Remarks and Replies

Movement under Control

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We examine the three categories of empirical argument that Landau (2003) puts forward against a movement theory of control (MTC): overgeneration cases, alleged arguments in favor of an MTC, and raising/control contrasts. We show that the problems cited either have plausible alternative analyses that leave the MTC unscathed or, in fact, are not nearly as dire for the MTC as Landau supposes. We conclude that the ''standard'' theory enjoys no obvious empirical advantages over the MTC and that the MTC is superior on conceptual and methodological grounds.

Keywords: control, minimalism, movement, raising, θ-role

Control has recently become a hot topic largely for the light it promises to shed on minimalist approaches to grammar. We believe that the reductive impulse behind the movement theory of control (MTC) in its various versions fits snugly with the explanatory ambitions of the Minimalist Program. If empirically viable, reduction *always* enhances explanation and so is *always* methodologically favored. The only relevant questions are whether a proposed reduction is technically possible and at what empirical cost, given the perennial tension between data coverage and explanatory depth. It is against this backdrop that we will consider the objections proffered in Landau 2003.

Landau 2003 enumerates several (mostly empirical) arguments against the MTC.[1] Showing that these arguments are inconclusive (or better still, incorrect) will add empirical heft to the aforementioned methodological advantages of the MTC. Consequently, we welcome this opportunity to consider the issues raised in Landau 2003.

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[1] The arguments are specifically directed against Hornstein 1999. However, many would extend against any version of the MTC, and there are several, including those developed in Bowers 1973, Manzini and Roussou 2000, O'Neil 1997, and Martin 1996. Indeed, Landau's own (1999, 2001) proposal to analyze obligatory control as an instance of Agree arguably belongs to this category, as Agree is the operation underlying movement in current formulations of the Minimalist Program (see Chomsky 2000, 2001). Ironically perhaps, once control is treated in terms of Agree, it becomes very hard to rule out movement within control structures in a principled fashion given that the latter amounts to Agree plus ancillary pied-piping induced by an EPP requirement. To put this another way, if Landau's (1999, 2001) proposal is correct, then it would be quite puzzling if *no* language derived control via movement given that movement is simply Agree plus a little more.

Before starting, however, it pays to recall Chomsky's (1993) observations. There are advantages to meeting Occam's demands. The relevant one here is the dictum that methodologically superior options must merely show that they are empirically no worse than their competitors to be judged preferable. Covering yet more ground is a bonus. We believe that the MTC has enjoyed many empirical successes. We will not concentrate on them here (but see footnote 28 for some relevant references). Rather, we will focus on the arguments in Landau 2003 and show that the problems cited either have plausible alternative analyses that leave the MTC unscathed or, in fact, are not nearly as dire for the MTC as Landau supposes.

The structure of this article is as follows. To allow for easy comparison, we go through the arguments in Landau 2003 in order. After sketching the views of control to be contrasted and making a brief point about the featural status of θ-roles, we examine the three categories of empirical argument that Landau brings to bear on the issue: overgeneration cases, alleged arguments in favor of a movement theory of control, and raising/control contrasts.

1 Two Views on Control

The MTC holds that obligatory control (OC) is a subspecies of (A-)movement. In standard cases, then, control approximates familiar instances of raising. Specifically, just as raising takes an element from the (embedded) lexical domain and moves it to the subject position of the finite clause (1), so control takes an element from the (embedded) lexical domain, remerges it in the (embedding) lexical domain, and finally moves it to the subject position of the finite clause (2).

(1) [$_{TP}$ John [$_{VP}$ seemed [to [$_{VP}$ ⟨John⟩ like apples]]]]

(2) [$_{TP}$ John [$_{VP}$ ⟨John⟩ tried [to [$_{VP}$ ⟨John⟩ like apples]]]]

Note: although control *approximates* raising under such a view, the two instances of movement *cannot be collapsed.* There is an important difference between the two. In control, but not raising, the moving element remerges into a θ-position prior to reaching Spec,TP. The principal MTC strategy is to explain all differences between raising and control in terms of this one difference.

The MTC contrasts with what Landau (2003) calls the ''standard'' theory of control. According to Landau (p. 473), the latter makes the following three assumptions:

(3) a. PRO exists, and it is distinct from NP-trace.
    b. Hence, control involves two argument chains, while raising involves one.
    c. The control module exists.

Landau (pp. 473–474) claims that

> the assumptions in (3) are as minimal as one can get. Thus, they are not committed to any *particular* view of PRO (anaphor, pronoun, Ā variable, etc.)—only to its nonidentity with NP-trace. Nor are they committed to any particular view of the control module—the choice of controller and the interpretation of PRO may involve predication, lexical entailments, pragmatic factors, and so on . . .

We agree with Landau in the appraisal that (3) is ''minimal'' (though in the unenviable sense of saying next to nothing, in our view), but we think that this is a serious problem. (3) does not suffice to limn any theory at all. The two core elements of (3)—the precise nature of PRO and the details of the control module—are left completely unspecified.

A theory is a set of assumptions that, once adopted, help narrow analytic options. The MTC is a theory: it rests on a construct—A-movement—that is well understood and on the supposition that movement into θ-positions is licit. It is a purely syntactic view of OC that leaves little explanatory work for other components of the grammar (or mind) in accounting for OC's basic properties. This may be right or wrong (and it has proven all too easy to see what could make it wrong!), but it is relatively clear. Not so for (3): What is PRO? What makes it distinct from NP-traces? How is it licensed? What does *involve* mean in (3b)? Does it mean movement, or some rule of construal? Can it be either, depending on the situation? Can it be a parameter? What does the control module consist of? Is it some amorphous bag? We don't know how to answer those questions.[2] And this is because (3) lacks sufficient specificity. Worse still, the vagueness of (3) also leaves it unclear how and/or if the assumptions in (3) suffice to account for the problems that Landau enumerates.

So, from the start, the comparison between the MTC and (3) is not really apposite. However, because Landau focuses largely on the empirical shortcomings of the MTC and because we will be trying to parry these conclusions (rather than comparing alternative proposals), we will put this to one side.[3]

2 Alleged Internal Problems

Landau (2003) begins by outlining some alleged internal problems for the MTC and argues against the supposition that θ-roles function like features in licensing movement.[4] The latter is required if the MTC is to be viable. Is this supposition reasonable?[5]

We believe it is, for at least three reasons. First, the ban on movement into θ-positions has largely been motivated on theory-internal grounds. Moreover, these grounds have proven to be

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[2] Nor does anyone else so far as we can see. To date, very little is known about the control module. Its powers are generally invoked when there is no principled or insightful alternative at hand.

[3] Of course, the fact that we are not comparing proposals here should not prevent interested readers from doing it for themselves. Looked at in detail, it is remarkable, in our opinion, how little PRO-based theories explain. What they tend to do is track the data rather than explain them.

[4] Given the current technology, there is something archaic about worrying whether θ-features can license movement. The reason is that, lately, movement is forced by the Extended Projection Principle (EPP) and nothing else. Features are largely irrelevant. Moreover, what the EPP is, is far from clear. In its OCC(urrence) guise, it is not a feature at all, at least the way Chomsky (2000, 2001) talks of it. Note that if movement is divorced from features, then arguing that θ-roles cannot license movement because they are not featural is really beside the point. We proceed, however, on the earlier (and, we think, correct) assumptions that feature checking drives movement and that θ-roles are in the same boat as Case and agreement in this regard.

[5] Note that we say *reasonable,* not *true.* The reason is that this is not the sort of issue that will be settled by casual inspection of some data sets but by seeing if the assumption proves fruitful.

very weak. In fact, as noted in Hornstein 2001, the main reason for the ban has been the distinction between raising and control properties, which, it appears, can (largely) be accommodated without making this assumption. Couple this with some intriguing empirical evidence that such movement is needed elsewhere (see Bošković and Takahashi 1998, Hornstein and Motomura 2002, Rodrigues 2003), and the general prohibition against movement into θ-positions loses its privileged status. It becomes just another example of a principle which though plausibly true is also plausibly false. Second, there is nothing semantically amiss if we assume that the same element occupies various thematic/argument positions. This, in fact, is how variables in logic are generally understood. In short, were movement between θ-positions possible, we would know how to interpret the resulting structure. Third, in an approach to grammatical architecture like the Minimalist Program that does not recognize a level of representation like D-Structure, such movement would actually be quite natural. Thus, if in contrast to Government-Binding-style theories one dispenses with D-Structure, then it is natural to dispense with the restrictions that D-Structure brought with it. One of these is movement into θ-positions. Of course, one *need* not dispense with the ban on movement into θ-positions if one dispenses with D-Structure (see, e.g., Chomsky 1995, 2000). But it is certainly a natural option to consider. For all these reasons, then, it is *reasonable* to consider what grammars would look like if they allowed this kind of derivational option, and this is what the MTC does.

    Landau (2003) observes that θ-roles, unlike other features, are clearly relational: a DP is plural regardless of its syntactic environment, but whether an NP is a theme or an experiencer depends on its syntactic context. The observation has some merit, but is not clearly relevant. First, many features that drive narrow syntax are relational: Case on NPs, ϕ-features on v/T, and so on. For example, whether an NP is nominative or accusative depends on its syntactic environment, but few would deny that Case is a feature. Second, Landau does not distinguish between *features* (θ-, Case, ϕ-, etc.) and feature *values* (theme, dative, plural). It has been convincingly argued that thematic values are irrelevant to syntax (see Lasnik 1988), just like other features (languages raise NPs regardless of whether they are plural or singular). But it has been equally convincingly shown that θ-roles (say, the number of θ-roles assigned/checked) play a role in narrow syntax. Classing θ-roles as features is one way of cashing this out. It may even be an optimal way, in that it allows us to view every instance of Merge as ''greedy''—that is, forced by checking (subject to Last Resort).[6] And just as NPs can participate in multiple agreement relations, so the null hypothesis would be that they can participate in multiple θ-relations.

    Landau also claims that viewing θ-roles as features introduces ''massive redundancy'' into the grammar, as all thematic information now needs to be encoded at the semantic interface and in the grammar. Again, distinguishing between features and feature values would relieve this

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[6] If Merge and Move are actually just two instances of Merge (internal vs. external), then if internal Merge must be greedy in the sense of having to check a feature, then so too must external Merge—which, after all, is the same operation. This virtually *requires* that one treat θ-roles as features in the sense required for the MTC: they suffice to license movement.

tension (thereby decreasing redundancy), as it is plausible that the interface focuses primarily on values, while narrow syntax makes sure that argument NPs check θ-roles.[7]

In addition, all recent (neo-Davidsonian) attempts to sever arguments from verbs encode θ-roles in (semi)functional projections (Borer 2003, Lin 2001, Ramchand 2002, Ozturk 2004). This, in effect, treats θ-roles as features used to project structures. Interestingly, all these attempts favor a syntactic approach to word formation on grounds of economy (nonredundancy). So ultimately, a featural perspective on θ-roles may in fact reduce redundancy.

Finally, Landau argues that the featural view of θ-roles fails to distinguish between the deviance of (4a) and that of (4b).

(4) a. #Sincerity fears John.
    b. *Children fears John.

Again, although the distinction Landau points at exists, it cannot be taken as an argument against the featural view of θ-roles required by the MTC. Recall that θ-roles have always had a dual status within grammatical theory. Many have observed that in one sense, θ-roles are purely formal elements expressing differences in a predicate's adicity. The more substantive notion provides these arguments with positions and claims that they lend a standard meaning to the clauses they inhabit.[8] The MTC can make do with the weaker notion.[9] Note that *fears* is a dyadic predicate in both (4a) and (4b). Thus, from one point of view these two sentences are thematically indistinguishable. On this view, the unacceptability of (4a) is a matter not of θ-role but of the incompatibility of semantic/selectional features of the sentence's terminal elements. It is what philosophers call a category mistake, as in *The stone is thinking of Vienna* or *The bachelor is married* or *The number three is standing on the corner.* So, what one makes of these sentences (e.g., do they crash, or converge as gibberish?) depends on how one treats selectional restrictions and how one relates them to θ-roles. The MTC by itself is mute on these matters. All it requires is that θ-roles suffice to license movement. They need be featural *in no other way.*

One last point: even if we grant the premises behind this argument, it fails. Thus, for example, native speakers have a clear sense that (5a) and (5b) are deviant in different ways. This, however, does not invalidate the claim that Relativized Minimality blocks both.

(5) a. *John seems that it was told ⟨John⟩ that it was raining.
    b. ?*Who did John wonder whether Bill saw ⟨who⟩?

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[7] Though we have responded to this point, truth be told, we do not understand what Landau intends here. The observation Landau makes would seem to hold for any features whatever—say, person and number features. Does this imply that they too should be eliminated? In fact, as Howard Lasnik (personal communication) notes, if θ-roles (for example) weren't features, would the redundancy be eliminated if thematic information remained grammatically relevant? Perhaps Landau means that θ-roles are grammatically inert. But in that case, a little more argument is required. The Uniformity of θ-Assignment Hypothesis (UTAH), for example, seems to exploit θ-roles.

[8] The difference is discussed extensively by Dowty (1989) and Grimshaw (1990).

[9] The Government-Binding Theory θ-Criterion was also concerned with the first, formal notion, not the second, substantive one.

In sum, it is not too hard to find analogies between θ-roles and those elements that are uncontroversially taken to be features. In addition, there is by now some nonnegligible empirical evidence in favor of θ-roles-as-features; see, for example, Bošković 1994 (on restructuring), Bošković and Takahashi 1998 (on scrambling), Hornstein and Motomura 2002 (on psych verbs), and Rodrigues 2003 (on secondary predicates). These works all assume that θ-roles license movement. This, to repeat, is the only sense in which the MTC requires θ-roles to be features. We therefore conclude that the supposition that θ-roles are featural (in the sense that they can license movement) is conceptually, theoretically, and empirically reasonable.

3 Empirical Arguments

3.1 Control across Passive

The first empirical argument against the MTC has to do with the ungrammaticality of (6).

(6) *John was hoped to win the game.

(6) is to be compared with the acceptable raising case in (7) and the acceptable control case in (8).

(7) John was believed to be smart.

(8) John was persuaded to come to the party.

The gist of the argument is that raising under passivization (7) and even control under passivization (8) are both possible. How, then, can we account for (6)?

We believe the problem in (6) resides not in control but in passivization. In particular, we would like to claim that the English passive morpheme *-en* can only be attached to verbs that, when active, can Case-license the element that is passivized. Thus, only verbs that take complements that can check Case (DPs and CPs) can be passivized.[10] Thus, what *-en* does is remove the Case-checking/-assigning powers of a Case-marking verb. The following paradigm follows from this assumption:

(9) a. John believes it.
    b. John persuaded him to go.
    c. John hoped that Mary would come.
    d. John hoped *it/for it.

Assuming the *-en* attachment condition above, we expect the following pattern:

(10) a.   DP was believed
     b.   DP was persuaded

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[10] On CPs checking Case, see especially Bošković 1995.

  c.   CP was hoped/It was hoped CP[11]
  d.   *DP was hoped

This is enough to rule out (6). The MTC can remain unchanged.[12]

3.2 Sideward Movement from Complements

The second argument concerns the use of sideward movement (movement taking place between two subtrees that are not connected at the time of movement).[13] Hornstein (1999, 2001) uses sideward movement to allow movement (/''control'') out of adjuncts, as in (11).

(11) John laughed [in order to impress Mary].
     Derivation:
     a. Sideward-move *John* from adjunct-internal VP to matrix VP
     b. Merge adjunct and VP
     c. Raise *John* from matrix VP to Spec,IP

This type of derivation is defended at length in Hornstein 2001 and has been used in a wide range of constructions, well beyond control (see Nunes 2001 for review and references).[14]

   Landau (2003) observes that sideward movement appears to incorrectly rule in situations of ''control'' from Spec,DP, as in (12).

(12) *John's friends prefer [to behave himself].
     Derivation:
     a. Sideward-move *John* from embedded VP to Spec,DP
     b. Raise entire DP *John's friends* to Spec,IP

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[11] We take expletive *it* in this case to be a DP satellite, a stand-in for the subject CP, in the sense of Koster 1978.
[12] Note that we have not provided an explanation for the generalization that only Case assigners can passivize. But this is something a theory of passivization/Case should account for, not a theory of control.
   One possibility is that exceptional Case-marking (ECM) verbs can take non-CP clausal complements. If CPs are phases but TPs are not, then this would remove a phase from between the launch site and the landing site of the passivized DP.

   (i) $DP_1 \ldots$ was $[_{vP} \ldots [_{vP} \ldots ([_{CP}][_{TP} \ldots DP_1 \ldots]]](\,)$

In (i), the passivized DP_1 would move across CP and vP (this presupposes that weak vP is phaselike, as in Legate 2003) if passivized. Note that the movement plausibly crosses two phases here. In a control structure, in contrast, the movement would look like (ii), with DP_1 targeting Z or X depending on whether it is an external or internal argument.

   (ii) $\ldots$ was $[_{vP} \ldots Z \ldots [_{vP} \ldots X \ldots ([_{CP}][_{TP} \ldots DP_1 \ldots]]](\,)$

In short, in the ''control'' cases, as opposed to the ''raising'' cases, at most one phase is traversed. If one assumed that at most one phase could be traversed, this would naturally make control possible where ''passive''/''raising'' is blocked. Of course, if ECM verbs can ''remove'' CPs or take TP complements, then we arrive at the conclusion that *believe* should passivize while *hope* should not.
[13] The discussion of sideward movement is interesting but in some sense orthogonal to the main issue. The MTC could be correct for standard cases of movement even if sideward movement does not exist. Thus, strictly speaking, the following problem raised by Landau is somewhat beside the point. That said, sideward movement is an interesting theoretical option, given certain reasonable theoretical assumptions, and it seems to do admirable service in cases of adjunct control.
[14] Landau does not discuss this literature, nor does he cite these references.

To rule out this case, we would like to adopt the idea that specifiers of DP in general raise from within the DP complement. In particular, we would like to argue that in a case like *John's friends,* a derivation like (13) obtains.[15]

(13) a. [small clause friends John] → merger of D^0 (*'s*)
      b. 's [friends John] → movement of *John* to Spec,DP
      c. John's [friends ⟨John⟩]

In (13a), *John* θ-marks *friends;* that is, the small clause roughly corresponds to *the friends are John's.* This assumption suffices to rule out sideward movement/control in (12) since it would require *John* to be both a θ-assignee (receiving/checking a θ-role from *behave*) and a θ-assigner (assigning/checking a θ-role inside the DP). In Fregean terms, this would be ruled out by saying that an element cannot be both an object and a concept.

There is a second line of inquiry concerning possessive DPs that also suffices to prevent sideward movement to Spec,DP. It has been argued that possessives are adjuncts, at least in cases like *John's book.*[16] If this is correct, then movement to Spec,DP will be blocked if there is no movement to adjoined positions, as is commonly assumed.[17]

In sum, there seem to be at least two reasonable ways of blocking sideward movement in (12) on independently motivated grounds.

3.3 Implicit Controllers

The third argument is about implicit control (14).

(14) John said (to the visitors) [to return later].

As Landau notes, ''no agreement has been reached concerning the proper analysis of [implicit control]'' (p. 478). However, he adds that ''important insights have been established.'' The insight he capitalizes on is Rizzi's (1986a) claim that implicit (dative) controllers are represented in the lexicon (as thematic slots), not in the syntax. That is, they are not instances of pro. The rationale behind Rizzi's claim is that implicit datives can control, but cannot bind (15).

(15) *John said (to the visitors) [to wash themselves].

Landau claims that Rizzi's conclusion cannot be maintained in an MTC. If control is movement, controllers must be represented syntactically; in the case of implicit control, pro is the obvious candidate. Landau notes that ''Rizzi's binding/control asymmetry can be explained in terms other than the pro/thematic slot distinction. The burden of proof, however, resides with Hornstein'' (pp. 478–479).

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[15] See Kayne 1994 and much work since, building on Szabolcsi 1983. See also Hornstein, Rosen, and Uriagereka 1996.
[16] Tellier (1990) distinguishes possessives in relational nouns like *friend* and *father* from those in alienable nominals like *John's book, John's picture, John's house.* The latter are adjuncts. Safir (1999) also treats possessive DPs as adjuncts.
[17] Such movement would typically violate Greed on the assumption that adjuncts cannot check features.

Two observations are in order here. First, no compelling argument, of the type that clearly indicates who must bear the burden of proof, can be made if ''no agreement has been reached concerning the proper analysis'' of the phenomenon one wants to base one's argument on. Second, the control/binding asymmetry observed by Rizzi is interesting, but his conclusion that implicit controllers are not represented syntactically cannot be taken as irreducible. What the lexicon looks like today may be different from what it will look like tomorrow (witness the shift of perspective introduced by Distributed Morphology). Second, Rizzi's conclusion that implicit controllers cannot be instances of pro depended on a specific theory of pro, going back to 1986. That, too, cannot be expected to remain constant. All in all, we are left with an interesting factual observation (the control/binding asymmetry), an account of which is up for grabs. We can hardly conclude from this that ''the burden of proof . . . resides with Hornstein.''

Nevertheless, we think that Hornstein's (1999, 2001) proposals can make sense of Rizzi's asymmetry. Assume that implicit controllers are syntactically represented as pro and that they control/move the way any other DP does. The fact that they cannot bind an anaphor follows, we submit, from the fact that binding also falls under the theory of movement. In particular, Hornstein (2001, 2003) has argued that anaphors are lexicalized copies of their antecedents, roughly as in (16).

(16) John has [⟨John⟩ killed ⟨John⟩ → himself]

Suppose we say that an anaphor cannot be lexicalized if its antecedent is not, which is the case with pro.[18] (This may well be another illustration of the tendency languages show to lexicalize higher copies, and not lower ones. In this case, lexicalization of a lower copy would be dependent on lexicalization of a higher one.) From this it would follow that pro can control (nothing needs to be lexicalized), but cannot bind. Notice that this account not only makes sense of Rizzi's asymmetry, but also keeps to the idea that control is syntactic. Rizzi's original account would force us to say that lexical control exists along with syntactic control (and possibly other kinds of control). Deciding which control takes over when is no trivial matter, and certainly weakens the theory (see our remarks on (3) above). So our account leads to a more restrictive theory.

3.4 The Status of the Minimal Distance Principle

Landau (2003) revamps the old observation that *promise* (17) (and a handful of verbs patterning like it) does not behave like other transitive verbs (18) in that it fails to demand object control and instead requires subject control.

(17) John promised Mary [to ⟨John⟩/*⟨Mary⟩ go to England].

(18) John persuaded Mary [to *⟨John⟩/⟨Mary⟩ go to England].

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[18] Italian-style pro can bind, because that pro is lexically realized as inflection on the verb, as many have independently argued.

The control choice in (18) has been argued by Rosenbaum (1967) to follow from the Minimal Distance Principle (''Pick the closest controller''), which the MTC is able to deduce from Shortest Move (''Move the closest candidate''). That (17) does not pattern like (18) suggests to Landau that it is a mistake to implicate the Minimal Distance Principle in (18), let alone to deduce it from something deep like Shortest Move. Culicover and Jackendoff (2001) previously voiced a version of the *promise* argument, and we have already tried to make clear why we think that argument is incorrect (see Boeckx and Hornstein 2003c:272–274). The fact is that children acquiring control constructions are puzzled by *promise* (building on Courtenay 1998, we indicate that adults are too) and acquire the pattern in (17) extremely late in linguistic development. The standard logic in generative grammar, whose goal is to account for how linguistic knowledge is acquired, is to relegate (17) to the periphery of the grammar, and certainly not to take it as a building block for theory construction. Landau notes that the pattern in (17) is ''far too systematic to be dismissed as 'highly marked' exceptions'' (p. 480) and claims that verbs of commitment and verbs of request for permission behave like *promise*. But this misses an important point: it does not matter that linguists can identify some systematicity, children acquiring language must do so. And it is a fact that children do not. Hence, we reiterate our claim that *promise* must be treated as exceptional. (For lack of space, we refer the reader to our (2003c:274) discussion regarding the exceptional treatment of *promise* we favor.)

3.5 Delimiting the Category of Nonobligatory Control

The MTC makes a distinction between obligatory control (OC) and nonobligatory control (NOC), the former licensed by movement, the latter in some other way. The distinction itself is a venerable one, going back to the earliest days of generative grammar when Equi NP Deletion was distinguished from Super Equi. It has long been recognized that OC and NOC PROs have very different properties (for review, see Hornstein 1999, 2001, 2003). What seems to have been less well appreciated is one problem that the distinction between these two kinds of control poses. The problem can be framed as a question: Why are OC and NOC PROs in complementary distribution? In other words, why is NOC impossible in cases like (19)?

(19) a. John wants PRO to leave.
      b. John saw Mary before PRO entering the room.

We *know* that these PROs are of the OC variety by their behavior (they require local, c-commanding antecedents, they resist split antecedents, etc.; see Hornstein 1999, 2001, 2003, and Boeckx and Hornstein 2003c for discussion). The question is not *what* they are but why they cannot be NOC PROs. What, in short, is wrong with configurations like (20a–b), if we assume (following Hornstein (2001), among others) that NOC PRO is a kind of null pronominal?

(20) a. John wants pro to leave.
      b. John saw Mary before pro entering the room.

The answer offered in Hornstein 2001 is that pronouns are last resort expressions that cannot be

used if the derivation can converge without their use. More specifically, if OC PROs are ''traces'' of A-movement, then pro can be used just in case the requisite movement is forbidden.

One pleasant feature of this approach to NOC PRO is the consequence that islands should only be able to host NOC PROs. The reasoning is straightforward: OC PROs are creatures of movement; islands bar movement; thus, a PRO within an island, as in (21), cannot be the residue of movement and so must be an NOC PRO (i.e., pro). Note that in (21) the PRO is inside a subject island. Thus, the relation between *John* and PRO cannot be one of movement and so this PRO must be an NOC PRO.

(21) John said that [PRO washing himself] was not worthwhile.

Several points are worth observing here. First, any theory of control should explain (not merely list) how OC PROs are distinguished from NOC PROs. Second, this theory should explain why the two are in complementary distribution. The MTC provides the beginnings of a principled distinction between the two, as exemplified above, and explains why NOC only obtains when OC does not. And third, if NOC PROs are pronominal, then the class of NOC PROs should be as varied as the class of pronominal expressions. In other words, though there may be a principled distinction between OC and NOC PRO, the range of possible NOC PROs may well be quite motley. With this in mind, we turn now to the discussion in Landau 2003.

Landau claims that the approach to NOC outlined in Hornstein 1999 misclassifies cases of OC and NOC in two ways. First, it cannot account for the fact that examples like (22a–d) are cases of NOC.[19]

(22) a. [After PRO_{arb} pitching the tents], darkness fell quickly.
   b. $Mary_1$ was baffled. [Even after PRO_1 revealing her innermost feelings], John remained untouched.
   c. [PRO_1 having just arrived in town], the main hotel seemed to $Bill_1$ to be the best place to stay.
   d. *[PRO_1 having just arrived in town], the main hotel collapsed on $Bill_1$.

The problem is purported to be that Hornstein's (1999) account would have to treat (22a–d) in terms of sideward movement. Hence, Landau claims the PROs in these examples would have to be classified as OC PROs, contrary to fact.

Contrary to Landau's assertions, we fail to see why Hornstein's (1999, 2001) account would have to treat these cases in terms of sideward movement and so classify these PROs as OC PROs. In fact, what is clear is that for Hornstein (2001), the PRO in (22a) would appear to have no potential antecedent in the sentence and so should be an NOC PRO, whereas the relation between PRO and *Mary* in (22b) could not be one of movement, so the PRO would have to be an NOC PRO *with the indicated indexation.*

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[19] The examples are from Landau 2003 (his (21a–b, d–e)). Landau refers only to Hornstein 1999. The approach lightly limned there is elaborated in far more detail in Hornstein 2001. We refer the interested reader to both sources.

A few descriptive points are worth making here. Pace Landau, it is quite difficult to get a PRO$_{arb}$ reading in (22a). This is not surprising given that the corresponding reading with *one* is very forced.

(23) *After one's pitching the tents, darkness fell quickly.

The reading one does get in (22a) has the PRO referring to the speaker(s) as in (24a) or to a previously identified person in the discourse as in (24b).

(24) a. After my/our pitching the tents, darkness fell quickly.
      b. John got a good night's sleep. After his pitching the tents, darkness fell quickly.

This fits if NOC PROs are pronominal. One will expect the PRO reading to track those with overt pronouns. As the impersonal reading with *one* (the so-called arb reading) is not easy to get when *one* is overt in (22a), it should not be easy to get if phonetically null. And, as both (24a–b) are fine with overt pronouns, (22a) should be fine with a similar interpretation for PRO. In contrast to (22a), (25) easily carries an impersonal reading, both with PRO and with an overt *one*.

(25) After PRO/one's pitching one's tent, sleep comes quickly.

Note also that contrary to Landau (2003:482) the left edge/right edge position of the adjuncts seems irrelevant in (22a). Both are acceptable. This contrasts with (22b), where the position of the adjunct does seem to matter. (26) is far less felicitous than (22b).

(26) *Mary$_1$ was baffled. John remained untouched even after PRO$_1$ revealing her innermost feelings.

Interestingly, even when we add an overt pronoun, the example does not dramatically improve. Contrast (27a) and (27b).

(27) a. ?*Mary$_1$ was baffled. John remained untouched even after her$_1$ revealing her innermost feelings.
      b.   Mary$_1$ was baffled. Even after her$_1$ revealing her innermost feelings, John remained untouched.

It is not clear why cases like (27a) are rather unacceptable, in contrast to cases like (27b).[20] However, the acceptability of the latter suffices to explain why the analogous sentence with NOC PRO is also fine.

What of (22c–d)? They raise slightly different questions as far as the MTC is concerned. In (22c), PRO may well have a sideward movement derivation and so have a possible OC PRO interpretation. In (22d), PRO cannot be derived via sideward movement and so it should be pronominal.[21] There is an interesting disanalogy between the two cases that comes to light when

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[20] It would be nice to know why cases like (27a) are poor, as it might help explain why examples like (26) are unacceptable. But we have nothing to offer at this point.

[21] Because of space constraints, we do not show this here; for details, see Kiguchi 2002 and Hornstein and Kiguchi 2001.

quantificational antecedents to PRO are considered. Structures similar to (22c) allow these, while those similar to (22d) do not. The former PROs act like gates with respect to weak crossover, while the latter do not. This follows if the former can be OC PROs while the latter cannot.[22]

(28) a. PRO_1 having just made his_1 way to town, the main hotel seemed to $everyone_1$ the best place to stay.

b. *PRO_1 having just made his_1 way to his_1 room, the awning collapsed on $everyone_1$.

If so, the two PROs in (22c–d) are quite different, the relation between PRO and *Bill* in (22c) being an instance of OC and the one in (22d) an instance of NOC.[23] Why is the indicated coindexing unacceptable in (22d)? We are not sure. However, it seems relevant that analogous sentences with overt pronouns, indexed as in (29), are also judged very odd.

(29) ??[Him_1 having just arrived in town], the main hotel collapsed on $Bill_1$.

If the PRO here is of the pronominal NOC variety, then the oddity of (29) should lead us to expect (22d) to be odd as well.

The second misclassification Landau (2003) highlights is also easy to counter. Landau observes that cases of OC like (30) are problematic.

(30) John said that Mary asked [how ⟨Mary⟩ to feed herself].

He notes that previous research had identified cases of control into embedded questions as instances of NOC, but he argues that such cases are in fact instances of OC. Let's say that he is correct. We are not sure how the ''standard'' theory handles (30), but we will not pursue the matter here. Instead, let us focus on the logic. According to Landau, the MTC is untenable since in order to get an OC interpretation, movement is required out of an island (embedded interrogative complements). Landau does not mention that embedded interrogative infinitives are very weak islands (31). In fact, Ross (1967) took embedded interrogative infinitives to suggest that what came to be known as the *wh*-island did not exist. As is well known, embedded interrogative complements are very porous.

(31) (?)What did John say how to cook?

But even if we were to agree that embedded interrogative infinitives are islands, the standard view would treat them as Relativized Minimality islands—that is, islands blocking movement of

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[22] Kiguchi (2002) and Hornstein and Kiguchi (2001) argue that the PROs that act as gates in the sense of Higginbotham 1980 are the OC PROs formed via Move.

[23] Note the following contrast as well:

(i) $Himself_1$ having just arrived in town,
a. the main hotel seemed to $Bill_1$ to be the best place to stay.
b. *the main hotel collapsed on $Bill_1$.

The possibility of an overt reflexive in (ia) (but not (ib)) is what we expect if the former is a case of OC. See Hornstein 2001, Kayne 2002, Grohmann 2003, Lidz and Idsardi 1997, and Zwart 2002 for movement approaches to (local) reflexives.

wh-type (Ā) elements. Since control is an instance of A-movement under the MTC, it is not clear that it would be affected.

3.6 The Interpretation of PRO in Nonobligatory Control

The sixth argument addresses the claim that the subject of the infinitive in NOC structures is a *pro* and thus receives pronominal interpretation. Landau (2003) notes that super-equi constructions like (32) indicate that the null subject in such (NOC) structures behaves like a logophor.

(32) Mary knew that [PRO perjuring himself/herself] disturbed John.

We will not question Landau's factual conclusion here. But we fail to see how it seriously affects the point at issue. For all we know, logophors are pronouns. They are what one might call special pronouns or ''pronouns + ''. So logophoricity is compatible with *pro*'s being the subject in NOC structures. We cannot offer more than that, because to the best of our knowledge no solid definition of logophor exists (see the attempts collected in Cole, Hermon, and Huang 2001). But surely it will involve some pronominal component, as Hornstein (2001) proposes.

4 Pseudoarguments

After presenting the arguments reviewed so far, Landau (2003) takes up three arguments that Hornstein (1999) offers in favor of the MTC. Landau argues that the first argument (based on reflexive verbs) rests on an incorrect analysis, that the second (based on *de se* interpretation) constitutes no improvement over other analyses, and that the third (based on *wanna*-contraction) rests on a narrow empirical fact. Therefore, none of them can be taken as independent arguments for the MTC.

We think that all of these conclusions are incorrect. However, space does not permit us to address each point fully. Here we focus on the discussion of *wanna*-contraction (see Hornstein 1999 and Boeckx 2000 for relevant discussion).

The MTC readily captures the well-known contrast in (33).

(33) a. Who do you want to/wanna see?
     b. Who do you want to/*wanna vanish?

We will not repeat the details of how a movement analysis provides a straightforward analysis of (33), since Landau grants the analysis. But he notes that the *wanna*-contraction facts are ''too idiosyncratic and peripheral to warrant any general conclusions'' (p. 484)—in other words, that ''this is a particular fact about the behavior of one or two verbs, in one language—hardly a solid basis for general conclusions'' (p. 487).

We could not disagree more with this conclusion. It is precisely the ''idiosyncratic'' nature of the phenomenon that makes it an ideal window into the structure of Universal Grammar (UG). Let's consider the logic carefully. Thornton (1990) has shown that young children have no problem mastering the *wanna*-contraction paradigm; it must therefore be deeply embedded inside UG, given that the phenomenon is manifested in a very small (idiosyncratic?) portion of the data

available to the child. If so, it is far from a ''peripheral'' phenomenon if one's interest lies in the structure of UG. Just like ECM and *do*-support, which are very limited and idiosyncratic to English, *wanna*-contraction is an excellent datum for approaches that address Plato's problem. Curiously, *wanna* presents just the opposite of the argument cited above regarding *promise*. It is because children have trouble with *promise* that we can conclude that it is a peripheral and idiosyncratic fact (no matter how it looks to the linguist); and it is because children have no trouble with *wanna*-contraction that we are right to emphasize its importance for analyses of control (no matter how limited in distribution the facts appear to the linguist).[24]

5 Raising/Control (Non)arguments

Landau (2003) next points to some asymmetries between raising and control. The general conclusion is that since the difference between raising and control is minimized in the MTC, any dissimilarity that obtains between them argues against a movement approach to control.

Two points are in order. First, although the MTC minimizes the difference between raising and control (it reduces to the checking of an additional θ-role in the case of control), it does not eliminate it. That is, the MTC is a *movement* theory of control, not a *raising* theory of control. As a result, differences between raising and control are not unexpected.[25] Second, it is a theoretical virtue of the MTC that it leaves little room for how differences between raising and control arise. There is really only one source for such differences: the checking of an additional θ-role in the case of control. Under theories that do not relate raising and control (such as the ''standard'' theory in (3)), the sources for the raising/control asymmetries are virtually unpredictable.

5.1 Complementizers

The first raising/control argument has to do with the generalization in (34), which Landau (2003: 488) takes to be correct.

(34) Control complements may be introduced by complementizers; raising complements never are.

Landau claims that this generalization is mysterious under the MTC, which treats raising and control ''alike.''

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[24] Though this is not the place to debate these points more fully, we suspect that the disagreement between ourselves and Landau (2003) over these two cases reflects a deep divergence concerning the aims of linguistic theory and its setting in a wider psychological/biological context. For the sake of clarity, we make explicit here what we take the goal of linguistic theory to be: namely, to adumbrate the structure of the language acquisition device (viz., the initial state of UG), as stated in Chomsky 1965.

[25] The situation is parallel to what we find in, say, the Ā-domain. Ever since Chomsky (1977) unified comparatives, *wh*-movement, topicalization, and other constructions under the umbrella term *Ā-movement,* linguists have taken *wh*-movement and topicalization to be reflexes of the same basic operation, but no one claims that *wh*-movement and topicalization thereby behave exactly alike in all situations.

Let's grant that (34) holds universally.[26] We do not know how the ''standard'' theory in (3) predicts that (we know of some versions of (3) that would (e.g., PRO Theorem–based approaches to control), but we also know of some that do not (null Case approaches)). But setting that aside, it seems to us that there is a way in which the minimal difference between raising and control (the presence of an additional, matrix θ-role in control cases) is enough to make sense out of (34). Control predicates unambiguously θ-mark their clausal complements. It is not clear that raising verbs like *seem* do. Raising verbs in many languages are functional elements, being built on adverbs (like English *likely*). Suppose we say that all raising verbs are functional and lack θ-assigning abilities. If we combine this with Longobardi's (1994) influential idea that arguments need to be DPs to be θ-marked, and if CPs are somewhat like DPs, (34) follows immediately. Crucially, it does not affect the view that control is a species of movement.

There remains, of course, the case of ECM verbs, which are raising verbs and which θ-mark their clausal complements. But interestingly, in the case of ECM, a special step of complementizer deletion (perhaps underlying the markedness of the construction) has been argued to take place, so that at some stage of the derivation, ECM raising verbs really do take CP complements. As a matter of fact, Kayne (1984) ties the availability of ECM in English to the availability of a prepositional complementizer in the language, a connection reinforced by acquisitional evidence in Sugisaki 2002. If true, such results indicate that (34) is to be refined, in a way that is compatible with a movement theory of control.

5.2 Unaccusative Properties

5.2.1 En-*Cliticization*   The second raising/control argument rests on the following contrast (Landau 2003:490):

(35) a. Le directeur du   département espère être   accepté.
        the head   of-the department hopes to-be accepted
        'The head of the department hopes to be accepted.'

    b. *Le directeur espère *en*   être   accepté.
        the head   hopes of-it to-be accepted

(36) a. Le directeur du   département semble être   accepté.
        the head   of-the department seems to-be accepted
        'The head of the department seems to be accepted.'

    b. Le directeur semble *en*   être   accepté.
        the head   seems of-it to-be accepted

From (35b) and (36b), it appears that French allows stranding of partitive *en* in raising, but not in control. Landau (2003) takes this to indicate that control cannot be movement. Let us first note

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[26] But see Marušič 2003 for important discussion. Randall Hendrick (personal communication) notes that in Tongan, raising complements are headed by overt complementizers. For other instances of raising across complementizers, see Ura 1996 on ''hyperraising'' and ''superraising.''

that the facts in (35b) and (36b) are not robust across speakers of French (a fact Landau acknowledges). But even if we grant the contrast, it is not clear why it argues against the MTC. All we need to do is adopt Pollock's (1998) claim that *en* is a clitic heading a big DP, taking its NP associate as a complement. Since *en* is the head, it is the element that is θ-marked. Hence, it could not be stranded in the embedded clause in (35b) under the MTC, as it would have to move to obtain an additional θ-role. No such requirement holds in raising (assume that *en* is inherently Case-marked, hence cannot head the whole DP when the latter moves to a structural Case position like Spec,TP; so stranding is forced in this case).

5.2.2 Si-*Reflexivization*   The third raising/control argument builds on the asymmetry noted by Rizzi (1986b) for Italian (Landau 2003:490).[27]

(37) a. *I   due candidati si   risultavano poter   vicere.
         the two candidates to-each-other appeared   to-be-able to-win
         ('The two candidates appeared to each other to be able to win.')
     b. I   due concorrenti si   sono promessi di essere leali.
         the two competitors to-each-other were promised DI to-be loyal
         'The two competitors promised to each other to be loyal.'

Rizzi's basic insight is that an element cannot raise across a coindexed element like a clitic. This accounts for (37a). But by the same reasoning, (37b) should be excluded if control is movement.

Again, there are reasons to believe that the problem in (37) is independent of the idea that control is movement. Note first that Rizzi's idea undergenerates once we adopt the VP-internal subject hypothesis, as it appears to block any movement of the subject from Spec,VP to Spec,TP across a reflexive clitic in simple clauses. So the problem lies with raising in (37a), not with raising in (37b). A simple way to account for the contrast in (37) under the MTC is to say that by having to move into the matrix VP to obtain an additional θ-role, the subject and the reflexive become equidistant for purposes of locality. By contrast, in (37a) the embedded subject is not part of the same domain as the reflexive before movement to Spec,TP. Hence, raising is blocked by some version of (''indexical'') minimality. We confess that this is just a sketch of an analysis, but note that Landau offers no analysis of (37). (It is not enough to appeal to Rizzi 1986b, since we know that that idea cannot be right; see McGinnis 1998 for extensive discussion.) Facts (contrasts like (37)) in and of themselves do not speak in favor of one or the other hypothesis. Only facts accompanied by a theory do.

5.3 Each-*Association*

The fourth raising/control argument rests on the contrast in (38) (Landau 2003:491).

(38) a. One interpreter each seemed to have been assigned to the visiting diplomats.
     b. *One interpreter each tried to be assigned to the visiting diplomats.

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[27] As far as we know, the Italian facts do not hold systematically in all Romance languages. As a result, we may not want too deep an explanation for (37).

Again, Landau (2003) merely points out the contrast and claims that it invalidates the MTC. Interestingly enough, we have independently analyzed the binominal *each* construction (see Boeckx and Hornstein 2003b, adapting ideas proposed in Safir and Stowell 1987). We will not replicate and justify the analysis here. Suffice it to say that we treat *XP each* as pied-piping a copy of the plural associate NP (*diplomats*) and that the presence of this copy prevents the *XP each* phrase from remerging into a θ-position associated with a predicate distinct from the one the plural associate NP merges with. This effectively rules out (38b).

5.4 Case Concord

We have also independently analyzed control in Icelandic (Boeckx and Hornstein 2003a), which constitutes the fifth raising/control argument in Landau 2003. The argument rests on data from Icelandic discussed in detail in Sigurðsson 1991. Icelandic has a morphologically rich Case/agreement system, and some of that richness shows up in control structures. Because secondary predicates and floating quantifiers overtly display the Case of the DPs they agree with, Sigurðsson uses them to test two questions (call this the *concord test*): (a) Is PRO Case-marked in Icelandic? and (b) If so, which Case does it bear?

   Sentences (39) and (40) show that by the concord test, PRO in Icelandic can bear either structural or quirky Case.

(39) Jón   bað   Bjarna   að koma einan.
     Jon.NOM asked Bjarni.ACC to come alone.ACC
     'Jon asked Bjarni to come alone.'

(40) Jón   bað   Bjarna   að leiðast   ekki einum.
     Jon.NOM asked Bjarni.ACC to be-bored not   alone.DAT
     'Jon asked Bjarni not to be bored alone.'

Landau argues that since Icelandic shows that PRO is Case-marked and can bear a Case distinct from that of the controller, PRO must exist independently of the controller. It cannot simply be a copy of the controller as would be required in a movement approach to control. In short, the observation that PRO can bear a Case distinct from that of its antecedent is taken to argue *against* a movement approach to control and to argue *in favor of* a PRO-based account. However, we have shown in Boeckx and Hornstein 2003a that this argument does not undermine the movement approach when the facts are considered in their entirety. In fact, just the opposite. The facts divide into two basic groups: instances of quirky Case concord and instances of structural Case concord. The former require some theoretical adjustments with regard to multiply Case-marked DPs in order to be incorporated into any account of control, be it a movement account or any other (PRO Theorem, null Case, Agree, etc.). The latter are very easily integrated into a movement approach but are quite surprising given a PRO-based theory. All in all, then, careful consideration of the control facts in Icelandic strengthens, rather than refutes, the movement approach to control. (For the argument in its entirety, see Boeckx and Hornstein 2003a.)

5.5 Partial Control

Landau's (2003) final argument is that languages like English appear to license what he dubs ''partial control'' (see Landau 1999, 2001), which in essence means that the referent of PRO appears to include the antecedent plus other individuals. Raising and simple predicates do not appear to license that reading.

(41) a.   John preferred [to meet at six].
      b.   *John seems to meet at six.
      c.   *John met at six.

Landau concludes from this that PRO must exist independently of its antecedent. Hornstein (2003: 37ff.) develops a technical account of partial control that is consistent with a movement analysis. (Basically, Hornstein claims that the ''extra individuals/plural'' reading arises as an instance of covert adjunction of a comitative; for a related proposal, see Barrie 2003.) So the facts are compatible with a movement theory of control. But here we would like to make a slightly different point. It is not clear to us that ''partial control'' readings are illicit in raising/simple clausal contexts. Consider the following passage:

(42)   John is a really busy professor. His days are filled with meetings, with students, deans, colleagues, lunch appointments, etc. Can you imagine?! Yesterday John met at 8 a.m., 9 a.m., 10 a.m., noon, and 7 p.m. His wife told me, ''John seems to be meeting all the time!''

This monologue is an acceptable piece of English. We are inclined to think that it is a special lexical property of *meet* and a handful of other verbs that allows them to give rise to a partial control reading. From (42), it certainly does not seem to be a special property of control. If so, no PRO or special mechanism is required to account for ''partial control.''

6 Conclusion

We have examined Landau's (2003) empirical arguments against a movement theory of control and have shown that they do not stand up to closer scrutiny. Thus, we conclude that the ''standard'' theory enjoys no obvious empirical advantages over the MTC. In fact, we believe the opposite to be the case. Though we did not discuss them here, there are several phenomena that we believe are very hard, if not impossible, to account for in ''standard'' PRO-based approaches to control.[28] These empirical advantages nicely complement the MTC's evident conceptual and methodological advantages. Thus, Landau 2003 in fact lends additional empirical credibility to the MTC.

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[28] In fact, we think that some of the arguments Landau raises, such as the Case concord facts from Icelandic, are precisely of that nature. There are many others. For instance, cases of backward control, of the type analyzed in Tsez (Polinsky and Potsdam 2002), Korean (Monahan 2003), Romanian (Alboiu 2003), and so on, or the Greek facts from Kapetangianni and Seely 2003, constitute a strong empirical case for a movement analysis (see Hornstein 2003 and Boeckx and Hornstein, in preparation, for discussion).

References

Alboiu, Gabriela. 2003. The ups and downs of obligatory control. Paper presented at the 1st North-American Syntax Conference, Concordia University, May 2003.

Barrie, Michael. 2003. Moving toward partial control. Paper presented at NELS 34, SUNY Stony Brook.

Boeckx, Cedric. 2000. A note on contraction. *Linguistic Inquiry* 31:357–366.

Boeckx, Cedric, and Norbert Hornstein. 2003a. Control in Icelandic. Ms., Harvard University, Cambridge, Mass., and University of Maryland, College Park.

Boeckx, Cedric, and Norbert Hornstein. 2003b. On eliminating D-Structure: The case of binominal *each*. Ms., Harvard University, Cambridge, Mass., and University of Maryland, College Park.

Boeckx, Cedric, and Norbert Hornstein. 2003c. Reply to ''Control is not movement.'' *Linguistic Inquiry* 34:269–280.

Boeckx, Cedric, and Norbert Hornstein. In preparation. Control and minimalism. Ms., Harvard University, Cambridge, Mass., and University of Maryland, College Park.

Borer, Hagit. 2003. Structuring sense. Ms., University of Southern California, Los Angeles.

Bošković, Željko. 1994. D-Structure, Theta-Criterion, and movement into theta-positions. *Linguistic Analysis* 24:247–286.

Bošković, Željko. 1995. Case properties of clauses and the Greed Principle. *Studia Linguistica* 49:32–53.

Bošković, Željko, and Daiko Takahashi. 1998. Scrambling and Last Resort. *Linguistic Inquiry* 29:347–366.

Bowers, John. 1973. Grammatical relations. Doctoral dissertation, MIT, Cambridge, Mass.

Chomsky, Noam. 1965. *Aspects of the theory of syntax.* Cambridge, Mass.: MIT Press.

Chomsky, Noam. 1977. On *wh*-movement. In *Formal syntax,* ed. by Peter Culicover, Thomas Wasow, and Adrian Akmajian, 71–132. New York: Academic Press.

Chomsky, Noam. 1993. A minimalist program for linguistic theory. In *The view from Building 20: Essays in linguistics in honor of Sylvain Bromberger,* ed. by Kenneth Hale and Samuel Jay Keyser, 1–52. Cambridge, Mass.: MIT Press.

Chomsky, Noam. 1995. *The Minimalist Program.* Cambridge, Mass.: MIT Press.

Chomsky, Noam. 2000. Minimalist inquiries: The framework. In *Step by step: Essays on minimalist syntax in honor of Howard Lasnik,* ed. by Roger Martin, David Michaels, and Juan Uriagereka, 89–155. Cambridge, Mass.: MIT Press.

Chomsky, Noam. 2001. Derivation by phase. In *Ken Hale: A life in language,* ed. by Michael Kenstowicz, 1–52. Cambridge, Mass.: MIT Press.

Cole, Peter, Gabriella Hermon, and C.-T. James Huang, eds. 2001. *Long-distance reflexives.* Syntax and Semantics 33. San Diego, Calif.: Academic Press.

Courtenay, Karen. 1998. Summary: Subject control verb PROMISE in English. http://linguist.org/issues/9/9-651.html.

Culicover, Peter, and Ray Jackendoff. 2001. Control is not movement. *Linguistic Inquiry* 32:493–512.

Dowty, David. 1989. On the semantic content of the notion of ''*thematic role.*'' In *Properties, types, and meanings,* vol. 2, ed. by Gennaro Chierchia, Barbara Partee, and Raymond Turner, 69–129. Dordrecht: Kluwer.

Grimshaw, Jane. 1990. *Argument structure.* Cambridge, Mass.: MIT Press.

Grohmann, Kleanthes K. 2003. *Prolific domains.* Amsterdam: John Benjamins.

Higginbotham, James. 1980. Pronouns and bound variables. *Linguistic Inquiry* 11:679–708.

Hornstein, Norbert. 1999. Movement and control. *Linguistic Inquiry* 30:69–96.

Hornstein, Norbert. 2001. *Move! A minimalist theory of construal.* Oxford: Blackwell.

Hornstein, Norbert. 2003. On control. In *Minimalist syntax,* ed. by Randall Hendrick, 6–81. Oxford: Blackwell.

Hornstein, Norbert, and Hirohisa Kiguchi. 2001. PRO-gate and movement. Ms., University of Maryland, College Park.

Hornstein, Norbert, and Mituse Motomura. 2002. Psych verbs, theta roles and reconstruction. In *Proceedings of 2002 LSK International Summer Conference,* vol. 2, 39–58. Seoul: Linguistic Society of Korea.

Hornstein, Norbert, Sara Rosen, and Juan Uriagereka. 1996. Integral predication. In *Proceedings of the 14th West Coast Conference on Formal Linguistics,* ed. by José Camacho, Lina Choueiri, and Maki Watanabe, 169–183. Stanford, Calif.: CSLI Publications.

Kapetangianni, Konstantia, and Daniel Seely. 2003. Control in Greek: Explanation by deduction, agreement by degrees. Ms., University of Michigan, Ann Arbor, and Eastern Michigan University, Ypsilanti.

Kayne, Richard. 1984. *Connectedness and binary branching.* Dordrecht: Foris.

Kayne, Richard. 1994. *The antisymmetry of syntax.* Cambridge, Mass.: MIT Press.

Kayne, Richard. 2002. Pronouns and their antecedents. In *Derivation and explanation in the Minimalist Program,* ed. by Samuel David Epstein and T. Daniel Seely, 133–166. Oxford: Blackwell.

Kiguchi, Hirohisa. 2002. Syntax unchained. Doctoral dissertation, University of Maryland, College Park.

Koster, Jan. 1978. Why subject sentences don't exist. In *Recent transformational studies in European languages,* ed. by Samuel Jay Keyser, 53–64. Cambridge, Mass.: MIT Press.

Landau, Idan. 1999. Elements of control. Doctoral dissertation, MIT, Cambridge, Mass.

Landau, Idan. 2001. *Elements of control.* Dordrecht: Kluwer.

Landau, Idan. 2003. Movement out of control. *Linguistic Inquiry* 34:471–498.

Lasnik, Howard. 1988. Subjects and the Theta-Criterion. *Natural Language & Linguistic Theory* 6:1–17.

Legate, Julie Anne. 2003. Some interface properties of the phase. *Linguistic Inquiry* 34:506–516.

Lidz, Jeffrey, and William Idsardi. 1997. Chains and phono-logical form. In *Proceedings of the 22nd Annual Penn Linguistics Colloquium,* ed. by Alexis Dimitriadis, Hikyoung Lee, Christine Moisset, and Alexander Williams, 109–126. University of Pennsylvania Working Papers in Linguistics 5.1. Philadelphia: University of Pennsylvania, Penn Linguistics Club.

Lin, Jonah. 2001. Light verb syntax and the theory of grammar. Doctoral dissertation, University of California, Irvine.

Longobardi, Giuseppe. 1994. Reference and proper names: A theory of N-movement in syntax and Logical Form. *Linguistic Inquiry* 25:609–665.

Manzini, Maria Rita, and Anna Roussou. 2000. A minimalist theory of A-movement and control. *Lingua* 110:409–447.

Martin, Roger. 1996. A minimalist theory of PRO and control. Doctoral dissertation, University of Connecticut, Storrs.

Marušič, Franc. 2003. CP under control. Paper presented at Formal Description of Slavic Languages 6, Leipzig.

McGinnis, Martha. 1998. Locality in A-movement. Doctoral dissertation, MIT, Cambridge, Mass.

Monahan, Philip. 2003. Backward control in Korean. Paper presented at the 22nd West Coast Conference on Formal Linguistics, University of California, San Diego.

Nunes, Jairo. 2001. Sideward movement. *Linguistic Inquiry* 32:303–344.

O'Neil, John. 1997. Means of control. Doctoral dissertation, Harvard University, Cambridge, Mass.

Ozturk, Balkiz. 2004. Case, referentiality, and phrase structure. Doctoral dissertation, Harvard University, Cambridge, Mass.

Polinsky, Maria, and Eric Potsdam. 2002. Backward control. *Linguistic Inquiry* 33:240–282.

Pollock, Jean-Yves. 1998. On the syntax of subnominal clitics: Cliticization and ellipsis. *Syntax* 1:300–330.

Ramchand, Gillian. 2002. First phase syntax. Ms., Oxford University.

Rizzi, Luigi. 1986a. Null objects in Italian and the theory of *pro. Linguistic Inquiry* 17:501–557.

Rizzi, Luigi. 1986b. On chain formation. In *The syntax of pronominal clitics,* ed. by Hagit Borer, 65–95. New York: Academic Press.

Rodrigues, Cilene. 2003. Thematic chains. Ms., University of Maryland, College Park.

Rosenbaum, Peter S. 1967. *The grammar of English predicate complement constructions.* Cambridge, Mass.: MIT Press.

Ross, John Robert. 1967. Constraints on variables in syntax. Doctoral dissertation, MIT, Cambridge, Mass.

Safir, Ken. 1999. Vehicle change and reconstruction in Ā-chains. *Linguistic Inquiry* 30:587–620.

Safir, Ken, and Tim Stowell. 1987. Binominal *each.* In *Proceedings of NELS 18,* vol. 2, ed. by James Blevins and Juli Carter, 426–450. Amherst: University of Massachusetts, GLSA.

Sigurðsson, Halldór Ármann. 1991. Icelandic Case-marked PRO and the licensing of lexical arguments. *Natural Language & Linguistic Theory* 9:327–363.

Sugisaki, Koji. 2002. Innate constraints on language variation: Evidence from child language. Doctoral dissertation, University of Connecticut, Storrs.

Szabolcsi, Anna. 1983. The possessor that ran away from home. *The Linguistic Review* 3:89–102.

Tellier, Christine. 1990. Underived nominals and the Projection Principle: Inherent possessors. In *Proceedings of NELS 20,* vol. 2, ed. by Juli Carter, Rose-Marie Déchaine, Bill Philip, and Tim Sherer, 472–486. Amherst: University of Massachusetts, GLSA.

Thornton, Rosalind. 1990. Adventures in long-distance moving. Doctoral dissertation, University of Connecticut, Storrs.

Ura, Hiroyuki. 1996. Multiple feature checking: A theory of grammatical function splitting. Doctoral dissertation, Cambridge, Mass.

Zwart, C. Jan-Wouter. 2002. Issues relating to a derivational theory of binding. In *Derivation and explanation in the Minimalist Program,* ed. by Samuel David Epstein and T. Daniel Seely, 269–304. Oxford: Blackwell.

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