH and the accounts of it that have been offered.

3. THE RICH AGREEMENT HYPOTHESIS

3.1 The RAH: paradigm-based accounts

The earliest formulations of versions of the RAH within the domain of Germanic verb movement, are, as far as I know, Roberts 1985 (on diachronic changes in English), Kratzer 1984 (comparing German and English),[6] Clahsen 1988 (on the acquisition of German and English) and Kosmeijer 1986 (for the Scandinavian contrasts discussed above). Kosmeijer replicates Travis's analysis of the word order difference in (1) and while he proposes that this be related to the strength of inflection in the various languages, he does not in fact discuss the overt morphological paradigms involved. This extra step is added by Holmberg (1988) and Platzack (1988) (considering diachronic evidence in Swedish), who both suggest the presence versus absence of subject agreement as the morphological characteristic with which to correlate the syntactic variation.[7] Thus, by the end of the 1980s, in parallel with Pollock's (1989) proposal for the French versus English contrast, the RAH in essentially the form in (3), repeated here with its diachronic corollary, took shape.

[6] Cited in Clahsen 1988:52.

[7] Holmberg 1986:90 had already noted the lack of subject agreement in Swedish and the presence thereof in Icelandic in the context of the word order difference in (1), but both Holmberg 1986 and Platzack 1986 treat the word order variation in terms of differing adjunction sites of the adverb, with verb raising to Infl in both Icelandic and Swedish. Note also that Holmberg 1988 does not take the morphological variation to cause the syntactic variation, as Platzack 1988 does. Holmberg subsequently moves to Platzack's position in joint work by these authors.

(3) *The Rich Agreement Hypothesis*

Rich agreement (and only rich agreement) causes V-to-I movement.

Corollary: If a language loses rich agreement, it will lose V-to-I.

As noted in the introduction, central to the discussion since the inception of (3) was the corollary that the loss of verb raising to Infl "is an immediate consequence of the loss of agreement…" (Platzack 1988:216). Since the corollary only follows from (3) if (3) is interpreted in its strong form, i.e., "Rich agreement *and only rich agreement* causes V-to-I movement," it may safely be concluded that this strong hypothesis was implicit in all work which makes reference to the diachronic evidence. We return to this issue in section 4.

As it is stated in (3), the RAH is disarmingly simple and thereby presumably derives much of its appeal. Note that the hypothesis was first proposed during the period in which there was substantial optimism concerning another proposed relationship between "rich" agreement and syntactic variation, namely, the Null Subject Parameter (see the papers and references in Jaeggli & Safir 1989). Defining "rich", though, for the purposes of the RAH turned out to be no easy matter. Important steps in this context in pursuit of such a definition from the paradigm-based perspective include Platzack & Holmberg 1989, Holmberg & Platzack 1990, 1995, Vikner 1990 (revised and included in Vikner 1995), Roberts 1993, Falk 1993, Bobaljik & Jonas 1993, Rohrbacher 1994, Vikner 1997, Rohrbacher 1999 and Koeneman 2000.[8]

The original suggestion was that the simple presence (Icelandic, Faroese, Old Scandinavian) or absence (M.Sc.) of subject-verb agreement was responsible for the contrast (Holmberg and Platzack 1990); this was soon revised such that distinctions in number alone were taken to be insufficient to count as true subject agreement, (Platzack and Holmberg 1989). Though English was not clearly accounted for, the centrality of person (as opposed to number) seemed necessary to account for data from certain non-standard varieties of Swedish and Norwegian, initially presented in Platzack 1988 and Trosterud 1989. Other authors suggested that overt marking for [plural] was the key factor (Roberts 1993:267, to a lesser degree Falk 1993).

The most detailed study of the issue, bringing together synchronic and diachronic evidence and giving consideration beyond the Scandinavian languages, is Rohrbacher 1994 revised and expanded in Rohrbacher 1999. Considering a wide array of languages, including non-standard varieties of M.Sc. languages (and with comparison beyond Germanic), Rohrbacher arrives at the following, essentially descriptive generalization.[9]

(7) The Paradigm Verb Raising Correlate (Rohrbacher 1999:116)

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

[8] An alternative is considered briefly in Trosterud 1989, proposing to link the difference in verb raising to the presence (Icelandic) versus absence (M.Sc.) of overt morphological case marking on nouns. The approach is challenged by those varieties of Faroese which pattern syntactically with M.Sc. yet retain overt case-marking on nouns. Not included in this list are those approaches which are crucially not paradigm-based, beginning with Johnson 1990 and extended in Bobaljik 1995 and Thráinsson 1996; these are discussed in section 3.2.

[9] In a brief consideration of extensions of his theory of verb movement, Rohrbacher 1999, §5.3 extends his notion of lexically listed Infl to the domain of *pro*-drop. This will not be discussed here, though the general considerations in section 4 would seem to apply equally to that domain as well, see Snyder 1995, Sprouse 1998.

This is clarified further as follows:

> "A privative feature such as [1st] or [2nd] is distinctively marked if and only if the forms bearing this feature are distinct from the forms lacking this feature. Accordingly, a language has V to I raising if its regular verbs distinguish the forms for first and second person in at least one number of one tense from each other, as well as from the forms for 'third' person in that tense/number combination and from the form for the infinitive." (pp.116-117)

Rohrbacher's theory of the causality here (i.e., why it is that distinct marking of the [1st] and [2nd] person somewhere in the paradigm should have the effect of forcing verb raising) is stated in terms of whether or not the inflectional affixes are listed individually in the lexicon.[10] If the affixes are so listed, then they are inserted in Infl and verb raising is forced by something like Lasnik's (1981) Stray Affix Filter, otherwise, they are "phonetic spell-outs of feature bundles that are abstractly represented on syntactic nodes" (p129).[11] Note that quite crucially, this choice cannot be made on an affix by affix, or sub-paradigm by sub-paradigm basis, but is made once and for all for the whole language. For present purposes, we may set aside the finer details of Rohrbacher's theory, focussing on the claim that in some specified way, the structure of the paradigm is responsible for verb raising to Infl.[12]

To see how (7) works, consider the present and preterite (i.e., past) tense paradigms of Icelandic, English and Swedish, given in (8).

(8) Icelandic: *kasta* 'to throw' English: *tremble* Swedish: *smaka* 'to taste'

	Present	Preterite	Present	Preterite	Present	Preterite
1 psn sg	kasta	kasta **-ði**	tremble	tremble **-d**	smaka- **r**	smaka- **de**
2 psn sg	kasta **–r**	kasta **–ði -r**	tremble	tremble **-d**	smaka- **r**	smaka- **de**
3 psn sg	kasta **–r**	kasta **–ði**	tremble **-s**	tremble **-d**	smaka- **r**	smaka- **de**
1 psn pl	köst **–um**	köstu-**ðu-m**	tremble	tremble **-d**	smaka- **r**	smaka- **de**
2 psn pl	kast **–ið**	köstu-**ðu-ð**	tremble	tremble **-d**	smaka- **r**	smaka- **de**
3 psn pl	kast **-a**	köstu-**ðu**	tremble	tremble **-d**	smaka- **r**	smaka- **de**

Icelandic meets the basic "distinctness" conditions in the plural number of both the present and the preterite tenses. The [1st] and [2nd] person forms in the plural are distinct from each other, from the third person forms and from the infinitive. Nowhere in the English or Swedish paradigms are these "distinctness" conditions satisfied. Hence, by (7), Icelandic, but not English or Swedish, should display verb movement to Infl, correctly, as we have seen above.

The Icelandic data presented here illustrate an important aspect of the paradigm-driven approaches, mentioned above. While these approaches take the morphology to drive the syntax,

[10] Rohrbacher calls his lexically listed affixes "referential", treating [1st] and [2nd] person as "referential" features.

[11] Koopman 1984:149ff has criticized appeal to the Stray Affix Filter to motivate syntactic movement. She notes that movement to satisfy a putatively phonological property (affixhood) occurs in cases where the putative affix triggering movement is not a phonological affix, e.g., when there is no phonologically overt material (English, French), or when the triggering item stands alone as an independent word (e.g., particle). The relevant notion of "affix" for triggering movement thus is not the morphological or phonological property of being an affix, but rather amounts to the syntactic property of triggering movement, and the appeal is circular.

[12] For recent critiques of Rohrbacher 1994, see Neeleman 1996 and Koeneman 2000.

it is not the morphology of individual verbs that determines how these verbs behave in the syntax, but rather the constitution of the whole paradigm.[13] For example, in the present tense singular forms in Icelandic, the distinctness conditions are not met: the [2sg] form in the present is not distinct from the [3sg]. Similarly in the past singular, the [1sg] and [3sg] forms are identical. Despite this failure to meet the criteria in (7), singular verbs do not behave any differently from plural verbs, which do meet the criteria. Thus, in order to determine whether or not a given instance of an inflected verb will raise to Infl or not, the entire verbal paradigm must be inspected; this is a basic property of the paradigm-based approaches to which we will return, below.

A more striking contrast is provided by two non-standard M.Sc. dialects, Älvdalsmålet Swedish and Hallingdalen Norwegian (Trosterud 1989). While both show more agreement than the standard varieties of Swedish and Norwegian, only Älvdalsmålet showed person distinctions (in the plural) and only Älvdalsmålet had verb raising to Infl.[14]

It is of course not true that all of the theories mentioned at the beginning of this section share all assumptions or postulates with Rohrbacher 1999's proposals. Nevertheless, the fundamental reliance on the internal structure of the paradigm as the ultimate cause of the syntactic differences is explicitly present throughout. Thus, Vikner 1997 proposes that "An SVO-language has V°-to-I° movement if and only if person morphology is found in all tenses" (p.207) and Koeneman 2000 proposes that the affixes in a paradigm are lexically listed (and hence force V°-to-I° movement) if the features [1st] [2nd] and [singular] are each contrastive somewhere in that paradigm (cf. Koeneman 2000:72).[15]

The reliance on counting distinctions within a paradigm, and the notion that it is the overt distinctions within the paradigm which cause the syntactic variation, are at the heart of the paradigm-based theories just outlined. Before turning to a presentation of the major empirical problems for these theories, I will present the basics of the paradigm-free alternative. This allows section 4 to be cast in a more contrastive light, bringing the differences between the two alternatives into that much sharper focus.

3.2 The Paradigm-free accounts

Given a (non-spurious) correlation between two properties, P1 and P2, there are three possibilities for an explanation of the correlation: (i) it could be that P1 causes P2, (ii) it could be

[13] In order to appreciate this subtle distinction, it may help to contrast this with theories involving "strong" vs. "weak" features, e.g., Chomsky 1991. In such theories, the "strength" of the feature is encoded on a particular lexical item or functional head and is inspected during the course of the derivation. The derivation "sees" the strength of the feature and acts accordingly.

[14] All data on Älvdalsmålet appears to originate with Levander 1909; it is not clear from the examples cited that the possibility of embedded verb second has been adequately controlled for. As the Älvdalsmålet and Hallingdalen examples are accessible in almost every work on the topic, I have not repeated them here.

[15] Koeneman 2000's implementation differs quite sharply from that of Rohrbacher 1994, 1999 and Koeneman provides many critiques of Rohrbacher 1994. For the purposes of the present discussion, though, the two theories share the same core aspects; both posit that verb movement (to Infl) is caused by the "referential" (Koeneman's "argumental") status of Infl, a status which is determined by inspecting the paradigmatic contrasts in the inflectional morphology. Note that Koeneman assumes only a one-way implication from morphology to syntax, though, a point we return to in section 4. Note also that Koeneman seeks an account that will not only cover the distribution of verb movement to Infl, but will also account for verb movement to C in the V2 constructions, providing (as Bobaljik and Thráinsson 1998 try to do) a unified mechanism for verb movement.

that P2 causes P1, or (iii) it could be that some third, deeper property (P3) has both P1 and P2 as consequences. In the domain we are considering here (observed correlations between overt inflectional morphology = P1 and verb movement to Infl = P2) it is striking to what degree one of these options (i) has been accepted without serious consideration of the alternatives. (As Rohrbacher noted, this is in a sense especially striking in that the option adopted is not consistent with current theories of morphology.)

That verb movement causes rich inflection (option (ii)) has, to my knowledge, not been explored at all in the literature, though perhaps with good reason. I know of no set of assumptions from which such a causal relation would follow.

This leaves option (iii), the possibility that some deeper parametric divide accounts for both the distribution of verb raising and the observed morphological variations. This possibility is, in fact, the position taken in one of the earliest works on the variation in Scandinavian (Holmberg 1988), though as noted, by the end of that year Holmberg had moved to position (i).[16] Holmberg's original idea is picked up and developed in an intriguing way in Johnson 1990. Johnson's paper is especially noteworthy in that it is, to my knowledge, the first serious attempt to derive the syncretisms observed in the Germanic inflectional paradigms *from* their syntactic structures (rather than the other way around).

3.2.1 The syntax of verbal inflection

At the end of the 1980's, as the connections between morphology and syntax embodied in the Null Subject Parameter and the RAH were being explored, two other seemingly unrelated ideas came to prominence. One was Pollock 1989's proposal to split IP into a more articulated structure, with not just separate heads but also separate phrasal projections for Agreement and Tense. The other was Halle 1990's argument that (inflectional) morphology interprets, rather than feeds, syntax. The syntax is taken to concatenate "abstract morphemes" such as TENSE, AGREEMENT and so on, with the specific, phonological form of the corresponding affixes (including Ø) provided after the syntax by rules of lexical insertion.[17] Johnson 1990 argued (i) that the inflectional domain should be split so as to include separate PERSON-AGREEMENT, NUMBER-AGREEMENT, and TENSE projections, and (ii) that while the hierarchical relation among these elements was fixed by U.G., languages could vary parametrically as to whether or not they had all of these projections.[18] Specifically, he proposed to treat the Germanic variation by arguing that the languages involved varied in the presence (Icelandic) versus absence (M.Sc.) of

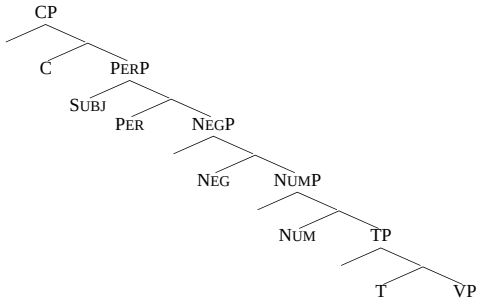
[16] See Holmberg and Platzack 1990, presented in Groningen in May, 1988. In a sense, Holmberg and Platzack equivocate on the important point. Their actual analysis has an abstract feature [+F] (for finiteness) and an abstract element "AGR" (in Infl) responsible for verb raising, and they offer no account of how the abstract element "AGR" should be connected to overt morphology. (Indeed, they seem to deny the necessity of any connection to morphology in nn. 7-8 of Holmberg and Platzack 1990, and elsewhere.) Despite this, in the text of both Holmberg and Platzack 1990 and Holmberg and Platzack 1995 they fairly consistently take their aim to be to show that "the major syntactic differences between I.Sc. and M.Sc. can be described as the effect of two parameters involving the *morphological* differences regarding subject-verb agreement and morphological case" (Holmberg and Platzack 1995:10, emphasis mine -JDB).

[17] This view of the relationship between morphology and syntax does not originate with Halle 1990, but can be traced back through the earliest generative work on the topic, see, for example, Bierwisch 1967, Chomsky & Halle 1968. Halle's 1990 paper however, brought the ideas back into the discussion after many years of neglect.

[18] That languages might vary with respect to their inventories of functional projections arose as a hypothesis almost immediately after the proliferation of functional projections that marked the end of the 1980s. Johnson's proposal, and the similar proposals considered in section 3.2.2 are important for our concerns for the way in which they connect this approach to parameterization to the morphology.

the PERSON-AGREEMENT (and in some cases NUMBER-AGREEMENT) phrase in the structure in (9).[19]

(9) Johnson's structure (p.6)[20]



In all the languages, the verb is taken to raise to the highest head. In Icelandic, this means that the verb would raise to the head of PERP, placing it in front of negation as in (1a), while in the other varieties, the verb would raise only as far as the head of NUMP or TP, thus following negation as in (1b). The syntactic variation, (presence or absence of particular functional projections) was, Johnson argued, responsible for both the verb raising patterns and the patterns seen in the inflectional morphology. If a language lacked PERP in the syntax, no subsequent morphological rule could provide a phonological instantiation for the head of that phrase—the language would lack person agreement. The causation here is the opposite of that expressed in the RAH; while the morphological evidence can be used to detect syntactic differences, the morphology is not itself the cause of those differences.

The particular details of Johnson's proposals suffered from certain drawbacks. For one thing, he offered no account of the position of the (pre-negation) position of the subject in a clause lacking PERP (1b). For another, he related the verb movement parameters to the simple presence versus absence of person agreement, a description now known to be inadequate. Finally, he sought to account for various morphological syncretisms in the languages considered through appeal to U.G., mechanisms which in some cases turned out to be too strong. For example, Johnson's system forces a syncretism between 1st and 3rd person everywhere except the present tense singular (see p.21, 29). Such syncretism is attested (even for the verb *be*) in English, the Mainland Scandinavian languages, Faroese and German, but we note that Icelandic systematically distinguishes 1st and 3rd person in the plural of both present and past (preterite) tenses (see (8)).

Note that the problems that Johnson's proposal faced are consequences of his specific implementation of the general assumptions and not a necessary consequence of those assumptions. Thus, details aside, Johnson's paper should have set the challenge: could the morphological patterns themselves be explained in terms of the syntax?

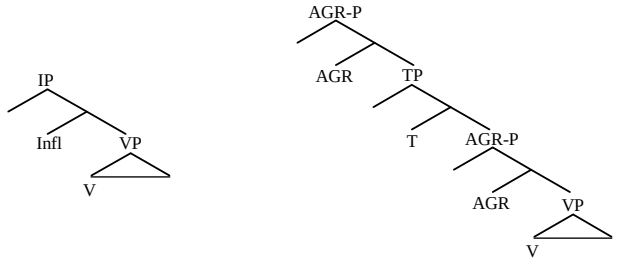
[19] Note that Rohrbacher 1999 departs from Rohrbacher 1994 in assuming, with Johnson 1990 and Bobaljik 1995 that the M.Sc. languages lack an Agreement Phrase. However, Rohrbacher's theory is still very firmly couched in the viewpoint of morphology-driven projection, in contrast to the approaches in this section.

[20] Johnson 1990 also has a Mood Phrase and an Addressee Phrase (distinguishing second person agreement from person agreement). These would complicate the exposition in ways that do not affect the point to be made here.

3.2.2 Agreement and Tense

Following a suggestion from David Pesetsky, Bobaljik and Jonas 1993 explored the possibility that the distinguishing characteristic of the morphology should not be formulated in terms of counting fine distinctions in the paradigms involved, but rather might have to do (more or less as Holmberg and Johnson had proposed) with the inventory of functional elements or projections that a language might make use of. Following Johnson's lead, though adopting more familiar phrase structure representations, Bobaljik 1995, Thráinsson 1996 and Bobaljik and Thráinsson 1998 propose what Thráinsson dubbed the *Split IP Parameter* (SIP): that languages may vary, syntactically, as to whether they have a pre-Pollockian, unsplit IP as in (10a), or an IP containing Agreement Phrases distinct from Tense, as suggested in Chomsky 1991 (10b).[21]

(10) a. b.



Bobaljik and Thráinsson 1998 note that there are a series of fairly straightforward consequences of assuming such a parameter, both for the syntax and for the morphology. These can be divided into three types:

(11) Consequences of a Split Infl:

a. More specifier positions in (10b) than (10a),
b. Non-local relations among "Infl-type" heads in (10b), and
c. More terminal nodes in (10b) than (10a).

These consequences may play out in a variety of ways. For example, on the assumption that each phrase has maximally one specifier position, a language with a simple IP structure as in (10a) should have only one specifier position in the IP domain, i.e., the region between CP and VP.[22] This contrasts with languages that have the more articulated IP structure in (10b); all else being equal, this should allow more arguments to appear in the IP domain. One might expect two VP-

[21] Note that on this proposal, (10a) is taken to have a single terminal node, and cannot be "doubly-headed" as in proposals of the early-1980s, c.f., Chomsky 1981:52. Note that even proposals which took Infl to be doubly-headed in some sense, typically treated the category as a single head for the purposes of, e.g., the Head-Movement Constraint; see Travis 1984:130ff.

[22] The theory therefore rejects the proposals of, e.g., Cinque 1999 regarding the universal inventory of functional heads and in particular the placement of adverbs among these. For some critique of Cinque's proposal concerning adverbs, see Bobaljik to appear; for critique of some of Cinque's arguments regarding verb positions, see Wurmbrand to appear.

external subject positions (Spec,AgrSP and Spec,TP), and perhaps a VP-external position for full DP objects. Bobaljik and Thráinsson 1998 claim that this is the correct interpretation of the evidence for multiple subject positions in, e.g., Icelandic and German as compared with the Mainland Scandinavian languages and English, as presented in Diesing 1992 for German and Bobaljik & Jonas 1996 for a wider range of languages. By using a class of adverbial elements taken to diagnose the left edge of TP (rather than VP, cf. Jackendoff 1972, Travis 1988) these authors have shown that subjects may surface in one of two positions in Icelandic and German (with interpretive consequences to the difference), while this is not true of, e.g., English or the M.Sc. languages. In essence, as the Icelandic examples below illustrate, subjects reflecting old information occur to the left of a TP-adverb, in Spec,AgrP (12a), while those reflecting new information occur to the right of such an adverb (12b) yet still external to the VP.[23]

(12) a. Í gær kláruðu {þessar mýs} sennilega *{þessar mýs} ostinn. (Ic.)
yesterday finished these mice probably these mice the.cheese
'These mice probably finished the cheese yesterday.'

b. Í gær kláruðu {?margar mýs} sennilega {margar mýs} ostinn.
yesterday finished these mice probably these mice the.cheese
'Many mice probably finished the cheese yesterday.'

Bobaljik and Jonas 1996:196

Other evidence taken to support the additional specifier positions in Icelandic as opposed to the M.Sc. languages, as discussed by Bobaljik and Thráinsson (1998), includes the cross-linguistic distribution of Transitive Expletive Constructions and the possibility of Object Shift of full DP arguments, constructions much discussed in the relevant literature, thus subsuming the principle content of the Spec-TP Parameter of Bures 1993, Bobaljik and Jonas 1996 and Jonas 1996b.

In addition, Bobaljik & Thráinsson develop the account of verb movement presented in Bobaljik 1995:chapter 5, demonstrating that it follows from the parametric variation posited in (10) on fairly straightforward assumptions. Specifically, assuming that movement is driven by some notion of "feature-checking", it is observed that the configuration in (10a) allows for feature checking between Infl and the verb without movement, as the two are in a local relationship.[24] In (10b) however, only the lowest functional head is in a local relation with (the maximal projection of) the verb, hence some degree of verb movement is required for feature checking with higher heads. Thus, the distribution of verb movement follows from the Split Infl Parameter, and is therefore (essentially correctly) predicted to co-vary with the other diagnostics for Split IPs.[25]

Finally, in recognizing that it is the terminal nodes of the syntax that correspond to abstract morphemes which may be the locus of lexical insertion, the Split Infl Parameter predicts that languages with only one inflectional head will be limited to one inflectional affix[26] after the verb

[23] There is a good deal more to be said here. In particular, it can demonstrated fairly straightforwardly that the lower subject position is, indeed, VP-external on the relevant assumptions. This was first noted by Jonas 1992. See Bobaljik and Jonas 1996, Bobaljik and Thráinsson 1998 and references cited therein for further discussion. See also Vangsnes 1998 for a more careful discussion of the interpretive characteristics of the subject positions in Icelandic.

[24] The most local relationship, in fact, i.e., sisterhood. This assumes that the head and its maximal projection share features (or are not distinct), an assumption implicit in work appealing to Specifier-Head feature checking.

[25] Of the properties considered, only verb raising is actually forced by a split IP, yielding a bi-conditional. All other diagnostics are merely potentiated by a Split IP. See Bobaljik and Thráinsson 1998, and section 4 below for some discussion.

[26] This requires that the mechanisms of Fission (Noyer 1997) and the post-syntactic epenthesis of Agr heads (Halle & Marantz 1993) be precluded.

stem while languages with a split IP will allow more.[27] This is correct. In a language like Icelandic, verbs may bear discrete markers of tense and agreement, as in the past plural forms in (8) (e.g., *köstu-ðu-m* 'throw-PAST-1PL').[28] In English, by contrast, tense and agreement are in complementary distribution (hence *tremble-s* with 3sg agreement, and *tremble-d* with past tense marking, but **tremble-d-s*).

It is important to stress exactly what implications for morphology fall out from the SIP. The structures in (10) constrain the maximal number of overt inflectional affixes which may surface on the finite verb in a given language. Given the existence of zero morphemes, no implications are made as to the lower bound. Thus, a particular finite Icelandic verb form may well be followed by no overt identifiable affix (cf. the first person singular of the class of verbs represented by *kasta* 'to throw'). What is claimed only is that *in order to have more than one identifiable inflectional affix on a verb stem* a language must have a split IP.[29] Quite importantly, no implication is made from morphological paucity—if the inflected verbs of a language never show more than one affix, it does not follow that this language necessarily has an unsplit IP. I will return to the relevance of the one-way implicature derived from this theory presently.

In sum, the theory set forth in Bobaljik and Thráinsson 1998 takes the primitive parametric variation to be in the pre-syntactic "bundling" of syntactic features into a single inflectional head in some languages, versus the distribution of these features across a range of heads in others. Five properties of the Germanic languages which generally cluster together are diagnostic of a Split IP; while any one of them may thus, in principle, may be a "trigger" for this parameter in acquisition (see below), none of these properties (including the morphology) "causes" the IP to split any more than puddles on the street "cause" rain. The properties are:

(13) a. the availability of two subject positions between CP and VP
b. the possibility of transitive expletive constructions
c. the availability of a VP-external derived object position
d. obligatory raising of the verb to Infl in non-V2 environments
e. the possibility of multiple inflectional morphemes on the verb stem

In the next section, I turn to a direct comparison of properties of paradigm-free approaches with the corresponding properties of the paradigmatic approaches.

[27] For the particular structure posited in (10b), it is thus a somewhat embarrassing accident that no Germanic language shows overt object agreement morphology. It is likely that the nodes have been mislabelled; e.g., German and Icelandic retain a distinct subjunctive inflection (cf. Pollock 1997 on French versus English) and it is arguably true that some forms simultaneously bear discrete markings for Past, Subjunctive and Agreement. If this is the correct labels for the three functional projections, though, the connection to Object Shift remains somewhat unclear. Note that I take the marker of the "s"-passive in, e.g., Swedish to be a clitic, along with its cognate *–st* in Icelandic, *-sja* in Russian, *se* in Romance, etc.

[28] In contrast to, e.g., Johnson 1990, Bobaljik & Thráinsson do not take Number to be a discrete head in either the syntax or the morphology; see Bobaljik and Thráinsson 1998:59, n.25, Bobaljik 1995:47, n.22. This is quite relevant to the discussion of Faroese below.

[29] This imprecision led Bobaljik 1997 (essentially Chapter I of Bobaljik 1995) to propose that the overt morphological distinctions filter the syntax, a position from which I have since retreated. Note though that even that position did not appeal to paradigms and their internal structure, merely to language particular constraints on what may be called the Autonomous Morphological Structure (Noyer 1997).

4. THE IMPLICATIONS OF A ONE-WAY ENTAILMENT

In this section, I will develop an argument against the RAH and in favour of approaches such as those in section 3.2. The shape of the argument is reasonably straightforward (even if the details are nevertheless complex). In the first place (section 4.1.1), it is shown that a bi-conditional correlation between morphology and syntax (such as (7), or that in Vikner 1997) is empirically untenable.[30] Four cases are examined in which syntactic variation is observed in the absence of morphological variation. These cover a spectrum from synchronic dialect variation, historical evidence, speaker variation, and variation among constructions within one language. While a bi-conditional generalization is thus rejected, a one-way implication is statable, as in (14).

(14) If a language has sufficiently rich morphology then it has verb raising.

Note that (14) does not license any inference from verb raising to morphology, nor does it license any inference from "poor" morphology. Specifically, it does not entail (as (7) does) that a language with few morphological distinctions—however characterized—will lack verb raising to Infl. After showing that the data supports (14), I will show that this particular state of affairs follows from the theory presented in Bobaljik and Thráinsson 1998. Moreover, I will show that despite the fact that various authors accept the validity of (14) over (7), adopting a morphology-driven approach based on a one-way implication as in (14) (as in Vikner 1995 and Koeneman 2000) is a serious weakening of their theories. As noted by Rohrbacher (1999:142, 149 n.17), this position risks the vacuity inherent in a claim of the form that verb movement is caused by morphology when it is caused by morphology, and is caused by something else when not caused by morphology.

The facts to be discussed here are not novel, and with the exception of the Faroese data, and to some degree the Icelandic infinitives, I am unaware of any substantial debate as to their accuracy or analysis. Previous authors have typically addressed them in one of two ways. Some authors have suggested that since (7) is untenable, there are no (synchronic) generalizations to be made and that all attested correlations are mere accidents of history and of the data (e.g., van Gelderen 1997:13, Alexiadou & Fanselow 2000). Others (Rohrbacher 1999, Vikner 1997) have left them as unresolved problems, on the premise that no alternative would fare any better. We now know that there is an alternative, presented in some detail in Bobaljik and Thráinsson 1998 but for which the crucial ingredients were already made explicit in Johnson 1990.

4.1 Syntactic variation, morphological uniformity

4.1.1 Dialect variation

The dialect of Swedish spoken in Kronoby, Finland is reported in Platzack and Holmberg 1989 to have no person or number distinctions at all (i.e., morphologically it is like standard Swedish), yet it provides a minimal contrast with the standard M.Sc. languages in having evidence for verb movement in embedded clauses. In (15), the finite embedded verb precedes negation, like in Icelandic and in contrast to the standard M.Sc. languages (see (1)).[31]

[30] Vikner 1995:135 explicitly rejected a bi-conditional in favour of a one-way implicature, largely in view of the empirical considerations discussed here, but in Vikner 1997:207 the correlation is elevated to an "if and only if" statement with no account of the problematic cases offered.

[31] While this example has been repeatedly cited, there is precious little data from this dialect. To my knowledge, it has not been investigated whether this is verb movement to Infl or a case of embedded verb second, cf. the discussion of Icelandic in section 2.

(15) He va bra et an **tsöfft** *int* bootsen. (Kronoby Swedish)
it was good that he bought not the.book
'It was good that he didn't buy the book.' Platzack and Holmberg 1989:74

If the Kronoby data is accurately described, then the bi-conditional is indefensible. Note that, since this involves verb raising in a language with "poor" inflection (the standard M.Sc. paradigm does not have person agreement and tense simultaneously), the weaker (14) is not compromised.[32] Rohrbacher 1999:120 leaves the Kronoby data as "an unresolved problem for [his] approach" as does Vikner 1997:211, n.19.

4.1.2 Diachronic variation

As noted above, Platzack 1988 and others subsequently argued for a causal link between the loss of agreement morphology and the loss of verb movement in the development from Icelandic-like Old Scandinavian to the modern Mainland Scandinavian languages. Curiously, there is often a lag of some one- to two-hundred years or so between the dates accepted for the morphological change and the dates accepted for the word order change.[33]

Vikner 1997:205ff notes this problem with data from the development of Danish. Thus, by about 1350, Middle Danish had lost person agreement, retaining essentially the morphological paradigm of (modern) Hallingdalen Norwegian (number agreement in the present). Yet while Hallingdalen lacks verb movement (supposedly as a consequence of (7) or something like it), Middle Danish at least as late as 1543 still has verb movement to Infl, as indicated by the order in (16).

(16) Lader oß nu see om ui **haffuer** *nogen tid* hört guds ort […] (Middle Danish)
let us now see if we have any time heard god's words
'Let us now see if we have ever heard god's words…' Vikner 1997:206[34]

Vikner recognizes that this is a problem for his theory, though offers no solution. It bears mentioning that the "lag" problem is quite significant if one accepts that generative grammar is concerned with the grammars of individuals and the acquisition of these by children (see, e.g., Hale 1998 for discussion of this point). If a bi-conditional such as (7) is a part of (or derivable from) U.G., then the acceptance of a "lag" would force the conclusion that for approximately 200 years, whole generations of Scandinavians were born, lived and died speaking something that was not, technically speaking, a language. I assume that this is not seriously entertained by proponents of the RAH and that there must be implicit assumptions about the nature of the data. Note that the lag problem does not arise for theories such as Bobaljik and Thráinsson 1998; there is for that approach no principled difference between Kronoby Swedish and Middle Danish.

[32] Rohrbacher 1994 is often cited as suggesting Finnish influence as the source of verb raising in this dialect, though he in fact noted that while "it is at least conceivable" to attribute the verb raising to "influences from Finnish", this "is an unattractive solution since it seriously weakens the restrictiveness of the theory" in the ways indicated below (Rohrbacher 1994:112-113).

[33] One may imagine objecting to the relevance of these examples as Rohrbacher 1999 does on methodological grounds, of particular relevance being the possibility of that the data "may reflect conservative tendencies in the written language rather than an actual discrepancy in the spoken language" (Rohrbacher 1999:177). I include the examples for the sake of argument accepting the assumptions regarding dating and significance of the sources from which they are taken, showing that on these assumptions, there is a problem for the conclusions they draw.

[34] Vikner attributes this to the 1543 text of Peder Palladius's: *En Visitatz Bog*, cited in Jacobsen 1925.

There is the question of why the descendants of Middle Danish speakers speak a language with the characteristics of Modern Danish, though given that languages like Kronoby Swedish persist, there can be no claim that the loss of verb movement is a necessary consequence of the loss of agreement.[35]

4.1.3 Faroese

Modern Faroese has been the subject of much discussion throughout the development of the RAH. Regular (weak) Faroese verbs display the inflectional paradigm in (17).

(17) Faroese: *kasta* "throw" (Lockwood 1964:76)

	Present	Preterite
1 psn sg	kast **-i**	kasta **-ði**
2 psn sg	kasta **-r**	kasta **-ði**
3 psn sg	kasta **-r**	kasta **-ði**
1 psn pl	kast **-a**	kasta **-ðu**
2 psn pl	kast **-a**	kasta **-ðu**
3 psn pl	kast **-a**	kasta **-ðu**

While there are some person distinctions in the present tense singular, the only distinction evidenced in the past tense is one of number (to which we return in section 4.1.5).[36] Faroese has been a thorn in the side of most analyses, since, as noted as early as Barnes 1987, both the Icelandic order (18a) and the M.Sc. order (18b) are attested, and individual speakers accept and produce both.

(18) a. Tey nýttu fleiri orð, sum hon **hevði** *ikki* hoyrt fyrr. (Faroese)
they used several words which he had not heard before

b. Tey nýttu fleiri orð, sum hon *ikki* **hevði** hoyrt fyrr.
they used several words which he not had heard before
'They used several words which he had not heard before.' (Barnes 1987:4)

Further investigation of literary examples, elicitations from native speakers, and the work of Faroese linguists has shown that the situation is rather complex, and that while all speakers apparently allow the M.Sc. order, there is a good deal of variation regarding the status of the Icelandic order (see especially Jonas 1996a and Thráinsson et al. in prep). Nevertheless, what can be said with certainty is that there is Faroese-internal syntactic variation regarding verb movement to Infl in the absence of corresponding morphological variation in the paradigms of the regular verbs.[37]

[35] For Rohrbacher 1999, Middle Scots English (15th C) poses a similar problem, retaining verb movement past negation in the absence of morphological distinctions that satisfy (7). Rohrbacher (p. 170) leaves this also as an unresolved problem. Note that Middle Scots English does show (1st) person agreement in the past tense and thus is not problematic for Vikner 1997 or Bobaljik and Thráinsson 1998.

[36] There is an archaic ending *–st* in the second person preterite singular of strong verbs, though apparently this is now a feature of the written language if used at all (Lockwood 1964:81, Jonas 1996a:118).

[37] Vikner's most current response to the Faroese situation is the claim Vikner 1997:209, n.2 that "the colloquial Faroese of speakers born after *c.* 1960" patterns with M.Sc. in not allowing verb movement to Infl, and thus, that his theory "only … valid" for that variety. Whether or not this is an adequate description of the variation, it leaves as mysterious the varieties of Faroese spoken by those over 40 years of age (who nevertheless have the morphological paradigm in (17)).

In any theory in which the triggers (be they syntactic or morphological) for verb movement to Infl leave no room for optionality, observed optionality in the data (as in (18)) can have only one account, namely, the postulation that speakers who accept/produce both patterns are bi-dialectal. I take this to be the content of the so-called "Double-Base Hypothesis" (e.g., Kroch 1990) and can see no alternative.[38] Strong support for the dialect split hypothesis about Faroese comes from Jonas's work, as she has shown that there is a correlation between accepting sentences with verb movement to Infl and accepting other constructions indicative of having a split IP, including transitive expletive constructions and multiple VP-external subject positions (Jonas 1994, 1996a:106, 115).

For theories accepting the one-way implication in (14), then, the data provide no immediate challenge; there is a dialect of Faroese which (like M.Sc.) prohibits verb raising, and a variety, like Icelandic, in which verb raising to Infl is required (as suggested by Barnes 1987). Some speakers simply happen to speak both dialects.[39] There is no (relevant) morphological difference between the dialects, but as Faroese has arguably poor morphology (see below, section 4.1.5) this is no different in principle than the data discussed in sections 4.1.1 and 4.1.2 above.

Rohrbacher 1999 :141ff accepts the bi-dialectal analysis of Faroese, though unlike other theories, accepting this does not remove the problem posed by speakers who allow verb movement to Infl. Like the speakers of Kronoby Swedish, Middle Danish or Middle Scots English, those speakers of Faroese who allow verb raising are in violation of (7) (even if they also speak another dialect, conforming to (7)). Acceptance of the morphology-driven approach entails that there must be a morphological correlate for the verb-raising dialect. Rohrbacher suggests such a correlate in the twenty or so verbs of modern Faroese which have a conjugation differing minimally from (17), as exemplified in (19).

(19) Faroese: *ger-a* 'to do (Lockwood 1964:80)

	Present	Preterite
1 psn sg	ger **-i**	gjør **-di**
2 psn sg	ger **-t**	gjør **-di**
3 psn sg	ger	gjør **-di**
1 psn pl	ger **-a**	gjør **-du**
2 psn pl	ger **-a**	gjør **-du**
3 psn pl	ger **-a**	gjør **-du**

The verbs of this conjugation have an additional suffix *–(r)t* for the [2sg], bringing this paradigm in line with (7). Rohrbacher's tentative account is that children acquiring Faroese "build their

[38] Where the term "dialect" is intended quite loosely, and is intended to subsume other labels such as "register". That many speakers control more than one speech variety, each with its own grammar, seems to me to be difficult to reject, though note that "mixing" the two often yields unacceptable sentences. My native variety of English has double negation "I didn't talk to no-one" and I do control a variety of English with negative inversion "On no account would I talk to anyone", but never the twain shall meet: *"On no account would I talk to no-one" (on intended simple negation interpretation).

[39] Koeneman 2000:84 observes that this characterization alone is perhaps insufficient, since as Jonas 1996a:95 notes, the speakers she consulted who accept verb movement, also accept the unmoved order. While these speakers may be bi-dialectal, the question arises as to whether verb movement to Infl is obligatory for any speaker. Relevant to this question may be the results of text studies, which show, for example, that the author Heðin Brú (b.1901) consistently uses the verb-adverb/negation order in embedded clauses (Sandqvist 1981, Barnes 1992, Jonas 1996a:96).

primary… linguistic system [i.e., grammar JDB] on the basis of the regular and incomplete agreement paradigms like the one in [(17)]" (p.143). Subsequently, speakers faced with the paradigm in (19) might "buil[d] a secondary linguistic system" (p.144). Effectively, the claim is that children have set their parameters based on one set of verbs, and at some point subsequent to the period in which positive evidence effects a change in grammar, they are exposed to verbs morphologically incompatible with their grammar and posit a second dialect. They may then, according to Rohrbacher, use any verb in either dialect (since verb movement to Infl is not restricted to only those verbs with a richer paradigm).

This account is questionable for a number of reasons,[40] and in the end, it appears, untenable on empirical grounds. Jonas 1996a:117ff discusses Rohrbacher's proposal explicitly and notes that in the southern dialect of Sandoy (in fact, the dialect of Heðin Brú, see fn. 39), where verb to Infl movement is prevalent, the [2sg] forms have lost their distinctive marking and collapsed together with the [3sg]. Indeed, Rohrbacher (p.154, n.29) notes that his source (Lockwood 1964) had already indicated than the crucial *–t* suffix was often omitted in the spoken language. For these speakers, Rohrbacher's account is thus unavailable.

In sum, even though there may be some morphological variation among Faroese speakers, and even though the variation has not yet been adequately described, it is nonetheless true to say that for some set of Faroese speakers, there is syntactic variation regarding verb movement to Infl (versus the absence thereof) in the absence of attested morphological variation. Once again, this involves variation in an (arguably) poor system (acceptable under (14)), but is a direct counter-example to existing bi-conditionals.

4.1.4 Icelandic infinitives

The final example of syntactic variation in the absence of morphological variation comes from infinitives in Icelandic. As demonstrated at least as early as Thráinsson 1984, some infinitival complements in Icelandic show evidence of verb raising out of the VP. In the complements of certain control verbs, the infinitive precedes the elements with which we have been diagnosing verb movement throughout, e.g., sentential negation, as illustrated in (20).

(20) a. María lofaði að **lesa** *ekki* bókina. (Icelandic)
Mary promised to read not the.book
'Mary promised not to read the book.'

b. *María lofaði að *ekki* **lesa** bókina.
Mary promised to not read the.book
'Mary promised not to read the book.' Sigurðsson 1989:50

As noted by Holmberg (1986:156) this contrasts with ECM and raising complements, which apparently do not show verb raising to Infl (see especially Sigurðsson 1989 and Thráinsson 1993 for subsequent discussion).[41]

(21) a. *Ég tel Jón **hafa** ekki lesið bókina. (Icelandic)
I believe J. have not read the.book
'I believe Jón not to have read the book.'

b. Ég tel Jón *ekki* **hafa** lesið bókina.
I believe J. not have read the.book
Holmberg 1986:156 (paraphrase added)

(22) a. *María virtist **lesa** *ekki* bókina. (Icelandic)
M. seemed read not the.book
'Maria seemed not to read the book'

b. María virtist *ekki* **lesa** bókina.
M. seemed not read the.book
Holmberg 1986:156

While questions remain about the exact bracketing of these examples (see fn. 41), the ungrammaticality of the (21a) and (22a) indicates that verb raising to Infl, across sentential negation or adverbs, does not take place within these infinitival complements, in contrast to the infinitival complements of control verbs, seen in (20). Importantly, there is no morphological contrast between the infinitive verbs in the two types of complements.[42] Once again, a syntactic

[40] Note that many of the verbs involved are verbs which one might presume are quite common in the Primary Linguistic Data, including *fara* 'go/travel', *gera* 'do', *spyrja* 'ask', *vera* 'be', *fáa* 'take/get', *goyggja* 'bark', *síggja* 'see'. Note moreover that there is no indication that speakers who do not permit verb raising avoid these verbs in any way. Clearly, one may speak a language which contains these verbs and which lacks verb movement to Infl. Simple exposure to (and acquisition of) the paradigm in (19) is therefore not a sufficient condition for positing a second dialect.

[41] The full force of the (b) examples in (21) and (22) remains unclear to the extent that one cannot exclude the possibility in the (b) sentences that negation is in the matrix clause, with the ECM subject having undergone object shift to the matrix Spec,Agro-P, as pointed out (with allowances for terminology) in Sigurðsson 1989:86. As Sigurðsson notes, to control for this effect requires using compound tenses, in which Object Shift is impossible (this is the effect known as Holmberg's Generalization, see Bobaljik 2000a for one account). Curiously, the data becomes extremely murky. Sigurðsson 1989 notes that the test cases do show the relevant contrast, with the order Adverb/Negation^Verb, as in (ia) and (iia) "better than" the raising order (ib), (iib). And while Sigurðsson does take these to indicate that the verb does not raise to Infl, he is clearly loathe to draw any firm conclusions, stating that he "would certainly not want to base any conclusions about Icelandic raising infinitivals on these elusive data" (p.87).

(i) a. ??Ég hafði talið [hana *varla* **mundu** lesa bókina]. (Icelandic)
I had believed her hardly would read the.book
b. *Ég hafði talið [hana **mundu** *varla* lesa bókina].
I had believed her would hardly read the.book

(ii) a. ??Hún hafði virst [*ekki* **vilja** snerta matinn].
she had seemed not want touch the.food
b. *Hún hafði virst [**vilja** *ekki* snerta matinn].
she had seemed want not touch the.food
Sigurðsson 1989:87

Despite this caveat, the argument in the main text is still valid; if verb raising to Infl were possible in all infinitivals in Icelandic, then (21a) should be grammatical, on a par with (21b), contrary to fact.

[42] Though note that the control complements have the infinitival marker *að* (homophonous with the complementizer), lacking in the ECM and raising constructions. Thráinsson 1993 argues that this is beside the point, noting that some raising constructions (in particular, with modal verbs—see Wurmbrand 1999) have the infinitival marker, yet still lack verb raising. The following minimal pair is offered by Thráinsson to indicate that the contrast is independent of the presence of *að* (though see Thráinsson 1993:200, n.21).

(i) Risarnir lofa [að **éta** *oft* [$_{VP}$ t_{verb} ríkisstjórnir]. (Icelandic)
The.giants promise "to" eat frequently governments.

difference is attested in the absence of a corresponding morphological difference, and once again, the forms in question show verb movement to Infl in the absence of "rich" morphology.[43]

Now, the early account of differences among classes of infinitivals was to assume that they involved different amounts of functional structure. Some infinitival complements were bare VPs, others IPs and still others full clauses, and this is more or less what is assumed by most authors who have addressed the problem (see Thráinsson 1993 and Johnson & Vikner 1994 for recent treatments, with references to earlier approaches). Wurmbrand to appear has recently provided careful and persuasive arguments not only that it is possible to treat some infinitival complements as VPs and others as more clausal in their structure, but that such differences in structure are required in order to explain differences in the licensing of long-passive and unaccusative movement in German, the distribution of anaphors in infinitival complements, and a range of other phenomena.

Recalling that the Bobaljik and Thráinsson 1998 account posits that it is the architecture of the clause above the VP (the presence of more than one functional projection) that is the cause of verb movement, the observed differences among different classes of infinitivals would appear to be directly amenable to this standard account (though details remain to be worked out). All and only those infinitival complements with more than one functional projection above VP (or vP) will show verb raising. To suggest that this is indeed on the right track, Bobaljik and Thráinsson 1998 note that control complements allow object shift (another indicator of a split IP) while other infinitival complements do not (see Thráinsson 1993). From a different perspective, given that different types of infinitival complements are of different "sizes", with control complements involving the most functional projections, the Bobaljik and Thráinsson 1998 approach predicts a priori that there will be differences in verb movement of the sort just discussed.[44]

Rohrbacher 1999:80 also accepts that the proper analysis of Icelandic infinitives may have to do with differences in the inventory of functional projections, and apparently sees no problem in adapting existing proposals to his theory. This is, however, not so innocuous an assumption. In Rohrbacher's account, the presence or absence of a "referential" Agr projection triggering verb movement is a direct consequence of the overt morphology. On this, he is quite explicit, claiming at more than one point that the "central claim … is that all syntactic parameters are set

(ii) *Risarnir eiga [að **éta** *oft* [$_{VP}$ t_{verb} ríkisstjórnir].
The.giants ought "to" eat frequently governments.
Thráinsson 1993:199

[43] For the morphology-driven approaches, the data in this section constitute a problem regardless of whether the verb movement parameter is set on the basis of individual (classes of) verbs—in which case (20) is a problem—or the parameter is set on the basis of the language as a whole—in which case (21) and (22) are problematic.

[44] Johnson and Vikner 1994 accept that the verb is in V in the infinitival complements without verb movement, but propose that when the verb moves, it moves to C°, past Infl. They argue that this is, in essence, a reflex of the V2 property and thus falls under the larger problem of explaining verb second, a problem potentially beyond the scope of Rohrbacher's and Vikner's proposals, if the RAH is only about verb movement to Infl. In essence, Johnson and Vikner's theory is that the infinitival marker *að* in Icelandic (but not its cognate *att* in Swedish) occupies C° and is thus in a position to govern PRO, if PRO is in Spec,IP in a control complement. To avoid the violation of the PRO theorem, they rely on the analysis of embedded V2 in Icelandic which invokes CP recursion (Vikner 1995), and they propose that PRO occupies a lower Spec,CP. Assuming a Barriers-like framework, they suggest that CP, unlike IP, is a barrier for government and thus PRO is licit in the lower Spec,CP. They argue further (p.68) that "a completely empty C° always attracts the verb." Note that this is not exactly true; in particular, there is no verb raising to C° in embedded questions in (most or all) Germanic languages, including Icelandic (note the ungrammaticality of (5c), above). Note moreover that the context that they posit for CP-recursion (irrealis complement) is one which Iatridou & Kroch 1992:15 argue is incompatible with CP-recursion on semantic grounds. These comments do not invalidate their theory, but they indicate that important aspects are left open.

exclusively on the basis of the concrete (i.e., phonetically perceptible) content of functional categories" (p.7). At no point does Rohrbacher indicate what phonetically perceptible elements distinguish the functional categories in, e.g., the control versus modal examples in fn. 42.[45]

To sum up, in the previous four subsections we have seen four sets of examples, drawn from the same body of evidence which is taken to support the RAH, all of which attest to the existence of syntactic variation in the absence of morphological variation. I have shown that, while some examples may potentially be explained away, in each case after considering Rohrbacher's objections, the pattern remains. That this situation occurs at all (let alone repeatedly) should in essence be a solid refutation of any bi-conditional statement such as (7).

Now, it would be an error of logic to conclude that since the bi-conditional in (7) is falsified, there are no linguistically significant generalizations to be made. Indeed, I have offered one such generalization in (14), and more importantly, I have shown that exactly the generalization which is arguably unfalsified (though not unfalsifiable) follows from the approach to morphology-syntax interaction presented in Bobaljik and Thráinsson 1998, modulo a concern about Faroese, to which we return presently. After the discussion of Faroese, there is one further aspect of (14) to be discussed. At least two authors, Vikner 1995 and Koeneman 2000, have noted that (7) is unsustainable, accepting (something amounting to) (14) in its place. Both authors have nevertheless maintained the RAH, i.e., the notion that the overt morphology causes verb movement. In section 4.2, I show why this is a very difficult position to defend, especially as compared to the one presented in 3.2.2.

4.1.5 Aside – do Bobaljik and Thráinsson fare any better with Faroese?

In the previous section, I have claimed that all the exceptions to the bi-conditional (7) involve verb movement to Infl in the absence of "sufficiently rich" morphology. In particular, I claimed that there are no cases of the absence of verb movement in a language, variety or construction in the verb may bear more then one overt inflectional affix. For all of the cases discussed except Faroese, this is evident upon cursory inspection of the relevant data. For Faroese, the claim relies on the segmentation of the past tense indicated in (17). Thus, even though there is a vowel alternation in the past tense (weak: singular *-ði*, plural *-ðu*, strong: Ø~*-u*), I take it that this alternation is an instance of vowel change (ablaut) in the past tense affix and not a sequence of two affixes, say tense and number. This seemingly esoteric point turns out to be of great relevance for the preceding discussion, since the validity of (14) appears to turn on this one question. All other data considered fit neatly within (14). I cannot see any way to make the argument that Faroese *must* be analysed in the manner suggested here. That said, I believe there are at least two (related) arguments which show that Faroese *can* be analysed in this manner. This is, of course sufficient to remove the problem otherwise posed by the Faroese past tense.

First, observe that vowel quality alternations are quite widespread throughout Germanic inflectional paradigms, and in particular, many are not reducible to (synchronic) phonology. There are strong arguments to the effect that the vowel quality alternations are not themselves

[45] Rohrbacher could claim that variation in the size of control versus modal/raising/ECM complements is fixed cross-linguistically; not being a locus of parameterization, it might then require no over morphological cue. But this would leave as a mystery the fact that control infinitives in Swedish contrast minimally with control infinitives in Icelandic, compare Swedish (i) to (20):

the exponents or signals of morphosyntactic properties (as in Item-and-Process theories of morphology) but rather that they are the result of allomorphy or readjustment rules, triggered in the context of other morphosyntactic features.[46] In particular, we see that number effects a vowel change in the stem in the strong past tense in Faroese as well, as in (23).

(23) Faroese: *strúk-a* "stroke" (Lockwood 1964:81)

	Present	Preterite
1 psn sg	strúk **-i**	streyk **-Ø**
2 psn sg	strýk **-ur**	streyk **-Ø**[47]
3 psn sg	strýk **-ur**	streyk **-Ø**
1 psn pl	strúk **-a**	struk **-u**
2 psn pl	strúk **-a**	struk **-u**
3 psn pl	strúk **-a**	struk **-u**

The strong paradigm shows clearly that number may effect vowel changes elsewhere in the complex verb, without attributing to the affected vowel the status of a separate "number" morpheme (let alone agreement). Thus it is possible to treat the vowel alternation in the preterite in (17) as an allomorphy of (or a readjustment rule applying to) the single preterite morpheme, rather than as a distinct agreement morpheme. Note in particular that such an analysis is almost forced on us for Icelandic in (8) where exactly the same *-ði/ðu*-alternation occurs inside discrete agreement suffixes.

The second reason to believe that an allomorphy/readjustment rule analysis cannot be excluded has to do with the absence of a coherent alternative within the general background of assumptions assumed on both sides of the debate. Imagine we were to start from the premise that allomorphy rules of the sort just advanced were excluded a priori. Some deep hypothetical principle (call it DHP) prevents the vowel quality of one morpheme from varying as a factor of its syntactic environment. This would force us to analyse the dental fricative *-ð-* as the preterite marker, and the vowels as number agreement *–i* ⇔ [singular], *-u* ⇔ [plural]. Note, though, that these vowels are absent in the present tense. There is a vowel signifying plural in the present tense, but it is a different vowel, namely *–a*. Thus, we would have to say that the morpheme signifying [plural subject] is *–u* in the past, and *–a* in the present. Yet this would violate the hypothetical DHP, yielding a contradiction.

We conclude that contextually-sensitive allomorphy is required regardless of the proper segmentation of the Faroese past tense;[48] since this allomorphy provides us with an analysis

(i) Han lofade att *inte* **läsa** boken. (Swedish)
He promised to not read the.book
'He promised not to read the book.' Holmberg 1986:154

[46] The strongest argument for this, noted in Noyer 1997, Halle and Marantz 1993 is that the stem changes (in particular vowel changes) are demonstrably independent of the affixal material. Thus, for each of the affixes (including –Ø) in the English past tense and past participle system, there are verbs without vowel changes (*prove ~ prove-n; dwell ~ dwel-t; hit ~ hit-Ø, yell ~ yell-ed*) and verbs with vowel changes in addition to the affixes (*swell ~ swol-en; kneel ~ knel-t; sit ~ sat-Ø, tell ~ tol-d*). The independence of stem change from affix is demonstrable in German as well, where the so-called "irregular" verbs display both a stem change and the regular past tense suffix (*brennen* 'burn' ~ *brann-te* 'burn-t' (3sg)).

[47] Archaic *streykst*, see fn. 36 above.

[48] A (partial) theory of contextually-sensitive allomorphy is presented in Bobaljik 2000b, couched in the framework of Distributed Morphology.

consistent with (14), we conclude that (14) is the strongest generalization that can be made which is consistent with the known data.

4.2 The incompleteness of a one-way RAH

Data of the sort discussed in section 4.1 has not gone unobserved in the development of the RAH, and at least three recent authors (Roberts 1993, Vikner 1995, and Koeneman 2000) have recognized that a one-way implication like (14) is the strongest generalization which is empirically tenable. They have accordingly worked such a statement into their respective theories, though continuing to maintain a causal link from the morphology to verb movement. This leads to some not insignificant problems.

Consider first Vikner 1995, who states (p.135) that in his theory "it is true that if a language has distinct endings in both singular and plural [his definition of "rich" –JDB], then it also has V°-to-I° movement, but it does not necessarily hold that if a language has V°-to-I° movement , then it also has distinct endings in both singular and plural ." He cites the Kronoby Swedish data (given in section 4.1.1) to demonstrate the neccessity of the one-way formulation, and from this concludes that "we obtain the correct predictions that German, Icelandic, Yiddish and French (like Älvdalsmålet) all have V°-to-I° movement and that Danish, English, Faroese, Norwegian and Swedish (like Hallingdalen) do not have V°-to-I° movement." In this passage, Vikner reveals himself to have fallen prey to a mis-step in the logic of the argument. His theory licenses predictions only from "rich" agreement to verb movement, and no prediction is made about the behaviour of languages with "poor" morphology; in particular, no prediction whatsoever (correct or otherwise) is made about verb movement in Danish, English, Faroese, Norwegian or Swedish.[49]

An approach, which seeks to avoid this pitfall, surfacing early among proponents of the RAH (Platzack and Holmberg 1989) and maintained in Koeneman 2000 is the claim that, while rich agreement morphology does indeed trigger verb movement to Infl, it is not the only trigger. Advocates of such a view (Koeneman included, cf. p.82, n.18) appear to believe that, for languages with poor morphology, V in situ is some sort of unmarked option, and that verb movement is marked, taking the relatively low number of such languages in the sample as support of this view. Laying aside objections to this type of "markedness" argument (see Newmeyer 1998 for discussion), this general approach leads quickly down a slippery slope, threatening to undermine the content of the RAH, as discussed by Rohrbacher (1999 p. 119, 142, 151 n.17).

If rich morphology is not the only trigger for verb movement to Infl (hence, we abandon the "only if" clause of (7)), then the question of what other triggers there are for verb movement to Infl becomes of paramount importance. One must now ask whether these other triggers, once established, might not also be sufficient to trigger verb movement to Infl even in the presence of

[49] Roberts 1993:267ff equivocates in this regard. On p. 267, he gives a one-way implicature, in (i)

(i) Agr^{-1} is postulated [i.e., causing verb movement -JDB] only if there's overt distinct morphological plural marking.

In discussing the implications further down the page, Roberts appears to treat this as a biconditional, e.g., "…[T]his requirement [(i)] fails in both NE [Modern English from 17th C] and Danish. In NE, there is no morphology in the plural… *Therefore* Agr^{-1} is not determined." (p. 267, my emphasis -JDB). On the next page, though, he is quite explicit: "Notice though that [(i)] is formulated as a one-way conditional, and so it allows Agr^{-1} to exist in the absence of the relevant morphology" (p.268). My reading of the discussion following these quotes is that Roberts has in mind something similar to the Platzack and Holmberg 1989 position (which he cites) I discuss immediately below.

rich morphology, making morphology redundant. The suggestion offered by Platzack and Holmberg (1989) and Koeneman (2000) is that, in the absence of cues from the morphology, the presence of verb movement in the input data (i.e., adult grammar) is sufficient to trigger a verb movement rule in the child grammar. Thus, Koeneman (2000) assumes (cf. Platzack and Holmberg 1989:74):[50]

> "…that the child has two clues in the input relevant for the verb movement parameter; word order and the agreement paradigm. [The child] may simply note the word order before it acquires the agreement paradigm and decide … to add a movement rule to their intermediate grammar in order to make it more in line with the adult state" (p. 83).

If word order alone (evidence for verb movement to Infl) is a relevant clue for the verb movement parameter, and if it may be (and apparently is, see section 5) acquired before the agreement paradigm, then surely children exposed to a language with verb movement to Infl and rich inflection might also set their parameter on the basis of evidence for verb movement. Having done so, it is unclear what role is left for the morphology to play, and even less clear in what way the morphology can ever be said to be causally linked to verb movement.

Truly paradigm-based or morphology-driven approaches are inescapably wed to bi-conditional statements, of the form in (7). If syntax is driven by morphology, then the absence of the relevant morphology must correlate with the absence of the relevant syntax. Such approaches are not only incompatible with current theories of morphology, they also fail repeatedly to meet the bar of descriptive adequacy even within the restricted domain of Scandinavian and English. Recognizing this at some level, various hybrid approaches have emerged, maintaining, effectively, that syntax is morphology driven when the morphology is present, and determined by other factors in the absence of the appropriate morphological cues. While such an approach is not a priori incoherent, the "other factors" appealed to in all current formulations involve cues that are also present in languages with rich morphology. This seriously undermines any notion of causality in these theories. Finally, we note that an alternative has been available all along, one which is consonant with theories of morphology (as noted already by Johnson 1990). On the alternative, the parameterization involved is syntactic, and the morphology is but a reflection of the underlying syntax. Accepting the existence of phonologically null affixes leads directly to the conclusion that the strongest generalization that can be made in principle about any correlation between morphology and syntax is the one in (14). This generalization, though falsifiable, is unfalsified within the domain of inquiry.

5. THE ARGUMENT FROM ACQUISITION[51]

In the end of the preceding section, I touched on the issue of acquisition and the notions of triggers and parameter setting. Rohrbacher 1999:127 claims that despite the various unresolved problems that continue to face his theory, only his theory fares well in the face of data from child language acquisition, when compared to alternatives such as Vikner 1997 or Bobaljik 1997. Indeed, versions of the RAH (in which overt morphological variation causes syntactic variation) have risen to prominence in the acquisition literature (with reference to verb movement in

[50] In a similar vein, Rohrbacher 1994 is cited as suggesting that verb movement in Kronoby Swedish is attributable to Finish influence. On p.112, Rohrbacher gives this as an "a priori .. conceivable" approach, but on the next page notes that it is unattractive for the reasons discussed in the text here, concluding that "it might be preferable … to let Kronoby Swedish stand as a n unresolved problem for this approach."

[51] My thanks to Lydia White for bringing my attention to the relevant literature and for discussing it with me.

Germanic, in addition to Rohrbacher see, e.g., Clahsen 1988:52 for L1, and Vainikka & Young-Scholten 1994 for L2). Sprouse 1998 and Lardiere 2000 have challenged Rohrbacher on this position, citing, as Rohrbacher does, evidence from French and German.[52] Consideration of the available data in particularly the L1 the acquisition literature suggests quite strongly that no theory which requires knowledge of complete paradigms in order to trigger verb movement is maintainable. I will review the argument briefly here.[53]

Rohrbacher 1999's argument against Vikner 1997 in particular is made largely on the basis of evidence that French children acquire verb movement to Infl strikingly early. Pierce 1992:65ff (see also Verrips & Weissenborn 1992) notes that children display an (adult-like) alternation in which the finite verb precedes negation but the non-finite verb follows negation, right from the earliest uses of finite verbs (as early as 20 months, Pierce 1992:65). This is long before they have acquired the past tense, and thus Rohrbacher concludes that theories such as Vikner's which make reference to the morphological paradigms in all tenses are insufficient. Since Rohrbacher's theory requires that the distinctions identified in (7) only be made in any number of any tense, the child need only acquire one such sub-paradigm to posit verb movement for the language.

The problem, though, as Lardiere 2000:106 notes, is that "developmental data from much child language acquisition research suggest that children know extremely early whether verbs raise or not in the language they are acquiring, almost certainly long before they've acquired the myriad relevant morphological distinctions required under either Rohrbacher's or Vikner's analysis". Thus even for French—the basis of Rohrbacher's criticism of Vikner—it appears that children move their finite verbs even before they have met Rohrbacher's less stringent (i.e., "in any sub-paradigm") criteria.

This is immediately obvious if one takes the inflectional suffixes in French to be the markers of agreement—for the most common class of verbs (those in *–er* in the infinitive), all singular forms and the 3rd person plural are homophonous in the present tense (i.e., Ø); children do not produce the 2nd plural at all during the period during which they clearly acquire verb movement (Meisel 1993:100), and the status of the 1st plural is dubious, given the recognized tendency in the spoken language to replace this with impersonal forms (e.g., *on mange*, literally "one eats", for 'we eat'). The challenge to Rohrbacher is no less valid, though, if one allows the (clitic-like) subject pronouns to count as inflectional morphology (and thus relevant for (7)), as Rohrbacher does (p.219) (see also Meisel 1993, Pierce 1992 among others). Meisel 1993 presents evidence that both French and German children "acquire" (i.e., use) 3rd person singular finite forms significantly prior to their use of any other forms. Moreover, as soon as they reliably use such finite forms, they are used in the correct word order, i.e., having undergone movement (to Infl in French, to C° in German). Meisel's conclusion can be stated in this way. When French children

[52] Note also the studies reported in Snyder 1995 considering other putative cases of morphological variation causing syntactic variation. He concludes tentatively that it could be the case that

> "the implicational direction should be completely reversed … [T]here might be a grammatical basis for an association between syntax and morphological paradigms, but knowledge of the morphological paradigms would not reliably allow the language-learner to infer the relevant syntactic properties, and it would be a mistake to suppose that the syntactic knowledge was in any sense represented as knowledge of the morphology." (p.94)

[53] There is a certain regard in which one should feel uncomfortable with the discussion of the acquisition data and the use to which it is put in the arguments below (and in the works cited). Implicit throughout is the assumption that the child's grammar is accurately reflected in the child's production data. For example, if a child produces only a partial agreement paradigm, then they must "know" only that incomplete paradigm. This assumption strikes me as remarkably implausible, and any conclusions based on this are therefore equally suspect. On the need to distinguish between competence and performance (e.g., production) in acquisition, see, e.g., Hyams 1994 (section 5), and Grondin & White 1996 (p.5).

acquire their first finite verb form, it is the 3rd person singular and it is moved to Infl. When German children acquire their first finite verb form, it is the 3rd person singular, and it is moved to C°. In both cases, it is some months later before the children learn any contrast beyond ±Finite; that is, French and German children acquire movement of finite verbs prior to the acquisition of anything remotely resembling a paradigm, notwithstanding the possibility that subject clitics could be included in the paradigm in French.

It appears, then, exactly as Lardiere 2000 concludes, that children acquiring their first language acquire first the phrase structural elements that trigger movement (in our terms, the evidence for a Split IP); from the moment they begin to use finite verbs, these verbs are significantly in the correct place. Having acquired the heads in the syntax, children go about learning the relevant morphological pieces which instantiate these heads, exactly as might be expected on a separationist view of morphology.[54]

Note finally that I do not exclude the possibility that morphological distinctions, in principle, (in particular, the appearance of multiple inflectional morphemes on a single verb) may provide a clue for the child to set some syntactic parameter. Consider again the ramifications of the Split IP Parameter noted in (13) in section 3.2.2.

(13) a. the availability of two subject positions between CP and VP
b. the possibility of transitive expletive constructions
c. the availability of a VP-external derived object position
d. obligatory raising of the verb to Infl in non-V2 environments
e. the possibility of multiple inflectional morphemes on the verb stem

Since all of these properties are direct consequences of having a split IP, proper identification of any one of these by the child would count as an unambiguous trigger for the setting of the parameter in their language. Note, though, that with the exception of (13d), the absence of any of these properties is simply uninformative and does not imply an unsplit IP. Which particular properties children are exposed to first in different languages may well vary: in Yiddish (and

[54] Some mention need perhaps be made at this point of Borer & Rohrbacher 1997a, 1997b. It would appear, from passages such as the following, that this work represents a departure from Rohrbacher 1994, 1999.

> "It is sometimes suggested that the acquisition of functional projections is triggered by the acquisition of the corresponding functional morphemes, i.e., that the knowledge of morpho-phonology precedes, and triggers, the knowledge of syntax. Upon closer inspection, however, this assumption turns out to be extremely problematic." (Borer and Rohrbacher 1997b:5).

The discussion throughout this article concerns the type of theory which countenances the RAH, of which Rohrbacher 1999 is emblematic, and which has been quite influential. However, a close reading of Borer and Rohrbacher 1997b suggests that the theory presented there does not mark a true departure from the morphology-driven account of parametric variation offered in Rohrbacher 1999. Borer and Rohrbacher 1997b argue against the gradual, morphology-driven acquisition of functional structure, maintaining instead that children do have all functional projections from the outset (The Full Competence Hypothesis, see Poeppel & Wexler 1993). Nevertheless, they do argue for a causal relation between the acquisition of morphology and *movement* to the functional projection. Thus:

> "as long as an inflectional marker has not yet been acquired, the corresponding functional variable [i.e., feature in a functional head –JDB] is bound by a null auxiliary which is adjoined to the appropriate functional head. As soon as an inflectional marker is acquired and is specified on the main verb, the verb must move at least as high as the relevant functional variable node…" (Borer and Rohrbacher 1997b:28).

Borer & Rohrbacher also appear to treat the parametric variation which is at the core of the present article (and of Rohrbacher 1999) as a matter of covert vs. overt movement, suggesting that there is still room in their approach for a theory based on the RAH.

many varieties of spoken German) only compound past tenses are used and, since there are no present tense markers, evidence for (e) is non-existent. Similarly, direct evidence for verb movement to Infl (in non-V2 environments) in Icelandic and Yiddish is possibly restricted to embedded questions with negation and certain adverbials, hence it is doubtful that (d) is the key trigger for the SIP for Icelandic children.[55] There is more than one trigger for the Split IP Parameter; on the theory advocated by Bobaljik and Thráinsson (1998), there is no reason to expect any one trigger will be directly relevant in all languages. Importantly, the synchronic "cause" of verb movement is the presence of multiple inflectional projections, which does not permit feature-checking in situ. Morphology may—though need not—provide a clue to the existence of this state of affairs, but it does not cause it.

6. WHY MORPHOLOGICAL FEATURES CANNOT PROJECT

In the preceding sections, I have shown that the available empirical evidence leads to a rejection of theories in which any mechanism of counting contrasts in a paradigm is taken to uniquely determine the syntactic behaviour of that language. I have also argued that weaker theories, in which morphology only sometimes determines syntax, are inconsistent with the general morphology-driven approach. Lest the reader despair of there being any hope of finding correlations between morphology and syntax, I have shown that there are empirically sustainable correlations to be drawn, and I have shown the direction in which one might find a theoretical explanation for those correlations. In particular, these must be stated in terms of a theory of morphology which accepts what has come to be known as the Separation Hypothesis (cf. Beard 1995)—there is a logical separation between the morpho-syntactic features involved in a given representation and the phonological strings that instantiate those features. The arrangement of features in the syntax, in such a system, is logically prior to the morphology/phonology. If the conclusion is correct, then the considerations in this paper provide an argument against strongly "lexicalist" theories in which the syntax is projected from (phonologically defined) morphemes, as in Lieber 1980, Selkirk 1982 and Williams 1981.

In this context, then, I would like to state here one general consideration which militates against the lexicalist model. To the extent that the theories incorporating the RAH are morphology-driven, this consideration adds a final argument against the paradigm-based approaches to morphology-syntax interactions.

In a nutshell, the key consideration is that the (interdependent) notions of "underspecification" and "competition" (e.g., as embodied in the Elsewhere Principle) are fundamentally inconsistent with the basic tenet of (strong) lexicalism, which is that the features involved in syntax are uniquely projected from lexical items. This consideration has been alluded to before (see especially Anderson 1992, Halle and Marantz 1993 and Marantz 1997) though not in the context of present concerns, hence it bears restating.

It has long been recognized that inflectional morphology displays a large amount of syncretism and that underspecification is an important part of understanding this syncretism. For example, the Ø present tense in English is used with all person-number combinations for the subject, except 3sg (*I walk-Ø, you walk-Ø, they walk-Ø*…). This is formally captured not by positing five homophonous (zero) affixes (i.e., one for 1sg, another for 2sg, etc) but rather by positing a single zero affix which is unmarked for person and number features.[56] The invocation of

[55] These considerations call into question the view espoused in, e.g., Roeper & Weissenborn 1990, whereby each parameter has a unique trigger, designated by U.G.

[56] The argument here is unaffected if one chooses not to represent the bare form with a zero affix, but rather as an

underspecification has consequences for percolation-based theories of morphology, and in particular for our understanding of why *I walk-Ø* and *She walk-s* are grammatical, but **She walk-Ø*, and **I walk-s* are ill-formed. In particular, underspecification entails that a simple requirement of Feature-Matching between the subject and the agreeing verb is unsustainable. Since the *–Ø* form is unspecified for person and number features, it clearly does not match the [1sg] features of the pronoun *I*. The alternative is of course to appeal not to feature-matching but rather to non-disjointness of features, or to unification with a ban on feature clash. While this move admits (correctly) *I walk-Ø*, since there is no clash between [1sg] and the empty set [__], it also admits (this time incorrectly) *She walk-Ø*, as again, there is no clash between [3sg] and [__].

The "solution" routinely invoked is an appeal to "blocking", i.e., some form of the Elsewhere or Subset Principle familiar from phonology, in which a (fully or partially) unmarked form is unavailable when a more highly marked, licit alternative exists. It appears to be generally taken as self-evident that this blocking condition can be stated in the percolation-based approaches. However, as Anderson 1992:chapter 4, and Noyer 1997:chapter 0 (among others) have noted, it is far from obvious that this is workable, for at least two reasons.

Consider first the nature of such a principle. One version, put forth by Jensen (1990) is given in (24).

(24) Jensen's version of the Elsewhere Condition (p.142)

> If two or more morphemes can be attached to a third form, the morpheme with the more specific subcategorization frame takes precedence and blocks the attachment of the other morpheme. A subcategorization frame, A, is more specified than another, B, if A specifies everything that B specifies and more.

This is not relevant for the bulk of inflectional morphology. Certainly, the affixes *–Ø* and *–s* share the same subcategorization frames: they both attach to uninflected verb stems.[57] There is no way to appeal to the agreement features in this case, since on a lexicalist approach, these features are supposed to be contributed by the affix, and certainly cannot be contained in the subcategorization frame of the affix as this would presuppose their existence in the stem, independently of the affix. In addition, as Noyer 1997 notes, framing the principle as in (24) seriously undermines the entire lexicalist program: "[T]he guiding insight of the representation-based model is that affixes attach *freely* to compose words, subject only to their own subcategorization requirements, and not to the subcategorization frames of other affixes. Appealing to the Elsewhere Condition in the lexical model requires that an affix may attach to a stem only if another, more specific affix *could not* have attached" (p.*l*, emphasis in the original).

Note that this criticism applies equally to the version of morphological blocking put forth by Andrews (1990:519), which is stated not as competition among affixes but as competition among the resulting words. The essence of Andrews's proposal is given in (25):

(25) A word W1 is licit in a syntactic context S iff

> i. the features of W1 are consistent with (do not conflict with) those of S, and
> ii. no word W2 exists such that W2 satisfies (i) and subsumes the features of W1

uninflected form.

[57] cf. Jensen's treatment of Latin, where this is brought up. Jensen 1990:142, where person/number affixes share the subcategorization frame __]V. Note that Jensen's system requires essentially fully differentiated affixes; as presented, his Unmarking Principle (p.141), if applied to an underspecified form such as English Ø, would yield the unwelcome result that *walk-Ø* is third person singular.

On this story, in the context *She_____*, the word *work* satisfies condition (i), but the word *work-s* is a "better fit"—it also satisfies condition (i) and, it is associated with a superset of the features of *work* (i.e., all the features of the bare stem, plus the features 3sg). This faces an additional problem, recognized in part by Andrews (see also Blevins 1995) in the determination of possible competitors, i.e., how to check for competing W2s. Since, on a lexicalist approach, the word *work-s* does not exist prior to the concatenation of the two independently listed constituent pieces, determination that *work-s* blocks *work* in the environment of a 3rd person singular subject requires (minimally) that the stem *work* be combined with all affixes with which it may combine, and the results inspected. Roughly, prior to using the word *work* in some context, the grammar must first combine this stem with all affixes that subcategorize for a verb and check to see that there is no better fit. It is not clear that the procedure is finitely tractable.

Even granting tractability for the sake of argument, the second problem concerns the determination of the syntactic environment. Recall that the hallmark of the percolation-based theories (and implicitly of morphology-driven theories generally) is the idea that the properties of a word are projected from its constituent pieces. But in order for any kind of blocking condition like (25) to work, the features of the syntactic context must be determinable independently of the word being evaluated. In order to state competition, as Anderson noted, it must be competition for the realization of some set of properties. It is not at all clear how this can be evaluated, unless the properties are determined independently.

Considering the examples generally cited, adherents to the lexical approach appear to take the subject to determine the features for environment, relative to which the verb is evaluated. Thus, the pronoun *she* provides the features [3,sg,fem] relative to which the candidates *work-Ø*, *work-s* (and, in principle, *work-er*, *work-man-ship*, etc…, see above) are evaluated. But just as it is clear that the pronoun may provide the features constitutive of the syntactic environment for the verb, it is equally clear that in other cases the pronouns lack such features. An example is provided from the inflectional system of Russian.

Jakobson 1932/1984:7 argues that the masculine form in Russian is unmarked with respect to the feminine. Given that the verb in the past tense shows agreement with a singular subject for gender, the pattern in (26) therefore directly mirrors the English situation just considered. The unmarked form (masculine) is ungrammatical when a more marked form exists (feminine) compatible with the syntactic environment.

(26)	a.	on	rabotal-Ø /	*rabotal-a	(Russian)
		he	worked- /	*worked-FEM	
	b.	ona	rabotal-a /	*rabotal-Ø	
		she	worked-FEM /	*worked-	

Only third person pronouns, however, mark gender. The first and second person pronouns are gender-invariant. Agreement for gender is nevertheless obligatory with first and second person subjects (27).

(27)	a.	ja	rabotal-Ø /	*rabotal-a	(spoken by a man)
		I	worked- /	*worked-FEM	
	b.	ja	rabotal-a /	*rabotal-Ø	(spoken by a woman)
		I	worked-FEM /	*worked-	

As the pronoun in this instance does not encode gender features, it is impossible to maintain on a percolation-based theory that the gender of the pronoun contributes the feature for the syntactic environment. But as the examples in (27) stand as well-formed utterances, i.e., complete

sentences, there is no overt determinant of gender which may provide the syntactic context for the alternation in the verb. In other words, if all features in the representation originate in the lexical entries of the individual morphemes, there is no possibility to invoke blocking (25) to exclude the use of the unmarked form *rabotal* 'worked' when the speaker is female.

This is by no means an isolated example. If number contrasts are expressed as the presence (marked) versus the absence (unmarked) of the feature [Plural], a standard interpretation of underspecification theory, then blocking conditions such as those in (25) can also not exclude the English sentence in (28) used to refer to a plurality of canines.

(28) The dog walked.

The past tense verb in English shows no number distinction, and the noun is, by hypothesis, unspecified for number. As the feature [Plural] is nowhere introduced into the representation, this feature cannot serve as the context characterizing the syntactic environment, and hence cannot trigger blocking, e.g., of unspecified *dog* by more highly specified *dogs*.

The point is quite straightforward, and has long been recognized in work on morphology (see Anderson 1992 for discussion, see Bierwisch 1967 for an early and extensive analysis couched in these terms). The recognition of the importance of underspecification (Natural Classes for Bierwisch) requires a theory in which the syntactic context is logically not only independent of but prior to whatever function governs the choice of overt morphological exponents. Features may be active in the syntax, despite finding no overt realization in the morphology. It follows directly that such features cannot be projected from the morphology.[58]

The alternative, of course, is a theory in which the terminal nodes in the syntax are (bundles of) features, fully specified by the end of the (syntactic) derivation. These terminal nodes are then provided with phonological content by a procedure we may call lexical or vocabulary insertion. The rules (or pieces) which are implicated in this procedure need not be, indeed often are not, fully specified, and the result of this underspecification is the appearance of syncretism. There are no paradigms in this theory; no more than there are sentences in the lexicon. Paradigms, like sentences, are derived from their constituent parts via the operation of the grammar. As noted above, there is no reconciliation between this framework (the dominant one in morphology, for apparently strong reasons) and a framework in which syntactic variation is caused by paradigmatic contrasts in the overt morphology. Importantly, the strongest correlation between syntactic variation and morphological variation that the theory will allow is that in (14), again, with no reference to paradigms per se. Conveniently, this is also the strongest correlation that the empirical evidence allows.

[58] Anderson's example to illustrate almost the same point is the Georgian verb form in (i).

((i) mo= g- k̇lav (Georgian)
PREVERB 2OBJ kill
'I will kill you.' (Anderson 1992:87)

The meaning of the verb is unambiguous, despite the absence of any overt signal of the first person singular. Moreover, Anderson carefully details the argument that there is no zero morpheme with the features [1sg] in this particular form, i.e., an allomorph of the *v-* prefix for 1st person subjects which occurs elsewhere. We recognize instead that it is a quirk of Georgian morphology that there is one "slot" (however this is to be formalized) preceding the verb stem; a number of prefixes, including */g-/* corresponding to 2OBJ and */v-/* corresponding to 1SUBJ compete for insertion at this position, but only one will be inserted (see also Halle and Marantz 1993:119f). Crucially, in a language such as Georgian in which the verb itself may serve as the utterance, there can be no determinant of syntactic environment that is projected from something other than the verb. Yet here the meaning is unambiguous. This again reinforces Jakobson's position that the theory of morphological exponence is realizational, i.e., that the syntactic terminal nodes are fully specified for the relevant features, even if the rules introducing the phonological material realizing these nodes are underspecified.

7. CONCLUSION

In the investigation of fine variation among the closely related Scandinavian languages in the late 1980s, it was observed that there was a correlation to be drawn between morphological and syntactic properties. Diachronic evidence showing that these morphological changes preceded syntactic changes led to the assumption that the morphological changes caused the syntactic changes, and thus to the hypothesis that overt morphological properties are the cause of observable syntactic properties. While this hypothesis, which I have dubbed the Rich Agreement Hypothesis, conflicts sharply with results in morphological theory, it has gained currency in the syntactic and L1 and L2 literature. In this paper, I have suggested that the strong form of the RAH as applied to the relation between agreement morphology and verb raising, is empirically untenable and is falsified by data from diachronic and synchronic variation, the latter including variation among dialects, idiolects, and constructions. Proposed weakenings of the RAH which nevertheless retain the basic tenet that morphological properties causes syntactic properties were shown to be at best incomplete, and possibly inconsistent with the approach they claim to instantiate. An apparent argument from acquisition was considered, and it was shown that the argument, if anything, refutes the RAH. These negative results were contrasted with an alternative approach which does not take the morphology to be the cause of the syntax, but rather, in keeping with the results of morphological research over many years, takes overt morphology to be a reflection (albeit a somewhat imperfect one) of the prior syntactic structure. While details of the particular instantiation of such a theory remain to be worked out, such a theory avoids the pitfalls faced by the RAH, and accounts not only for the observed, weak correlations between morphology and syntax, but goes a good deal of the way towards explaining why the one-way implication in (14) is in principle the strongest statement that can be made.

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