Some Purported Problems for the Movement Theory of Control

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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

We begin in section 2 with a brief outline of the MTC. Section 3 responds to the arguments in Wood (2012) and section 4 to those in Ndayiragije (2012). We conclude 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 [$_{TP}$ ~~[John]~~ to win].

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

(2) $[_{TP}$ [John] $[_{vP}$ ~~[John]~~ wants $[_{TP}$ ~~[John]~~ to $[_{vP}$ ~~[John]~~ win]]]].

θ-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.[2]

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 (Nunes 1995, Bobaljik & Brown 1997, Uriagereka 1998). An example adjunct control derivation is given in (3):

(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) $[_{CP}$ C $[_{TP}$ [John] $[_{vP}$ ~~[John]~~ laugh at Bill]]] *Workspace 1*
$[_{PP}$ 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 CP, the derivation could converge on the structure in (6):

(6) $[_{CP}$ [who] $[_{CP}$ C $[_{TP}$ [John] $[_{vP}$ ~~[John]~~ laugh at Bill]]]
$[_{PP}$ 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:

(7) $[_{CP}$ C $[_{TP}$ $[_{TP}$ [John] $[_{vP}$ ~~[John]~~ laughed at Bill]] $[_{PP}$ 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. The derivation therefore cannot resolve to a single workspace and crashes. In the control derivation in (3), 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 (2012)

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 begin by examining Wood's argument based on extraposed infinitives in Icelandic.

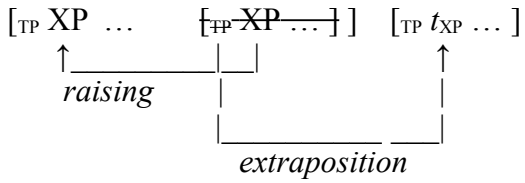
3.1. Extraposed infinitives

Wood, following Thráinsson (1979), notes that Icelandic control infinitives can occur together with the Case-marked pronoun *það* ('it'). On Thráinsson's analysis of this construction, the infinitive is extraposed and the pronoun is its associate. A′-movement and raising are impossible out of extraposed infinitives, but control into extraposed infinitives is possible. To all appearances, then, the correct generalization regarding extraposed infinitives is that they are islands for both A and A′-movement. Under non-movement analyses of control, it is unsurprising that control into extraposed infinitives is nonetheless possible. Wood argues that there is no principled way of reconciling these facts with the MTC, since if both control and raising are A-movements, they should be subject to the same constraints on extraction.

Echoing the preceding literature on the MTC, we emphasize once again that the MTC is *not* a raising theory of control. Differences between raising and control are perfectly compatible with the MTC if these can be shown to derive from the differing targets of A-movement in each instance: a Case position for raising and a θ-position for control. Wood's data provide a further illustration of this point. 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 therefore: 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:

(8)



The logic is identical for control, but for the fact the initial movement out of the embedded infinitive targets Spec,vP rather than Spec,TP. Given the hypothesis in (9), the MTC therefore provides a straightforward account of Wood's data:

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

So far we have worked on the assumption that extraposition is movement. If the extraposed infinitive is based-generated in an adjoined position then both control and raising are sideward rather than upward movements. The (illicit) derivation for raising is shown in (10), with the position where the extraposed infinitive is interpreted marked by [TP *e*]:

(10) *Illicit raising derivation which would be available if (9) were false*

Stage 1

Matrix clause is constructed up to T′ in workspace 1; extraposed associate of [$_{TP}$ *e*] is constructed in workspace 2:

Workspace 1	*Workspace 2*
[$_{T'}$ … [$_{TP}$ *e*] …]	[$_{TP}$ XP …]

Stage 2

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

Workspace 1	*Workspace 2*
[$_{TP}$ XP [$_{T'}$ … [$_{TP}$ *e*] …]	[$_{TP}$ ~~XP~~ …]
↑_____	_____|

Stage 3

Infinitive clause adjoins to matrix clause:

[$_{TP}$ [$_{TP}$ XP [$_{T'}$ … [$_{TP}$ *e*] …]] [$_{TP}$ ~~XP~~ …]]

Once again, raising is possible only if (9) is false. In the parallel control derivation, sideward movement of XP targets Spec,vP, and this movement is licit regardless of (9).[3]

How empirically plausible is the hypothesis in (9)? We don't know about Icelandic, but there is well-known evidence that is correct 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]$_1$ he should t_1

In addition, extraposition fails to bleed Condition C effects triggered by subject pronouns, which follows immediately if extraposed clauses sit below TP:

(12) * He$_1$ hates it that we don't like John$_1$.

In sum, rather than Wood's Icelandic data being problematic for the MTC, they may well constitute another argument in its favor.[4]

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 (which are in fact mentioned briefly by BH&N in footnote 8 (p. 122), and implicitly addressed on p. 120). The objections are 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) Honum er kalt/*kaldur/*köldum.
him.D *is* *cold.DFT*/**N.M.SG*/**D.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):[5]

(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')

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. The data in (14) are in fact 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 both Case and φ agreement so that the adjective surfaces in the invariant form. In (14b), by contrast, φ-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 φ-agreement. Thus, there is no reason to expect the adjective to surface in the invariant form.

To make the preceding discussion a little more concrete, we will now sketch a toy Distributed Morphology-style analysis for the adjective *kaldur* ('cold').[6] The table in (15) gives the full strong paradigm for *kaldur*, which (modulo a few vowel changes) follows the usual pattern of *-ur* adjectives in Icelandic. Since we are not concerned with the morphology of Icelandic adjectives as such, we simply give the full form of the adjective on the left of each vocabulary item.[7] It is helpful to be able to group masculine and feminine nouns and identify feminine nouns via a single feature. We therefore make use of two features ±A and ±B to specify gender, with masculine = [+A,+B], feminine = [+A,-B] and neuter = [-A,+B].[8] Masculine and feminine share [+A] while [-B] uniquely identifies feminine. Apart from this, we make use of a ±pl (plural) feature, and ±nom, ±acc, ±dat and ±gen features for Icelandic's cases. The toy analysis for the paradigm in (15) is given in (16):

(15)

	Singular			**Plural**		
	Masc	**Fem**	**Neuter**	**Masc**	**Fem**	**Neuter**
Nom	kaldur	köld	kalt	kaldir	kaldar	köld
Acc	kaldan	kalda	kalt	kalda	kaldar	köld
Dat	köldum	kaldri	köldu	köldum	köldum	köldum
Gen	kalds	kaldrar	kalds	kaldra	kaldra	kaldra

(16) **Vocab items:**

1. ‘kalt’	↔	[_]	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,_]	**Impoverishment rules:**		
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]

A Python script for computing the paradigm in (15) from the rules given in (16) is available at http://gist.github.com/addrummond/4548031.

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 ‘kalt’, and it is therefore this form which surfaces when there is specification for neither Case nor φ-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 φ-agreement (and PRO has no Case features), with the consequence that the only matching vocabulary item is the one for

'kalt'. In (14b), PRO likewise has no Case features, but since φ-agreement has occurred, the vocabulary item for 'kalt' is blocked by the more specific vocabulary item for 'kaldur', which is specified [+A,+B] (masculine).

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 (or the lack thereof) 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 spells 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 required morphological analysis is plausible. 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 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 highly underspecified form of the adjective which surfaces in the absence of any Case or φ-features.

In this light, consider objection (ii) above. Bobaljik & Landau's 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 in detail; "structural case" is simply 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.[9]

Since the presence of a +nom feature makes no difference to the morphological realization of an adjective, the present analysis leaves it an open question whether Icelandic has syntactic nominative at all. If +nom is syntactically present, then there are in general two ways of deriving real/apparent agreement w.r.t. a nominative specification:

(17) a. $X_{[\varphi[...],Case[+nom]]}$ $Y_{[\varphi[...],\ Case[+nom]]}$
X and Y agree for both φ and Case

b. $X_{[\varphi[...]]}$ $Y_{[\varphi[...]]}$
X and Y agree for φ only

If, on the other hand, +nom is not syntactically present (so that "nominative" elements are simply Caseless elements), then all apparent agreement w.r.t. a nominative specification is an instance of the configuration in (17b).

3.3. Falsifiability

Wood makes some brief remarks on the falsifiability of the BH&N theory:[10]

> Therefore, a notational variant of Boeckx, Hornstein, and Nunes's (2010b) analysis might say that DPs need "φ-complete valuation" rather than Case valuation, and then draw some strong formal connection between certain morphological case values and agreement with different φ-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, Hornstein, and Nunes (2010a,b) claim they do: only the latter involves movement into a θ-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 θ-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 (hence falsifiable). Perhaps, then, it is BH&N's claims regarding default case assignment which Wood takes to be unfalsifiable. However, we surely 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.[11] 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 Sigurðsson and others) BH&N are working with a different set of assumptions about the marked/unmarked status of various case patterns.[12]

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:[13]

(18) a. John_1 believes that [pro_1 washing himself] would delight Bill_2.

b. John_1 believes that [PRO_2 washing himself] would delight Bill_2.

Examples of this sort are discussed in Boeckx & Hornstein 2007, which develops a parsing-theoretic account of the distribution of NOC PRO. Ndayiragije does not discuss

B&H's analysis, but we believe that it can also account for the Kirundi examples which Ndayiragije discusses.[14]

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 data discussed in Boeckx, Hornstein & Nunes 2010a:132-136, where 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.[15]

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$_1$ promised Mary$_2$ [PRO$_{1/*2}$ to leave].

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 [$_{PP}$ [$_{P}$ 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 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[16]). Similar examples can also be given in English:

(21) a. I_1 promised each parent$_2$ PRO_1 to take care of his$_2$ child.

b. I_1 made a promise to each parent$_2$ PRO_1 to take care of his$_2$ child.

As is well known, variable binding is not constrained by strict c-command at Surface Structure, so Ndayiragije's data appear to be uninformative with regard to the presence or absence of a null preposition.[17]

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 together with the plausible hypothesis (9). The data which Ndayiragije presents seem largely uninformative with regard to the MTC (except insofar as they replicate well-known problematic English data).

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 a unified analysis of a wide range of syntactic dependencies in terms of A- 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 sidewards movement, requiring that one copy c-command all of the other copies in the final output. Hornstein (2001,2009) points out that there are a small number of OC configurations which are incompatible with this requirement. This raises two very interesting and difficult theoretical questions. First, is there any further empirical motivation for removing the c-command requirement? At present, we do not know of any compelling cases.[18] Second, if the c-command requirement is removed, how is sideward movement to be constrained? Subnumerations can be used to prevent some of the more "wild" cases of

overgeneration, and Merge over Move tends to prevent sideward movement into deeply embedded positions. A key empirical issue here is the correct analysis of "subcommand" configurations, which suggest that sideward A-movement into DP specifiers may sometimes be permitted.

(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. A related question is whether all control complements are CPs, and if so, whether movement out of a control complement proceeds via Spec,CP. Apparent instances of OC into finite clauses conditioned on the form of the complementizer lend some plausibility to this hypothesis (Potsdam & Polinsky 2007).

(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 perfectly viable 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 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.[19]

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. 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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[1]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.

[2]It is clear that the EPP cannot be the crucial factor, since DPs may control from non-subject positions. 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 8). The core claim is simply that whatever drives A-movement in (i)-(ii) is also what drives A-movement to subject and object case positions in instances of subject and object control:

(i) [John] seems [$_{TP}$ ~~[John~~] to have left].

(ii) John believes [$_{AgrOP}$ [Bill] [$_{TP}$ ~~[Bill~~] to be intelligent]].

[3]With regard to A′-movement, note that A′-movements are typically (or always?) driven by heads above T. The islandhood of extraposed infinitives for A′-movement therefore follows on the same logic. An issue arises here in connection with successive-cyclic movement through Spec,vP (see also point (iii) of the conclusion). We cannot permit sideward *wh*-movement into Spec,vP followed by upward movement to Spec,CP. If *wh*-movement stops off in Spec,vP, there must therefore be a ban on successive-cyclic sideward movement. This may follow if successive-cyclic movement is driven not by feature checking but by a requirement that chain links be a short as possible (Bošković 2002, Takahashi 1994). Sideward movement to Spec,vP is no "shorter" than sideward movement direct to Spec,CP, given Hornstein's (2009) path-based conception of Minimality.

[4]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 adjunct control (noted in Lasnik:1992:244).

[5]Note that Sigurðsson's gloss in (14b) embodies his assumption that this instance of PRO has structural nominative case (an assumption which we and BH&N reject).

[6]On DM, see Harley & Noyer (1999) and references cited therein. Our use of technology taken from the DM literature is incidental and implies no commitment to its foundational assumptions.

[7]A non-toy DM analysis would 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.

[8]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 items identify feminine adjectives solely via the [-B] specification. The impoverishment rules apply in order. Rule 16 may bleed rule 18, but this has no effect on output.

[9]There is, however, an issue here relating to the analyses originally proposed by B&H and BH&N. Both analyses appear to rest on the assumption that default nominative arises from some kind of post-syntactic morphological case-assignment rule. An anonymous reviewer argues that on this analysis, one might expect a default nominative specification to be purely morphological and hence invisible to syntactic agreement (so that no syntactic process could transfer PRO's syntactically-invisible default nominative Case onto the adjective). The present analysis obviates this concern since there is no default case assignment rule.

[10]It is our understanding that the orthodox view these days is that Case assignment is a byproduct of φ-valuation, so that it is really φ-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.

[11]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).

[12]See first complete paragraph of BH&N, p. 123.

[13]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.

[14]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 wait indefinitely to find the antecedent of the trace). On the other hand, if it is parsed as *pro*, (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.

[15]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 infinitive requires a PRO specifier. Ndayiragije's argument here is entirely dependent on the hypothesis that Focus can serve as a surrogate Case-licensor. One might equally well take Ndayiragije's data as a challenge to this hypothesis.

[16]Ndayiragije's example (7) does not actually show *promise* taking a control complement; we will assume that the benefactive is able to bind variables in the control clause.

[17]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. See Boeckx, Hornstein & Nunes (2010) for discussion.

[18]Hornstein (2001) points to examples such as "PRO_1 seeing Mary annoyed $John_1$." Sub-command phenomena in Chinese are also suggestive. See also Bruening & Tran (2006).

[19]This assumption is not necessarily tied to phase theory. It also a crucial component of e.g. the Barriers theory of Chomsky (1986).