

## **Some Purported Problems for the Movement Theory of Control**

**Abstract:** Ndayiragije (2012) and Wood (2012) present arguments against the Movement Theory of Control (MTC) based on data from Kirundi and Icelandic respectively. We show that these data are easily accounted for by current formulations of the MTC.

### **1. Introduction**

Two recent publications, Ndayiragije (2012) and Wood (2012), present data which is claimed to be problematic for the Movement Theory of Control (MTC). We will show that these data raise no serious difficulties for current formulations of the MTC. Indeed, some of the data points cited by Ndayiragije and Wood lend further support to the theory. We begin in section 2 with a brief outline of the MTC. Wood's paper is addressed in section 3 and Ndayiragije's in section 4. We conclude in section 5 with some general methodological remarks, and suggestions for more promising lines of attack on the MTC.

### **2. The Movement Theory of Control**

The core claim of the MTC is that obligatory control is derived via A-movement. For example, the subject of the embedded clause in (1a) is interpreted as a variable bound by *John* because *John* has undergone A-movement from the embedded to the matrix subject position:

- (1) a. John wants to win.  
b. [John] wants [<sub>TP</sub> [~~John~~] to win].

Viewed in more detail, the derivation sketched in (1b) involves three movements. *John* begins in a  $\theta$ -position where it receives the external  $\theta$ -role of *win*. It then moves via embedded [Spec,TP]<sup>1</sup> to a  $\theta$ -position where it receives the external  $\theta$ -role. Finally, *John* moves to matrix [Spec,TP] to receive Case:

(2) [TP [John] [<sub>VP</sub> [~~John~~] wants [TP [~~John~~] to [<sub>VP</sub> [~~John~~] win]]]].

$\theta$ -roles are treated as features within the MTC, and the movement into matrix [Spec,vP] is assumed to be driven directly by thematic role assignment. The MTC adopts the standard assumption that A-movement into matrix [Spec,TP] is driven primarily by the need of the subject DP to check/value its Case feature.<sup>2</sup>

To deal with adjunct control, Hornstein (2001) proposes that the operations Copy and Merge should be allowed to apply freely between workspaces, yielding so-called “sideward” movement.<sup>3</sup> An example adjunct control derivation is given in (3):

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<sup>1</sup>Movement to embedded [Spec,TP] would presumably be driven by the EPP. It is not in fact crucial to the MTC that this intermediate movement occurs.

<sup>2</sup>It is clear that the EPP cannot be the crucial factor, since DPs may control from non-subject positions:

(i) John persuaded [Bill] [TP [~~Bill~~] to leave].

It is worth noting, however, that the hypothesis that this movement is Case-driven is not a crucial component of the MTC (see also footnote 13 in this regard). The core claim is simply that whatever drives A-movement into subject/object position in (ii)-(iii) is also what drives the relevant instances of A-movement in (2) and (i):

(ii) [John] seems [TP [John] to have left].

(iii) John believes [<sub>AgroP</sub> [~~Bill~~] [TP [~~Bill~~] to be intelligent]].

<sup>3</sup>Hornstein’s proposal is modeled explicitly on Nunes (1995), which uses sideward movement to explain the properties of Parasitic Gap constructions.

(3) a. [John] laughed at Mary [without ~~John~~ falling over].

b. [VP laughed at Mary]                      *Workspace 1*  
[PP without [John] falling over]        *Workspace 2*

“Sideward” movement of ‘John’ from *Workspace 2* to *1*:

[VP [John] laughed at Mary]                *Workspace 1*  
[PP without ~~John~~ falling over]        *Workspace 2*

*Workspace 2* merges as an adjunct in *Workspace 1*:

[VP [VP [John] laughed at Mary] [PP without ~~John~~ falling over]]

Merger of matrix *T*; ‘John’ moves to [*Spec, TP*]:

[TP [John] T [VP [VP ~~John~~ laughed at Mary] [PP without ~~John~~ falling over]]]

The analysis of adjunct control in terms of sideward movement explains three key properties of the construction. First, it explains why adjunct control is not blocked by the CED, since *John* moves before the *without* PP is merged as an adjunct. Second, in conjunction with a “Merge over Move” constraint, the analysis explains why adjunct control is typically subject-oriented. If *John* had moved instead to become the object of *laughed at*, this would have violated Merge over Move, since an alternative option at this point in the derivation would have been to merge *Mary* from the numeration. Third, the analysis offers an account of why A'-movement out of adjuncts is impossible. Consider the derivation of the illicit (4):

(4) \* Who did John laugh at Bill before Mary spoke to?

The crucial stage in the derivation of (4) is shown in (5):

- (5)     [<sub>CP</sub> C [<sub>TP</sub> [John] [<sub>VP</sub> ~~John~~ laughed at Bill]]]     Workspace 1  
           [<sub>PP</sub> before Mary spoke to [who]]                     Workspace 2

At this point in the derivation, *who* in Workspace 2 can move sideward into [Spec,CP] of Workspace 1 without violating the CED. Thus, if it were then possible for the *before* PP to adjoin to the main clause, the derivation could converge on the structure in (6):

- (6)     [<sub>CP</sub> [who] [<sub>CP</sub> C [<sub>TP</sub> [John] [<sub>VP</sub> ~~John~~ laughed at Bill]]]  
           [<sub>PP</sub> before Mary spoke to [~~who~~]]]

Hornstein follows Reinhart (1983) in assuming that the relevant class of adjuncts must adjoin below C. If adjunction is subject to extension, it follows that there can be no derivation of (6) which satisfies both the requirements of the adjunct and the requirements of the *wh*-phrase (Hornstein 2001:89-90). If the adjunct adjoins at TP or below, then by the time C has merged, *who* will already be trapped in an adjunct island by the time C merges:

- (7)     [<sub>CP</sub> C [<sub>TP</sub> [<sub>TP</sub> [John] [<sub>VP</sub> ~~John~~ laughed at Bill]]] [<sub>PP</sub> before Mary spoke to [who]]]]

On the other hand, if the adjunct has not adjoined by the time C is merged, it has missed its opportunity to attach to the matrix structure, and the derivation crashes.<sup>4</sup> In the control

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<sup>4</sup>We assume that a derivation which does not eventually reduce to a single workspace crashes. We have not explained here how the analysis plays out with embedded clauses. That is, nothing we have said rules out the possibility that the PP in (7) could later merge at an appropriate position in a higher clause, thus saving the derivation. See Boeckx, Hornstein & Nunes (2010a) for a discussion of how derivations of this sort can be blocked using subnumerations.

derivation in (3), by contrast, sideward movement targets [Spec,vP] rather than [Spec,CP]. The adjunct can therefore attach below C as required, and the derivation converges. In general, there are two factors which determine whether or not a particular movement can escape an adjunct via sideward movement: (i) the maximum height of the adjunct in the main clause, and (ii) the height of landing site. The latter must be lower than the former.

### **3. Wood**

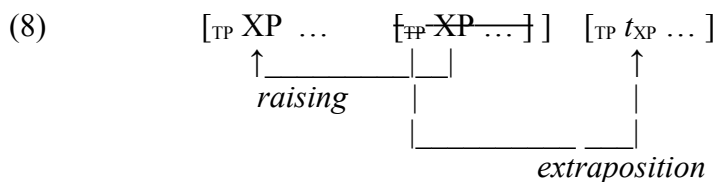
Wood begins his paper with some restatement of earlier criticisms of the MTC and its treatment of the Icelandic facts. We will come to these in sections 3.2 and 3.3. We will first examine Wood's argument based on extraposed infinitives in Icelandic.

#### *3.1. Extraposed infinitives*

Wood, following Thráinsson (1979), notes that Icelandic control infinitives can often occur with an optional Case-marked pronoun *það* ('it'). On Thráinsson's analysis, the infinitive is extraposed in this construction and the pronoun is its associate. A'-movement and raising are impossible out of extraposed infinitives, but control into extraposed infinitives *is* possible. Wood takes this to argue against the MTC. The logic of his argument is as follows. To all appearances, the correct generalization regarding extraposed infinitives is that they are islands for both A and A'-movement. On standard theories of control, it is unsurprising that control into extraposed infinitives is nonetheless possible. But on the MTC, Woods argues, one would have to make an unmotivated stipulation to permit some instances of A-movement to circumvent this generalization. We think that the situation is in fact precisely the reverse of what Wood suggests.

Whereas standard theories of control simply stipulate that control dependencies are not sensitive to the CED and other constraints on movement, the MTC can provide a principled explanation for Wood's data.<sup>5</sup>

To begin with, let us consider the ban on raising out of an extraposed infinitive. Assume for the moment that the extraposed clause is base-generated in an argument position and arrives in its extraposed position via movement or some other operation. In this scenario, there are two logically possible derivations which must be ruled out: one in which raising precedes extraposition, and one in which raising follows extraposition. If an extraposed clause is an island for extraction, we can immediately rule out the second possibility. The key question is, could raising occur prior to extraposition? Assuming that both raising and extraposition are subject to the extension condition, raising can precede extraposition only if extraposition targets a higher position than raising. That is, a position higher than [Spec,TP] in the case at hand:



Now consider in this light control into extraposed infinitives. In this case, the initial movement out of the embedded infinitive targets not [Spec,TP], but [Spec,vP]. Thus, for the extension condition to be respected, it is only necessary that extraposition target a

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<sup>5</sup>It is worth emphasizing here that the MTC is committed to the availability of sideward movement as a grammatical option. More precisely, given that adjunct control appears to display all the properties of complement control, if the latter are reflections of movement, then the latter must be as well. See conclusion for some further discussion.

position higher than [Spec,vP]. Thus, given the hypothesis in (9), Wood's data are straightforwardly accommodated by the MTC:

(9) Extraposed infinitives in Icelandic must adjoin below the finite subject position.

The logic is essentially the same in the scenario where the extraposed infinitive is base-generated in its extraposed position. In this case, the control and raising movements would be sideward movements. Again, the question will be whether control and raising target positions above or below the adjunction site of the extraposed relative. The derivation for raising is shown in (10). The position in which the base-generated extraposed infinitive is interpreted is shown filled by a null TP, [<sub>TP</sub> *e*].

(10) **Stage 1:**

Matrix clause is constructed up to T' in workspace 1; extraposed associate of [TP e] is constructed in workspace 2:

<i>Workspace 1</i>	<i>Workspace 2</i>
[T' ... [TP e] ... ]	[TP XP ... ]

**Stage 2:**

Sideward movement of XP from workspace 2 into [Spec,TP] in the matrix clause in workspace 1:

<i>Workspace 1</i>	<i>Workspace 2</i>
[TP XP [T' ... [TP e] ... ]	[TP <del>XP</del> ... ]
↑	
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**Stage 3:**

Extraposed infinitive clause adjoins to matrix clause:

[TP [TP XP [T' ... [TP e] ... ]] [TP ~~XP~~ ... ]]

Once again, the raising derivation in (10) is possible only if (9) is false, whereas the corresponding control derivation is available independently of (9). In the control derivation, sideward movement of XP targets a position lower than the finite subject



position ([Spec,vP]), and the extraposed infinitive is free to adjoin to vP following this movement.<sup>6</sup>

How empirically plausible is (9)? We don't know about Icelandic, but as mentioned in the last section, this is a standard assumption for English. The extraposed clause likely attaches around vP/VP, which is why it can be elided in VP ellipsis and fronted in VP fronting:

- (11) a. John hates it that Frank left and Harry does [~~hate it that Frank left~~] too  
b. John [hates it that Frank left], and [~~hate it that Frank left~~]<sub>i</sub> he should *t*<sub>i</sub>

In addition, extraposition triggers Condition C effects, which follows immediately if extraposed clauses sit below TP:

- (12) \* He<sub>i</sub> hates it that we don't like John<sub>i</sub>.

In sum, rather than Wood's Icelandic data being problematic for the MTC, they may well constitute another argument in its favor if (9) is correct.<sup>7</sup>

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<sup>6</sup> Recall that once the infinitive is adjoined, movement out of it is blocked by the CED, so adjunction must occur after movement of XP out of the infinitive.

<sup>7</sup> The analysis outlined in this subsection actually has a rather old pedigree, as it is isomorphic to the one provided by Hornstein (2001:119-121) for the absence of expletive control into base-generated adjuncts. That is, the analysis extends to explain the contrast between (iv) and (v) (noted in Lasnik 1992:244):

- (iv) \* There<sub>i</sub> was a crime without PRO<sub>i</sub> being a victim.  
(v) John<sub>i</sub> was a witness without PRO<sub>i</sub> being a victim.

### 3.2. *Retreading old ground*

Wood states that Boeckx & Hornstein 2006 and Boeckx, Hornstein & Nunes 2010 “fail to address the strongest argument against Case-driven movement: that the nominative Case borne by PRO is the same structural nominative seen in finite clauses...” Here, Wood appears to be referencing two objections first raised in Sigurðsson 2008 and Bobaljik & Landau 2009. These objections are in fact mentioned briefly by Boeckx, Hornstein & Nunes in footnote 8 (p. 122), and implicitly addressed on p. 120 (see in particular derivation (14)). The objections in question can be summarized as follows.

- (i) Sigurðsson (2008) notes that case-agreeing elements in Icelandic typically appear in the nominative when they are associated with PRO. If, as Boeckx & Hornstein (2006) suggest, this nominative is a default case rather than a structural one, how do we account for the fact that certain of these elements show up in an invariable, non-agreeing default form when associated with a quirky subject? Should we not expect these too to surface in the nominative default form, if it is indeed the default?
- (ii) Bobaljik & Landau argue that “The participial agreement facts are particularly relevant, since, as [Boeckx & Hornstein (2006)] note, ‘overt morphological agreement on ... passive past participles (Case, number, gender) *can only take place with elements bearing structural Case.*’...Since the passive participle in control complements obligatorily agrees with the subject of the infinitive...it follows — on B&H’s own assumptions — that this nominative is structural case, not default case.” (p. 123)

Objection (i) is based on Icelandic examples such as (13) (Sigurðsson 2008:407):

- (13) a. Honum er kalt/\*kaldur/\*köldum.  
           *him.D is cold.DFT/\*N.M.SG/\*D.M.SG*
- b. Hans er saknað/\*saknaður/\*saknaðs.  
           *him.G is missed.DFT/\*N.M.SG/\*G.M.SG*

Examples of this sort show that “predicative adjectives and participles that take a quirky subject (and do not also take a nominative object [...]) show up in an *invariable, default form*, regardless of the gender and number of the quirky subject” (p. 407, italics in original). The same appears to hold in embedded clauses with PRO subjects. If the predicate is one which takes a quirky subject, then predicative adjectives and participles do not agree. In contrast, if the predicate is one which does not take a quirky subject, these elements do agree. This is shown in (14a-b):<sup>8</sup>

- (14) a. Hann vonaðist til [að PRO verða ekki of kalt].  
           *he.N hoped for to D be not too cold.DFT*  
           ‘He hoped not to get (feeling) too cold.’ (≠ ‘be cool/daring’)
- b. Hann vonaðist til [að PRO verða nógu kaldur].  
           *he.N hoped for to N be enough cool/daring.N.M.SG*  
           ‘He hoped to be cool/daring enough.’ (≠ ‘be (feeling) cold’)

<sup>8</sup>Note that Sigurðsson’s gloss in (14b) embodies a theoretical assumption which BH&N reject. That is, whereas Sigurðsson assumes that PRO in (14b) has structural nominative case, and hence glosses it with *N*, BH&N take PRO in (14b) to lack structural case.

To keep things as simple as possible, we will leave aside the question of whether Sigurðsson's criticism is accurate as applied to its target, Boeckx & Hornstein (2006). The following, therefore, should not be read as a response to Sigurðsson's original argument, but rather as an response to Wood's application of this argument to BH&N. Contrary to Wood's suggestion, the facts in (14) are straightforwardly accommodated by the analysis of BH&N. The generalization is simply that predicative adjectives and participles which are associated with a quirky subject surface in the invariable form, and that predicative adjectives and participles associated with a Caseless PRO surface in the default nominative form. Crucially, BH&N's analysis distinguishes quirky-case-marked PRO from Caseless PRO, so it is not necessary to hypothesize that *both* forms result from one and the same a default assignment rule (which would obviously be problematic). Recall that on BH&N's analysis, quirky Case is assigned to the controller when it starts out in an embedded clause whose predicate takes a quirky subject. In ordinary instances of control, on the other hand, the controller is not assigned any Case until it reaches the matrix subject position. In (14a), the quirky Case features of the controller block Case and  $\phi$  agreement so that the adjective surfaces in the invariant form. In (14b), by contrast,  $\phi$ -feature agreement proceeds as normal. Since the controller in (14b) has no syntactic Case features, no Case features can be transferred to the adjective via Agree, and the adjective is consequently spelled out with default nominative Case morphology. In other words, the controller and the adjective both end up with nominative morphology not because Agree copies nominative features from the controller to the adjective, but rather because there are no Case features on the controller to copy, leaving the adjective too without a syntactic Case specification. The key point here, as BH&N note (p. 122fn8), is

that there is no reason to suppose that the lack of a syntactic Case specification should block  $\phi$ -agreement. Thus, there is no reason to suppose that the absence of such a specification should lead to the adjective surfacing in the invariant form. To make this a little more concrete, we will now sketch a toy Distributed Morphology-style analysis for the adjective *kaldur* ('cold').<sup>9</sup> The full strong paradigm for *kaldur* is given in (15). Since we have no interest in the morphology of Icelandic adjectives as such, we simply give the full form of the adjective on the left of each vocabulary item.<sup>10</sup> For the specification of the paradigm, it is helpful to be able to group masculine and feminine nouns, and to be able to identify feminine nouns via a single feature. We therefore make use of two features  $\pm A$  and  $\pm B$  to specify gender, with masculine  $[+A,+B]$ , feminine  $[+A,-B]$  and neuter  $[-A,+B]$ .<sup>11</sup> Masculine and feminine share  $[+A]$ , while  $[-B]$  uniquely identifies feminine. Apart from this, we make use of a  $\pm pl$  (plural) feature, and  $\pm nom$ ,  $\pm acc$ ,  $\pm dat$  and  $\pm gen$  features for Icelandic's cases. The toy analysis for the paradigm in (15) is given in (16):

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<sup>9</sup> On DM, see Harley & Noyer (1999) and references cited therein. Icelandic has both strong and weak adjectival agreement paradigms. Here we show the strong paradigm. Extending the analysis to cover the weak paradigm would be straightforward. A Python script for computing the paradigm in (15) from the rules given in (16) is at <http://pastebin.com/6vBD5iHc>.

<sup>10</sup> A non-toy analysis would of course split the adjective into a stem followed by one or more suffixes, with only the suffixes being spelled out via vocabulary items. Readjustment rules would trigger the vowel changes seen in some parts of the paradigm.

<sup>11</sup> Note that the impoverishment rules 15, 16 and 18 of (16) can yield the specification  $[-A,-B]$ . This is in effect a feminine specification, since the vocab rules in 1-10 identify feminine adjectives solely via the  $[-B]$  specification. The impoverishment rules apply in order. 16 may bleed 18, but this has no effect on output.

(15)	[Singular]	<b>Masc</b>	<b>Fem</b>	<b>Neuter</b>
	<b>Nom</b>	kaldur	köld	költ
	<b>Acc</b>	kaldan	kalda	költ
	<b>Dat</b>	köldum	kaldri	köldu
	<b>Gen</b>	kalds	kaldrar	kalds
	[Plural]	<b>Masc</b>	<b>Fem</b>	<b>Neuter</b>
	<b>Nom</b>	kaldir	kaldar	köld
	<b>Acc</b>	kalda	kaldar	köld
	<b>Dat</b>	köldum	köldum	köldum
	<b>Gen</b>	kaldra	kaldra	kaldra

- (16) **Vocab items:**
- |              |   |                    |                              |   |                    |
|--------------|---|--------------------|------------------------------|---|--------------------|
| 1. 'költ'    | ↔ | [_]                | 10. 'kalds'                  | ↔ | [-pl,+gen,+A,+B,_] |
| 2. 'kaldur'  | ↔ | [+A,+B,_]          | 11. 'kaldir'                 | ↔ | [+pl,+A,+B_]       |
| 3. 'köld'    | ↔ | [-B,_]             | 12. 'kaldar'                 | ↔ | [+pl,+A,-B,_]      |
| 4. 'kaldan'  | ↔ | [-pl,+acc,+A,+B,_] | 13. 'kaldra'                 | ↔ | [+pl,+gen,+A,-B,_] |
| 5. 'kalda'   | ↔ | [-pl,+acc,+A,-B,_] | <b>Impoverishment rules:</b> |   |                    |
| 6. 'köldum'  | ↔ | [+dat,+A,+B,_]     | 14. [+gen,_]                 | → | [+A]               |
| 7. 'kaldri'  | ↔ | [-pl,+dat,-B,_]    | 15. [+pl,+gen,_]             | → | [-B]               |
| 8. 'köldu'   | ↔ | [+dat,-A,+B,_]     | 16. [+pl,-A,_]               | → | [-B]               |
| 9. 'kaldrar' | ↔ | [-pl,+gen,-B,_]    | 17. [+pl,+dat,_]             | → | [-pl,+A,+B]        |
|              |   |                    | 18. [+pl,+acc,+B,_]          | → | [-pl,-B]           |

On this analysis, nominative forms are defaults in the sense that the relevant vocabulary items do not specify a +nom feature — it is only the presence of more specific accusative, dative and genitive forms which prevents the nominative morphology surfacing for adjectives with these Case specifications. The absolute default form is ‘költ’, and it is therefore this form which surfaces when there is specification for neither Case nor  $\phi$ -features (since all of the other vocabulary items impose restrictions on either number, gender or Case). Let us now consider how these observations relate to the contrast in (14). In (14a), the quirky subject blocks  $\phi$ -agreement (and PRO has no Case features), with the consequence that the only matching vocabulary item is the one for ‘költ’. In (14b), PRO likewise has no Case features, but since  $\phi$ -agreement has occurred, the vocabulary item for ‘költ’ is blocked by the more specific vocabulary item for ‘kaldur’, which is specified [+A,+B] (masculine).

Technical details aside, the essential point here is the following. Since BH&N’s analysis makes a featural distinction between the PRO in (14a) and the PRO in (14b), and since via Agreement this distinction gives rise to a similar distinction in the feature specifications of the two adjectives, it is straightforward to devise a set of morphological rules which will spell out the two adjectives differently in each case. The question, then, is not whether it is *possible* to derive the correct output given the MTC, but rather whether the morphological analysis proposed is plausible. This in itself gives some indication of the strength of the argument against the MTC based on Icelandic case and agreement morphology. Since morphological rules often are complex and arbitrary, it would hardly strike the death blow against the MTC if the postulation of complex and arbitrary morphological rules proved to be necessary in this instance. But in any case, the

underspecification analysis sketched above strikes us as distinctly un-egregious. It simply embodies the rather innocuous pair of assumptions (a) that nominative is in morphological terms the default Case in Icelandic, and (b) that there is a form of the adjective which surfaces in the absence of any Case or  $\varphi$ -features.

In this light, consider objection (ii) above. Bobaljik & Landau's choice of quotation here is somewhat misleading. The quotation is taken from the beginning of section 2 of Boeck & Hornstein (2006), which gives a brief descriptive summary of the relevant Icelandic control facts. At this point in the paper, none of B&H's theoretical proposals have been introduced, and "structural case" is simply being used in contrast to "quirky case." (As we have seen, quirky-case-bearing elements do not trigger participle agreement in Icelandic.) Nowhere do B&H suggest that DPs which receive default nominative should be invisible for agreement. And in any case, this assumption is not independently motivated, or required for any other aspect of B&H's analysis. Sigurðsson (2008:418) remarks that "if the notion of 'default nominative' is to make sense as a *different* notion than 'structural nominative', one would expect it to differ from the latter precisely in being an elsewhere case, invisible to agreement." We hope that the analysis sketched in (16) will make it clear how default nominative is fully compatible with agreement. There is, however, an issue here relating to B&H's and BH&N's original proposals. Both appear to rest on the assumption that default nominative arises from some kind of post-syntactic morphological process. An anonymous reviewer argues out that on this analysis, one might expect a default nominative specification to be invisible to syntactic agreement, so that no syntactic process could transfer PRO's syntactically-invisible default nominative Case onto the adjective. We are not sure how strong this



argument is — we see no obvious reason why assignment of default nominative could not be a “last resort” syntactic process which feeds Agree. But in any case, the present analysis obviates this concern, since given (16), there is no need for any kind of default assignment rule. If PRO has no Case specification, then the adjective will likewise have no Case specification. Since adjectives without a Case specification are spelled out with nominative morphology, the adjective will be spelled out with nominative morphology, giving the appearance of agreement. The present analysis therefore leaves it an open question whether nominative is a *syntactic* as well as a morphological default Case in Icelandic. That is, the vocabulary items in (16) are fully compatible with the hypothesis that there is a +nom feature (or feature value) in Icelandic syntax. If so, +nom is a feature (or feature value) which the morphological rules for adjectives simply ignore. If +nom is syntactically present, then there are two different ways of deriving real/apparent agreement w.r.t. a nominative specification:

- (17) a.  $X_{[\varphi[\dots], \text{Case}[+\text{nom}]]} \quad Y_{[\varphi[\dots], \text{Case}[+\text{nom}]]}$   
 $\underbrace{\hspace{10em}}_{X \text{ and } Y \text{ agree for both } \varphi \text{ and Case}}$
- b.  $X_{[\varphi[\dots]]} \quad Y_{[\varphi[\dots]]}$   
 $\underbrace{\hspace{10em}}_{X \text{ and } Y \text{ agree for } \varphi \text{ only}}$

If, on the other hand, +nom is not syntactically present (so that nominative is a true default Case within the syntax of Icelandic as well as the morphology), then all apparent agreement w.r.t. a nominative specification is an instance of the configuration in (17b).

We end this section by noting that (16) is intended as an illustration of the *type* of morphological analysis which can be given to address the concerns raised by various critics of the MTC’s analysis of the Icelandic facts. The key features of this analysis are its treatment of nominative as a morphological default case, and its distinction between the absolute default form ‘költ’ (which surfaces when an adjective has neither Case nor  $\phi$ -features) and the default nominative forms (which surface when an adjective has  $\phi$ -features but no Case features).<sup>12</sup> The use of technology taken from the DM literature is incidental. In particular, we should emphasize that the analysis is entirely neutral with regard to the status of the strong lexicalist hypothesis.

### 3.3. *Falsifiability*

Wood makes some brief remarks on the falsifiability of the BH&N theory:<sup>13</sup>

Therefore, a notational variant of Boeckx, Hornstein, and Nunes’s (2010b) analysis might say that DPs need “ $\phi$ -complete valuation” rather than Case valuation, and then draw some strong formal connection between certain morphological case values and agreement with different  $\phi$ -complete probes, while maintaining the position that control sentences are derived by A-movement. While raising and control would then be similar in that they both involve A-movement out of an infinitive, they would differ precisely where Boeckx,

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<sup>12</sup> Of course, ‘költ’ does not surface *only* for adjectives which have no Case/ $\phi$  specification, but also for neuter singular adjectives without Case, and for neuter nominative/accusative singular adjectives..

<sup>13</sup> We are a bit puzzled by Wood’s remarks on Case and  $\phi$ -features. It is our understanding that the orthodox view these days is that Case assignment is a byproduct of  $\phi$ -valuation, so that it is really  $\phi$ -features which drive A-movement (Chomsky 2001). On this understanding, Wood’s “notational variant” of BH&N just *is* BH&N interpreted in relation to currently prevalent theoretical background assumptions.

Hornstein, and Nunes (2010a,b) claim they do: only the latter involves movement into a  $\theta$ -position. This claim might seem unfalsifiable, as has been noted (see Sigurðsson 2008:418–419), but it could, in principle, be correct.

In the last sentence, “this claim” appears at first glance to refer to the claim that both raising and control involve A-movement, differing in only with regard to whether there is movement into a  $\theta$ -position. This is not, however, the claim which Sigurðsson takes to be insusceptible to any “theoretical test” in the cited passage. And of course, if Wood were really to say that the core theses of the MTC are unfalsifiable, this would frustrate the central aim of his paper, which is to establish that these theses are false (and hence falsifiable). Perhaps, then, it is BH&N’s claims regarding default case assignment which Wood takes to be unfalsifiable. However, this seems implausible without further elaboration. Surely, we cannot rule out on a priori methodological grounds the hypothesis that a particular entity has a certain kind of case morphology as the result of a default assignment rule.<sup>14</sup> What Sigurðsson in fact takes issue with in the cited passage is the claim of Boeckx & Hornstein (2006) that, in certain control examples with floating quantifiers in the embedded clause, the assignment of default nominative is “marked” process conditioned on the distance between the controller and the floating quantifier. This claim is not actually reproduced in BH&N, since (following observations of

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<sup>14</sup>Although this is not the place to discuss the philosophy of science (and we can hardly claim any expertise in this area), our impression of the field is that Popperian falsificationism (Popper 1935/1959) has never been a majority view, and nowadays has virtually no advocates whatever. Indeed, the very existence of a demarcation criterion has long been in doubt (Laudan 1983).

Sigurðsson and others) BH&N are working with a different set of assumptions about the marked/unmarked status of various case patterns.<sup>15</sup>

#### 4. Ndayiragije (2012)

Ndayiragije presents three sets of data which are alleged to raise problems for the MTC. The first is based on fronted control infinitives in Kirundi. Although this is not entirely clear from Ndayiragije's presentation, it should be emphasized that this part of his paper does not challenge the MTC itself (which is a theory of Obligatory Control relations) but rather the supplementary thesis that Obligatory and Non-Obligatory Control relations are in complementary distribution (Hornstein 2001:56-58, Boeckx & Hornstein 2004:§3.5). That this thesis is false can in fact be shown using English data:<sup>16</sup>

- (18) a. John<sub>1</sub> believes that [*pro*<sub>1</sub> washing himself] would delight Bill<sub>2</sub>.  
b. John<sub>1</sub> believes that [PRO<sub>2</sub> washing himself] would delight Bill<sub>2</sub>.

Such examples are a central concern of Boeckx & Hornstein (2007), who develop a parsing-theoretic account of the distribution of NOC PRO. Ndayiragije does not discuss B&H's proposals in this regard, but we believe that they can also account for the Kirundi examples which Ndayiragije discusses.<sup>17</sup>

<sup>15</sup>See first complete paragraph of BH&N, p. 123.

<sup>16</sup>An anonymous reviewer points out that it is not entirely obvious that OC is possible in (18b), since *pro* could in principle take *John* as an antecedent in (18a) to yield the same interpretation. However, (18b) has (for us at least) an obligatory *de se* reading, a hallmark of OC. This suggests that PRO is both possible and preferred to *pro* in this construction under the relevant interpretation.

<sup>17</sup>BH&N assume that the parser has two key properties: (i) it prefers to postulate movement dependencies instead of pronominalization dependencies where possible; and (ii) it assigns interpretations to traces and pronouns as soon as possible. In the examples Ndayiragije discusses, there is a conflict between (i) and (ii). If subject of the fronted infinitive is parsed as a trace (i) is satisfied but (ii) is not (since the parser must

Ndayiragije's second data set relates to attempts to account for Visser's generalization within the MTC. Visser's generalization (Bresnan 1982) is the generalization that subject control predicates do not passivize. Ndayiragije rightly points to some Kirundi data which are problematic for the analysis of Visser's generalization proposed in Boeck & Hornstein (2004). However, the data which Ndayiragije discusses are very similar to the data discussed in Boeckx, Hornstein & Nunes 2010a:132-136. BH&N propose a new analysis of Visser's generalization which Ndayiragije does not address. As far as we can see, Ndayiragije's Kirundi data would not be problematic for BH&N's analysis.<sup>18</sup>

Ndayiragije's next argument focuses on the puzzle posed by *promise*. This verb is one of few exceptions in English to the Minimal Distance Principle of Rosenbaum (1967). In the MTC, the Minimal Distance Principle is a corollary of Minimality. The basic datum is illustrated in (19). Although *promise* takes an object, it is the subject which controls:

(19) John<sub>1</sub> promised Mary<sub>2</sub> [PRO<sub>1/\*2</sub> to leave].

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wait indefinitely to find the antecedent of the trace). On the other hand, if parsed as a pronoun, (ii) is satisfied (since *pro* can be assigned a referent immediately) but (i) is not. It seems that as in English examples such as (18), the parser can be pulled in either direction, so that both OC and NOC are possible.

<sup>18</sup>Ndayiragije presents one additional argument against the MTC from passivization phenomena. This argument begins with the observation that in TECs with passivized control predicates, PRO cannot be replaced with an overt subject (whether or not this subject undergoes inversion). Ndayiragije takes this to be unexpected when the subject is inverted, since on his analysis, the Focus position which is the landing site of the inverted subject serves as a surrogate Case-licensor. In Ndayiragije's view, this distributional fact is to be explained simply via the stipulation that T in a control infinite requires a PRO specifier. Ndayiragije's argument here is entirely dependent on his analysis of subject inversion, according to which Focus serves as a surrogate Case-licensor. One might equally well take the facts which Ndayiragije points to as a challenge to his analysis of subject inversion in Kirundi.

Boeckx & Hornstein (2003) and Hornstein (2001), propose to reconcile (19) with Minimality via. the introduction of a null preposition. The structure introduced by this preposition blocks the c-command relation between *Mary* and the base position of *John*, so that *John* can move over *Mary* without violating Minimality:

(20) John promised [<sub>PP</sub> [<sub>P</sub> 0] Mary] [~~John~~ to leave].

Ndayiragije's two central contentions in relation to *promise* are the following. First, that there is a verb in Kirundi which displays essentially the same control behavior as *promise*; second, that the null preposition analysis cannot be correct for Kirundi. Unfortunately, Ndayiragije provides very little by way of argument for the second contention. This is crucial, since the mere fact that there is a Kirundi verb which patterns with *promise* poses no threat to the MTC (or at least, no greater threat than the English data alone). The only potentially relevant data point which Ndayiragije points to is the ability of the benefactive argument of the relevant Kirundi verb to bind a variable in the theme (see his example (7)). He takes this as evidence for a structure in which the benefactive c-commands the theme (or the control complement<sup>19</sup>). Similar examples can also be given in English:

(21) I<sub>1</sub> promised each parent<sub>2</sub> PRO<sub>1</sub> to take care of his<sub>2</sub> child.

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<sup>19</sup> Ndayiragije's example (7) shows the double object form of the Kirundi equivalent of *promise* rather than the control form, so it is not clear that this example has any implications whatever for the configuration of arguments in the control structure. In any case, we will assume that the benefactive is also able to bind variables in the control clause.

It is well known that variable binding is not constrained by strict c-command, so there is no reason to think that the presence of a null preposition introducing *each parent* in (21) should block binding. Thus, we do not see that Ndayiragije has given any argument against extending the null preposition analysis of *promise*-type control verbs to Kirundi.<sup>20</sup>

## 5. Conclusion

We have seen that the data presented in Wood 2012 do not pose any difficulty for the core thesis of the MTC that control relations are A-movement relations. Indeed, Wood's data can be construed as supporting the theory, since the interaction of extraposition with raising and control follows directly from the theory of movement assumed by the MTC. The data which Ndayiragije presents seem largely uninformative with regard to the MTC at present (except insofar as they replicate well-known English examples whose implications for the MTC have already been quite thoroughly hashed out).

Should we conclude, then, that all is well with the MTC? Far from it. The MTC is but one component of a research program which seeks to analyze all non-local syntactic dependencies in terms of A-movement and A'-movement. There are all sorts of problems facing this research program, and we would like to close by highlighting three of them.

(i) Sideward movement and Merge over Move. As we have seen in section 2, these two theoretical innovations are crucially implicated in the MTC's analysis of adjunct control. Nunes (1995) imposed a strict c-command constraint on sideward movement, requiring that one copy c-command all of the other copies in the final output. However, Hornstein (2001,2009) points out that there are a small number of OC

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<sup>20</sup> Ndayiragije also points to an instance of control shift with the Kirundi equivalent of *promise*. Control shift is a puzzling phenomenon, but Ndayiragije does not make it clear why the Kirundi example he cites raises problems which the familiar English examples do not.

configurations which are incompatible with this requirement. This raises two very interesting, and difficult, theoretical questions. First, is there any further empirical motivation for relaxing the c-command requirement? At present, we do not know of any compelling cases.<sup>21</sup> Second, how is sideward movement to be constrained? Subnumerations can be used to prevent some of the more “wild” cases of overgeneration (Boeckx, Hornstein & Nunes 2010a), but we still require an explanation of why sideward movement is the exception rather than the rule. Drummond (2011) argues that Merge over Move has the effect of restricting movement to a c-command configuration in most instances.<sup>22</sup>

(ii) Non-finite complementation. The MTC predicts that non-finite complements should all behave similarly w.r.t. control, since they are all transparent for A-movement. This implies that in general, any kind of complement clause which permits control should also permit raising, and vice versa. This is a strong prediction. Early indications are that it is correct. For example, Greek and Romanian allow both raising and control into subjunctive clauses, and Brazilian Portuguese allows both raising and control into indicative clauses (Boeckx, Hornstein & Nunes 2010a:70-74). However, further research might uncover languages in which raising and control do not pattern identically in this respect, and the existence of such languages would pose a significant problem for the MTC.

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<sup>21</sup>Hornstein (2001) points to examples such as *PRO<sub>i</sub> seeing Mary annoyed John<sub>i</sub>*. Sub-command phenomena in Chinese are also suggestive. See also Bruening & Tran (2006).

<sup>22</sup>Merge over Move, as Hornstein understands it, is a global economy condition. Drummond (2011), building on Graf (2010), argues that Merge over Move can nonetheless be formulated in a computationally constrained manner in a framework which permits sideward movement. However, more work remains to be done on formalizing Merge over Move and determining its precise empirical consequences.



(iii) Phases. The interaction of the MTC with phase theory has not so far received much attention. Since the MTC makes no appeal to phases, one option for a proponent of the MTC is simply to reject phase theory. However, if we do attempt to reconcile the MTC with phase theory, some interesting issues arise. First, there is a threat of overgeneration. Recall from section 2 that the MTC gives the following explanation for why control into adjuncts is possible even though *wh*-movement out of adjuncts is not. In the case of control, the controller can move out of the adjunct-to-be before it is merged as an adjunct, since the target of movement, [Spec,vP], is lower than the adjunction site. In contrast, *wh*-movement targets [Spec,CP], which is higher than the adjunction site. The overgeneration problem that arises in connection with phase theory is as follows. If *wh*-movement proceeds via the edge of the vP phase, it may be possible for the *wh*-phrase to move to the edge of vP prior to adjunction. Thus, there is a prima facie conflict between the MTC and the assumption that *wh*-movement proceeds via [Spec,vP].<sup>23</sup> An additional problem arises in connection with (ii) above. If control complements are CPs (as seems plausible<sup>24</sup>), then why is A-movement out of them not blocked by the PIC? One possibility is that controllers move out of CP via its left edge.<sup>25</sup> This would suggest that A-movement can make use of phase edges as intermediate landing sites (Legate 2003). Alternatively, it may be that the relevant CPs are “weak”, and hence not Spellout domains.

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<sup>23</sup>This assumption is not necessarily tied to phase theory. It also a crucial component of e.g. the *Barriers* theory of Chomsky (1986).

<sup>24</sup>Rizzi (1982), Landau (2003:488).

<sup>25</sup>Apparent instances of OC into finite clauses conditioned on the form of the complementizer lend some plausibility to this hypothesis (Potsdam & Polinsky 2007).

There is, then, no reason to be sanguine regarding the future fortunes of the MTC. We will have to see how satisfactorily (i)-(iii) and other issues can be resolved while retaining the explanatory and empirical successes of existing formulations of the MTC. Like any nontrivial theory, the MTC is susceptible to empirical evaluation only when supported by a web of background assumptions. It is a commonplace observation that problematic data points do not come packaged with instructions for pruning, rearranging and adding to this web. Many of the challenges to the MTC considered above involve phenomena which are themselves very poorly understood, such as quirky Case or non-obligatory control. In practice, all that such arguments tend to demonstrate is that we don't know very much about the phenomena in question. The real question is: are there alternatives to the MTC which are clearly superior in terms of explanatory scope and empirical range? The reader will not be surprised to hear that our answer to this question is "No". However, there is certainly room for reasonable people to disagree on this point. The MTC and its competitors are all worthy of, and in need of, further development. It is only by continuing to develop these competing theories that we can learn something more about the range of plausible candidate theories of control, and of grammatical dependencies more generally.

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