

Movement-Resistant Aspects of Control

Idan Landau

Ben Gurion University, August 2004

Abstract

This paper examines the solutions offered by Hornstein (2003) and Boeckx & Hornstein (2004) to the empirical challenges raised for the theory that Obligatory Control (OC) is a species of raising. The challenges include violations of the Minimal Distance Principle (MDP), partial control, OC into finite complements and into *wh*-infinitives, case-marking of PRO, PRO-gate effects, passive of subject-control verbs and sideward movement from OC complements. It is shown that none of the proposed solutions is adequate, and a superior account is generally available within an Agree-based approach to OC (Landau 2000, 2004). Furthermore, Hornstein's objections to that approach – in particular, to its treatment of partial control – are shown to be groundless, resting on misunderstandings of data or analysis.

1. Introduction

This paper is a contribution to the ongoing debate about the nature of Obligatory Control (OC) in recent syntactic theorizing. Although the debate has seen many participants and approaches, I will focus on two opposing views in particular: My own view, represented in Landau (1999, 2000, 2003, 2004), and the view represented in Hornstein (1999, 2001, 2003) and Boeckx & Hornstein (2004). On my view, OC is formed by an abstract Agree relation, mediated by functional heads. On Hornstein's view, OC is formed by A-movement, an instance of raising.

In the previous stage of the debate (Landau 2003), I provided numerous independent arguments to the effect that the theory developed by Hornstein (1999) fails to account for the most significant generalizations about OC and Non-Obligatory Control (NOC). In response, Hornstein (2003) addresses a variety of empirical problems and offers novel, sometimes ingenious, analyses for them. He also criticizes key aspects of Landau's (1999) proposal. Boeckx & Hornstein (2004), in turn, claim that most of Landau's (2003) arguments against the movement theory of control do not stand up to closer scrutiny.

These are welcome developments; as the debate proceeds, theoretical positions are continually sharpened, bringing to light more and more empirical consequences.

The purpose of the present paper is threefold. First, I will examine Hornstein's (2003) treatment of various empirical challenges to his theory and evaluate how well his solutions meet these challenges. Second, I will clarify, and defend, certain aspects of Landau (1999) that are criticized (and misdescribed) by Hornstein. Third, I will show why Boeckx & Hornstein's (2004) reply does not really address the essence of the critique expressed in Landau (2003). The general organization of my replies will more or less follow Hornstein's (2003) discussion, and then turn to some issues exclusively treated by Boeckx & Hornstein's (2004).

One cautionary note to the reader. As this debate has already some history to it, it becomes increasingly cumbersome to elaborate this history on each new installment. I therefore chose to omit much material – both in data and in arguments – that was well-represented in the previous stages of the debate. The discussion to follow presupposes some familiarity with the relevant literature and the published views of all parties. It is my hope that the arguments below will be appreciated and judged against the background of this knowledge.

2. The Minimal Distance Principle (MDP)

An immediate consequence of reducing OC to movement is the emergence of locality constraints. In particular, a very strict constraint – the MLC – which restricts A-movement, is now taken to restrict OC. Hornstein (1999) claimed this to be the source of the observation that most transitive control verbs display object control (the MDP). The famous case of *promise* is treated as a marked exception.

Elsewhere I have written extensively about the serious shortcomings of the MDP (Landau 1999/2000, Ch. 5; 2003). Most notably, the strictly configurational character of the MLC makes it look like an accident that the same semantic verb classes, again and again across languages, “violate” the MDP. I will not repeat those arguments here (see Jackendoff & Culicover 2003 for pertinent discussion). Instead, I will address some novel points made in Hornstein (2003).

As evidence for the workings of the MDP/MLC, Hornstein mentions (ex. 50) Rosenbaum’s observation that object control verbs, when optionally lacking an object, shift to subject control: “...when the object is not generated (at least in overt syntax), the subject can (and must) be the controller” (p. 29).

- (1) a. John₁ asked/begged/got Mary₂ PRO_{*1/2} to leave.
- b. John₁ asked/begged/got PRO₁ to leave.

This description, I would argue, is misleadingly selective. In fact, many verbs *retain* their object control interpretation even in the absence of an overt object. English exhibits this pattern with communication verbs, other languages (like Hebrew below) extend it to many other instances.

- (2) a. Mary₁ said/shouted to John₂ PRO_{*1/2} to leave.
- b. Mary₁ said/shouted PRO_{*1} to leave.

- (3) a. Gil₁ hirša le-Rina₂ PRO_{*1/2} la’azov.
 Gil permitted to-Rina PRO to-leave
 ‘Gil₁ permitted Rina₂ PRO_{*1/2} to leave’

- b. Gil₁ hirša PRO*₁ la'azov.
 Gil permitted PRO to-leave
 'Gil₁ permitted PRO*₁ to leave'

Consider now how an MLC-approach might account for the difference between *ask* and *say*. It could be argued that the null object of *ask* is not syntactically represented, whereas that of *say* is present in the syntax, i.e., a small *pro*. The latter then imposes object control but the former, being invisible to the MLC, does not. This account, again, does not speak to the obvious question, which is *why* things are this way and not any other way. It also approaches circularity: there is no *independent* evidence for the presence/absence of a *pro* object, other than the absence/presence of control shift.¹ Internally to Hornstein's theory, it is also not clear how English could license the Last Resort element *pro* in contexts that risk no grammatical violation.

By contrast, if controller choice is assigned to lexical and contextual factors to begin with, the finding that different verbs show different patterns of control shift is anything but surprising. Under this view, neither *ask* nor *say* license a *pro* object; the difference is located in their fine-grained lexical structure.

How is this view more explanatory than the MLC view? First, it opens up the way for a close investigation of the lexical semantics involved in such matters. I will not attempt here any elaborate investigation, but simply sketch the outlines of one possible approach. Notice that (1b) has the following paraphrase, with the italicized material tacitly "filled in".

- (4) John₁ asked *X* for [*X's permission to him₁ PRO₁ to leave*].

In this semantic representation, *X* controls the external argument of *permission* and *John* the internal one. *Permission* being an object control predicate, PRO gets to be controlled by the implicit *him*, identical to the matrix subject.

What would be the analogue for (2b)? Conceivably, the following paraphrase.

- (5) Mary₁ said to *X* [*her₁ instruction to X*₁ PRO*₁ to leave*].

¹ See Landau (2003) for similar comments on Hornstein's (1999) treatment of the distinction between reflexive verbs (*wash*) and non-reflexive verbs (*hit*).

Here, the understood noun, recovered from the meaning of *say*, is *instruction* (or perhaps *order*). Interestingly, the external argument of this noun is controlled by the matrix subject *Mary*, not by the matrix implicit goal *X*, which controls the internal argument of *instruction*. For this reason, the latter cannot also be coindexed with *Mary* or else condition B will be violated. Since the internal argument of the noun controls PRO, we obtain the result that PRO must be disjoint from the matrix subject.

I do not consider the above reasoning to be anything more than intuition spelled out loud. A formal account of these data must go much beyond such intuitions.² Nevertheless, the purpose of this exercise is simply to draw attention to the kind of considerations that are likely to provide fruitful insights in the study of control shift. As far as I can see, the MLC approach is rather limited in this domain. Notice that the even if implicit arguments are *pro*-like elements (but see the next point), the MLC fails to explain their effect on control. Whereas (4) conforms to the MLC, (5) does not – *Mary* controls the NP subject across the matrix goal.

Boeckx & Hornstein (2004) concede, indeed, that implicit control reduces, in their theory, to movement of a null *pro*. The alternative, they maintain, "would force us to say that lexical control exists along with syntactic control... deciding which control takes over when is no trivial matter, and certainly weakens the theory... so our account leads to a more restrictive theory" (p. 439). The comment reveals an alarming methodology, willing to sacrifice well-known facts on the altar of "restrictiveness". It *is* a fact that control shift is lexically governed; it *is* a fact that the control properties of *ask* and *say* differ when their object is unexpressed ("no trivial matter", indeed, hence, one would think, not a matter to be ignored); and it *is* a fact that a uniform *pro*-analysis of implicit control fails to explain this contrast. All these facts, apparently, do not suffice to compromise the a priori imperative of restrictiveness.

The second aspect in which the lexical/contextual account of control shift is superior to the MLC is more straightforward. Recall that Hornstein must posit a *pro* object in (2b) to explain the blocking of subject control. Null pronouns, unlike implicit arguments, are visible to condition A; in particular, they can bind anaphors (Rizzi 1986). We may ask whether the null object of *say* exhibits this property. The answer is no; unlike an overt pronoun, the null goal argument of *say* cannot bind an anaphor.

- (6) a. We all heard the amazing story about [Bill and Kevin]₁.
John said to them₁ at each other's₁ parties to take off their clothes.

² For several attempts in this spirit, see Farkas (1988), Sag & Pollard (1991) and Petter (1998).

- b. We all heard the amazing story about [Bill and Kevin]₁.
 * John said *to pro*_i at each other's₁ parties to take off their clothes.

This contrast casts further doubt on the idea that object control in (2b)/(3) is mediated via a syntactic object *pro*. But then, if something *other* than the MLC explains object control in (2b)/(3) (in fact, the MLC wrongly predicts subject control), that something might as well explain object control in the canonical examples (1a)/(2a). Both canonical and shifted control have little use for the MLC.

Boeckx & Hornstein (2004) argue that Rizzi's binding/control asymmetry can be explained without sacrificing the assumption that implicit control is mediated by *pro*. Following Hornstein (2001), they assume that binding itself is subsumed under movement, the anaphor being a "lexicalized copy" of its antecedent. To explain why *pro* can control but not bind, they propose the following condition: "An anaphor cannot be lexicalized if its antecedent is not" (p. 439). Hence, a null element (like *pro*) can antecede other null elements (unpronounced subject copies in OC) but not lexical ones (reflexives in binding).

Unfortunately, the proposed condition is massively disconfirmed in NOC environments, where PRO (or *pro*, according to B&H) happily binds a reflexive. Note that PRO in (7a,b) has no antecedent in its clause.

- (7) a. [PRO praising oneself/myself] wouldn't be polite.
 b. John was furious. [PRO to get himself a new CD-player like the one stolen] would cost a fortune.

The combination of (6b) and (7) presents a paradox to B&H; conceivably, they might introduce a novel distinction between *pros* that can, and *pros* that cannot bind, restricting the latter to non-subject positions. This, however, would merely replicate the already-existing distinction between implicit arguments (available in both internal and external positions) and PRO (restricted to subject positions).³

More generally, it appears that that the postulated *pro* controller in implicit control constructions creates more problems than it solves: It does not block control shift where it

³ Notice that implicit arguments must exist on *anyone's* theory of syntax, including Hornstein's, which frugally reserves *pro* as a last resort device. Otherwise, *pros* will proliferate without limit (in the sentence *The letter made mother angry*, does *letter* contain a Goal *pro*, mother a "son/daughter" *pro*, and *angry* a Subject-Matter *pro*?). If so, a theory which posits just one type of *pro*, always visible to binding, is to be preferred over more complex alternatives.

should, and it does not bind reflexives where it could. Its motivation is purely theory-internal - to secure the claim that control is movement.

Finally, a word about Hornstein's appeal to "markedness" to explain the exceptional behavior of *promise*, *vow*, *commit*, etc. Hornstein emphasizes that this view makes sense of the late acquisition of such constructions by children, first documented in C. Chomsky (1969). This may be so. What is *not* warranted by the acquisition evidence is the following statement: "...any view that "regularizes" the properties of *promise*, for example, by claiming that it falls under a broader generalization in terms of which its behavior is grammatically impeccable, *cannot* account for why it is acquired late" (p. 34).

First, let me note that this reasoning is dubious on general grounds. There is no obvious entailment from late acquisition to markedness. A host of grammatical constructions emerge relatively late in acquisition – passive, purpose clauses, parasitic gaps, etc. – none of which is marked in the relevant sense (i.e., an exception to a UG principle). Markedness is but one, probably minor, factor in fixing the time course of grammatical development.

Second, one cannot seriously argue for a particular syntactic theory on the basis of an isolated developmental fact. The rich literature on the acquisition of control has uncovered many facts that do *not* follow from Hornstein's analysis. For example, there is a systematic delay in the acquisition of OC into adjuncts compared to the acquisition of OC into complements (McDaniel & Cairns 1990, McDaniel et. al. 1991, Cairns et. al. 1994). Applying Hornstein's logic, one could argue that this delay "cannot" be explained by any analysis that reduces both types of OC to the same mechanism (e.g., A-movement). Clearly, the conclusion is unwarranted; but so is Hornstein's conclusion w.r.t. potential non-markedness accounts of the *promise*-delay.

Other developmental facts are equally puzzling. Young children (3 year olds) err in control assignment with object control verbs as frequently as they succeed in subject control verbs. Thus, although incorrect object control assignment to *promise* persists to age 7 or so, *tell* and *remind* elicit considerable incorrect subject control at age 3, which later dies out. (Sherman & Lust 1993). This progression from more to less MDP-violations is anything but expected on the markedness theory. Finally, parallels between children's preferences for controllers of PRO and antecedents of pronouns in comparable structures suggest that *non*-movement generalizations are operative in this domain

(Cairns et. al. 1994). The place and relative weight of the *promise*-delay effect within this rich empirical array still waits to be determined.⁴

3. Split Control

Hornstein (1999) took the ban on split control to be an exceptionless criterion for OC. This was challenged in Landau (1999, 2000), but Hornstein (2003, fn. 13) reiterates his position, pointing to the controversial status of the English examples.

It is somewhat pointless to argue about data. There is little doubt that English is pretty resistant to split control in OC. Hornstein is right (I stand corrected) – most OC verbs do not allow split control. But some do, as (8a), from Koster & May (1982, ex. 96), shows. The German example (8b) is from Wurmbrand (2003) and the Hebrew example (8c) is my own. Note that the latter involves a verb of “strong” influence.

- (8) a. John proposed to Mary to help each other.
- b. Ich habe ihm angeboten einander zu helfen.
I have him offered each-other to help
'I offered him to help each other'
- c. Gil šixnea et Rina le'hacig et acmam bli buša.
Gil persuaded ACC. Rina to-present ACC. themselves without shame
'Gil persuaded Rina to present themselves without shame'

Even if some (or most) speakers reject split control in some (or most) contexts, the fact that the possibility is real for others is a cause for worry. In fact, the very variability observed is already a cause for worry for theories in which controller choice is rigidly syntactic. More seriously, it is virtually impossible to imagine how split control can arise through raising, given that two distinct DP chains cannot share their tail position. I do not

⁴ Hornstein proposes that the peculiarity of *promise* might reside in the presence of a null (dative) preposition introducing the goal argument. Thus, the goal DP does not c-command PRO, and the MLC selects the subject as the closest controller. I do not understand how this proposal is consistent with the well-known observation – acknowledged by Hornstein himself (ex. 69) – that prepositions are invisible to c-command relations. Nor is it clear how the proposal could distinguish minimal pairs like the following.

- i. Bill₁ committed to Jane₂ [PRO₁ to marry her₂/*him₁].
ii. Bill₁ appealed to Jane₂ [PRO₂ to marry him₁/*her₂].

claim that my own Agree-based analysis fares much better.⁵ Rather, my point is that split control is a problem for everybody, and it cannot be simply wished away.

4. Partial Control (PC)

Section 1.6.4 in Hornstein's paper is dedicated to a close critique of the analysis of OC I proposed in Landau (1999). Most of Hornstein's comments apply to the technical implementation of my analysis. Unfortunately for this debate, Hornstein based his criticism on Landau (1999), ignoring the revisions made in Landau (2000). The latter, in fact, resolve many of the issues raised. Even so, Hornstein misinterprets several aspects of Landau (1999), so some clarifications are in order.

4.1. Semantic Plurality

Consider the structure of a PC configuration (F = matrix T/v).

- (9) a. The chair₁ preferred [PRO₁₊ to gather in the conference room].
 b. [_{CP} DP .. F .. [_{CP} [T-Agr+C⁰_T] [_{TP} PRO [_{I'} t_{T-Agr} [_{VP} t_{PRO} ..]]]]]
- | | | | |
|---------------------------|---------------------------|-------------|---------------------------|
| | | | |
| <u>Agree</u> ₂ | <u>Agree</u> ₃ | <u>Move</u> | <u>Agree</u> ₁ |

Agree₁ establishes embedded agreement (followed by raising of PRO to [Spec,TP]) and Agree₂ matrix (subject or object) agreement. A PC infinitive is tensed, hence headed by C⁰_T with an uninterpretable Tense feature. The embedded T-Agr raises to check off this feature, thereby reaching an edge position in which it is visible to matrix operations. Agree₃ applies to establish the link generating OC.⁶

Crucially, all valuation of normal ϕ -features flows from the single non-anaphoric occurrence of these features – the controller DP. In Landau (2000) I assumed that the special feature [SP] (semantic plurality), on which DP and PRO may differ, is inherently

⁵ Still, one can imagine that the infinitival Agr is targeted by both the matrix v and T. This would no doubt be an exotic option, but one that does not run into tree-geometric contradictions, as the raising analysis appears to.

⁶Boeckx & Hornstein (2004, fn.1) remark that since Agree is implicated in Move, "once control is treated in terms of Agree, it becomes very hard to rule out movement within control structures in a principled fashion". In fact, movement *is* ruled out in a principled way if θ -roles are not features and cannot drive movement. The problem is just the opposite: How to *prevent* the alleged θ -driven movement from applying where it should not. This is the overgeneration problem discussed in Landau (2003) (see sections 11, 12 below).

specified on both; just like lexical nouns are inherently specified as [+SP] (*committee*) or [-SP] (*chair*), so can PRO be specified for either value. In this respect I departed from Landau (1999), where the [SP] value of PRO was contextually acquired.

At once, the issues of Inclusiveness and the alleged “copying” capacity of *Agree*, raised by Hornstein (p. 39), disappear. Notice that in fn. 71, Hornstein repeats the allegation that my conception of *Agree* is non-standard in that it involves assignment, not checking. Even supposing this were a terrible flaw (which I doubt), the simple fact is that my conception of *Agree* is directly drawn from Chomsky (2000); namely, matching, valuation and deletion. To the extent that valuation does not violate Inclusiveness, nothing in (9b) does either.

Hornstein’s comments on the interaction of syntactic and semantic plurality in PC reveal a misunderstanding. For example, he writes that “...in PC cases, there is evidence both that the PRO is semantically plural and that it is syntactically singular. This is a problem for Landau’s (1999) proposal” (p. 44). In fact, not only is this not a problem – it is precisely the PC-effect that I had discovered! The key point about PRO in PC is that it behaves like a group name (e.g., *committee*), which is semantically plural but syntactically singular. Thus, the contrast in (10), in American English, parallels the one in (11).

- (10) a. The committee gathered before the vote.
b. * The committee consulted each other before the vote.

- (11) a. The chair preferred to gather before the vote.
b. * The chair preferred to consult each other before the vote.

Furthermore, in British English, where (10b) is fine – so is (11b). This is because the reciprocal phrase is licensed by *syntactic* plurality in American English, but merely *semantic* plurality in British English. Such variation is found across languages, dialects and even idiolects. This point is worth emphasizing – and it fact it was: “When testing the predictions of [the PC-generalization] via the licensing of “plural” morphemes in PC-complements, one should be careful to establish *independently* the specific agreement requirement of every relevant morpheme” (Landau 1999, p. 64; 2000, p. 50).

These warnings were to no avail, it appears. In fn. 78 Hornstein challenges my description of the facts on the basis of the failure of PC in (12a).

- (12) a. * John wants/decided to be similar/sing alike/be mutually supporting.
b. * John is similar/sings alike/is mutually supporting with Bill.

Hornstein notes that all these predicates also resist the commitative construction (12b), suggesting that “the relevant generalization behind PC is that certain verbs can select embedded committatives”.

Hornstein failed to test whether the predicates in (12) are licensed by semantic or syntactic plurality. In fact, they require the latter (in American English).

- (13) a. The members are mutually supporting.
b. * The committee is/are mutually supporting.

- (14) a. They sing alike.
b. * This team sing(s) alike.

- (15) a. John and Mary are similar.
b. * This couple is/are similar.

This observation is enough to rule out (12a), on a par with (11b). Furthermore, a commitative paraphrase is not necessary for PC.

- (16) a. The chair voted/decided to disperse until next week.
b. *The chair dispersed with the rest of us.

I conclude that none of the facts presented by Hornstein pose any challenge to the original statement of the PC-generalization. In fact, they are fully expected once the precise predictions of the generalization are understood.⁷

⁷Boeckx & Hornstein (2004) downplay the significance of partial control by saying 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” (p. 449). This statement is based on their observation that given the right context, even raising verbs support “partial readings”:

- i. John seems to be meeting all the time.

First, note that the relevant lexical property is absolutely systematic: All collective predicates participate in PC. Second, *any* predicate may be turned into a collective predicate by adding *together*, giving rise to productive PC constructions that cannot be relegated to idiomatic exceptions. While *meet* may be such an exception, other collective predicates are not, pointing to a clear distinction between raising and control environments:

4.2. Minimality

Next, Hornstein argues that (9b) violates minimality – F cannot Agree with T-Agr (or with PRO, in EC) across the closer DP, namely the controller. This type of Agree is thus “specially suited to the phenomenon at hand”. Strikingly, Hornstein failed to mention my explicit answer to this problem (Landau 1999, p. 84-86; 2000, p. 70-72). There I note that the structure of the MLC-violation in (9b) is formally equivalent to the structure of analogous violations of superiority in multiple-*wh* languages, as well as other dependencies discussed in Richards (1997): An attracting head first Agrees with a close target, then with a remote one. To accommodate these phenomena, Richards developed his Principle of Minimal Compliance (PMC), which I adopted for the OC dependencies. Hornstein may disagree with this account, but at least he has to address it.

4.3. Asymmetry of Agree

The next point Hornstein makes is that the “chaining” of Agree relations depicted in (9b) does not guarantee control. There seem to be two objections here, the first of which rests, again, on misattribution. For PRO to be controlled by DP, Hornstein argues, it is not enough that Agree is a transitive relation: “Landau must also be assuming that AGREE is symmetric” (p. 39-40). However, Agree is asymmetric, in virtue of the distinction between probe and goal. “...if AGREE is not symmetric, then we cannot establish a control relation through the series of AGREE operations... The reason is that we cannot deduce that DP agrees with PRO from the fact that other agreement operations took place” (p. 40).

This objection rests on Hornstein’s own equivocation between the terms AGREE and *agree*, revealed in the above quote; it finds no support in Landau (1999). As discussed above, I adopted the Chomskyan view of Agree (=AGREE), based on valuation, hence intrinsically asymmetric. The error in Hornstein’s argument is the assumption that it is AGREE itself that must be symmetric (to guarantee control), rather than its output – agreement. Indeed, Agree (T,DP) is asymmetric (DP values T’s ϕ -features, T values DP’s case feature), but crucially, its output is symmetric: We say that T and DP agree with

We admitted that...

- ii. * John seems to be working together/gathering all the time.
- iii. John planned to work together/gather all the time.

each other, simply because they come to share their features. When interested in agreement (as opposed to Agree), we care about a symmetric outcome of an asymmetric operation.

Consider an analogy from reference assignment in discourse.

(17) John₁ entered the room. He₁ looked around. He₁ sat down. Then he₁ poured himself₁ a glass of beer.

Analyzing the process by which reference is assigned in this discourse, we want to say that *John* fixes the reference of the first *he*, which fixes the reference of the second *he*, which fixes the reference of the third *he*, which fixes the reference of *himself*. These are all asymmetric relations. By contrast, focussing on the resulting interpretation, we may simply say that the five nominal expressions are coindexed, a symmetric relation. I see no problem in this description, here or in (9b).

The chain of Agree operations in (9b) achieves two goals. First, it matches the ϕ -features of PRO with those of the controller DP. Second, it coindexes those two elements, establishing variable binding. For some reason, Hornstein seems to think that coindexing can only be achieved by Move, not by Agree. As far as I can see, this is a stipulation. Furthermore, the claim (in fn. 72) that coindexing does not capture the tight referential dependency in OC (presumably, *de se* interpretation) is a double-edged sword; as I have shown previously (Landau 1999, p. 41-42; 2000, p. 29-30; 2003), neither does Hornstein's "complex monadic predicate" distinguish variable binding from *de se*.⁸

4.4. Null Case

Hornstein points out that my theory of OC is not obviously consistent with the theory of null case (Martin 1996, 2001). He then attempts to reconcile the two theories, again attributing to me unfounded stipulations. In fact, Landau (1999, 2000) says nothing about the distribution of PRO. Save for one unfortunate typo, the position is summarized at the outset: "Most probably, PRO occurs only in the subject position of non-finite clauses. This study has virtually nothing to add to this observation... perhaps the distribution of PRO is an irreducible fact of UG" (Landau 1999, p. 11; 2000, p. 2).

⁸ Hornstein wonders, more than once, how Agree can establish antecedence – "especially when it is as round about as in (76b)" (=9b) above). This is like wondering how coindexing can establish antecedence between *John* and *himself* in (17). Does the number of intermediate steps "weaken" the dependency?

I never advocated the null case theory, and I think that there are overwhelming reasons to reject it. Hornstein (2003) himself discusses several good arguments, and others can be adduced (see Baltin & Barrett 2002). Most notably, Martin's notion of [\pm tense] is both semantically incoherent and fails to demarcate control from raising complements. My own view, unlike Hornstein's, is that PRO is case-marked just like any other DP (see sections 6 and 8 below). Ample evidence for this comes from languages exhibiting subject-oriented case concord in controlled infinitives (Russian, Icelandic) or subjunctives (Greek, Romanian, Hebrew, Persian, etc.). These issues are extensively discussed in Landau (2004), where I also attempt (for the first time) to provide an alternative account of PRO's distribution, divorced from case theory altogether.

4.5. Gerunds and Tense

Next, Hornstein (ex. (78)) notes that gerundive complements tolerate PC, a "problem" for my analysis. The facts are well-known, though; I have noted similar examples in the past (Landau 1999, p. 58; 2000, p. 45). So what is the problem? First, Hornstein notes, Stowell (1982) argued against gerunds being [+tense] (the condition for PC). Second, gerunds are generally assumed to be TPs, not CPs, so they should not afford the PC mechanism, which is crucially linked to C.

As to the first point, we have to conclude that Stowell was wrong. While some gerunds are untensed, others are tensed; cf. the following contrast.

- (18) a. *Yesterday, John avoided leaving tomorrow.
b. Yesterday, John preferred leaving tomorrow.

Stowell argued that it is precisely this variability in temporal interpretation that attests to the lack of intrinsic tense in gerunds; supposedly, their tense is specified by the matrix verb. The problem is that the same logic can be applied to minimal *infinitival* pairs, potentially voiding Stowell's claim that they do contain tense.

- (19) a. *Yesterday, John managed to solve the problem tomorrow.
b. Yesterday, John hoped to solve the problem tomorrow.

The alternative, argued for in Landau (1999, 2000), is very simple. Taking the temporal mismatch as a reliable diagnostic for semantic tense, we conclude that the complements in (18a) and (19a) are untensed whereas those in (18b) and (19b) are tensed. Notice that

this distinction cuts across the gerund/infinitive categories. This is probably correct. Crosslinguistically, gerunds are significantly rarer than infinitives. Many languages, in fact, express (18a,b) with infinitival complements; crucially, their tense properties remain constant. Conversely, complements to English verbs that select either form (*prefer, hate, start, etc.*) are consistently tensed or untensed, whether realized as a gerund or as an infinitive.

As to the second point – is it a real problem to assume that gerunds are CPs? This is less clear today than it was 20 years ago. First, the lack of interrogative gerunds could be handled at the featural level, not necessarily the category level. *For*-infinitives are similarly never interrogative, yet we do not conclude that they are bare TPs. Second, it is quite possible that some gerunds do contain complementizers. In Landau (2002) I argued that *from* is a negative complementizer heading gerundive complements of negative verbs (*refrain, prevent, etc.*). All in all, the evidence against gerunds being CPs is very weak, at best. But if gerunds may be tensed CPs, nothing blocks them from displaying PC.

4.6. T-to-C

Still on the issue of tense, Hornstein suggests (p. 42) that the mechanism of raising T-Agr to C, posited in PC complements, may overgenerate to EC-complements. The reason is that EC-complements display strict tense dependency (in fact, identity) between the matrix and the embedded tenses. Presumably T would have to raise to C to impose this selectional restriction (without violating locality of selection). But then the contrast with PC complements is lost.

The point is correct, but harmless. Indeed, Landau (1999, 2000) left the realization of tense selection unstated. This lacuna is filled in Landau (2004), where a fully explicit theory of clausal complementation is laid out. Still, the problem could be easily solved within the framework of Landau (1999). EC-verbs could select the feature [–Tense] on the C head of their complements, which in turn is matched against the embedded T. Alternatively, the lack of [Tense] on C in these complements could allow for a direct selectional relation between the matrix verb and the embedded T, the idea being that selection, like checking, is obstructed only by potential interveners. Either way, T-to-C is not needed in EC-complements, and the contrast with PC-complements is preserved. That said, Landau (2004) recasts the EC/PC distinction in terms of pure Agree, without movement, although the key contrast – the nature of [Tense] on C – is retained.

Lastly, Hornstein argues (p. 42) that there is little evidence for overt embedded T-to-C in English, so my analysis of PC in (9b) is dubious. I could retort that there is

likewise little evidence for overt NP-raising in *John likes himself*, yet NP-raising from the object position allegedly occurs in this sentence, according to Hornstein (2001). More seriously, I doubt that Hornstein really endorses such a naïve (phonetic?) view of “evidence for overt movement”. He and I work within a richly theoretical framework, where quite a few degrees of freedom (e.g., null morphemes, vacuous movement, late insertion, etc.) separate phonetic strings from abstract syntactic trees. Evidence for movement *could* consist in phonetic rearrangement of terminals, but surely does not have to.

4.7. A Meaning Postulate for PC

In place of my syntactic account of PC, Hornstein proposes a lexical account. A meaning postulate could be built in the lexical entry of PC verbs (like *want*), specifying that the subject of their nonfinite complement may refer to either the controller, or a group containing the controller and some contextually specified others.

Hornstein admits that this is “a very uninteresting account”, but one that works. Yet it is important to realize what is being missed by this account. The meaning postulate approach leaves three major puzzles unanswered. First, why is it that the embedded tense is relevant at all to PC – rather than, say, the transitivity of the embedded predicate, the animacy of its object, the social status of the subject and so on? Second, why is it that *presence*, rather than absence, of tense, licenses PC? In other words, why does *decide* license PC and *manage* force EC, and not the other way round? Third, why is it that no raising predicate license PC? Evidently, meaning postulates are too unrestricted; although they can construct any list we design them to, they provide virtually no insight into the underlying generalizations. By contrast, the syntactic account crucially identifies the presence of embedded tense (and PRO) as the vehicle of PC, making sense of the observed generalization.⁹

⁹ In another recent attempt at a semantic reduction of PC, Jackendoff & Culicover (2003) propose that PC is coerced through an abstract INTENTION predicate. This explains, they claim, why PC is restricted to control verbs expressing intentions and to irrealis complements. The problem is that neither claim is true. As shown at length in Landau (1999, p. 58-62; 2000, p. 45-48), PC occurs under factive and propositional verbs, both allowing *realis* complements, and neither of which involves intention; e.g., the Italian examples:

- i. Maria pensava che Gianni si fosse pentito di essersi baciarti alla festa.
Mary thought that John had regretted to-be-SI kissed at-the party
'Mary thought that John had regretted to have kissed at the party'
- ii. Il presidente crede di essersi riuniti inutilmente la notte scorsa.
the chair believes to-be-SI gathered in vain the night last

Hornstein notes that meaning postulates can only be imposed on arguments, predicting lack of PC in adjuncts. The observation is correct, as can be seen in right-adjoined nonfinite adjuncts headed by *before/after/while/without*. Hornstein also includes rationale clauses in this set, unfortunately so, since these form a category of their own. Despite the OC characteristics displayed in Hornstein's example (85), rationale clauses also show NOC characteristics, a fact known since Williams' (1974) famous example *Grass is green (in order) to promote photosynthesis*. I refer the reader to Landau (1999, p. 206-211; 2000, p. 179-183), where the complexities of rationale clauses are addressed.

The lack of PC in temporal adjuncts, which are clearly tensed, does not jeopardize the PC-generalization. To begin with, Landau (1999, 2000) sets adjunct control apart from complement control. Only the latter is mediated by *Agree*, an operation which cannot penetrate islands (like adjuncts). My own view is that right-adjoined adjuncts are construed as predicates, following Williams (1992). Being directly predicated of a singular subject, they can support PC no more than any secondary predicate can.

- (20) a. * John called Mary before meeting in the restaurant.
b. * John called Mary together/while together/as a team/extremely polarized.

Thus, both the OC character of right-adjoined temporal adjuncts and the lack of PC follow straightforwardly from predication.

5. Non-Obligatory Control

In the previous section, I used the careful label "right-adjoined temporal adjuncts", rather than just adjuncts, in order to emphasize that not all adjuncts are alike, contra Hornstein's description. Rationale clauses, as mentioned above, do not obviously show OC. Moreover, *left*-adjoined temporal adjuncts support NOC (with subtle differences between participial and gerundive adjuncts), a fact documented at length by Bresnan (1982), Williams (1992), Kawasaki (1993), Landau (1999, 2000) and Lyngfelt (1999). These facts were brought up again in Landau's (2003) critique of Hornstein (1999), but received no attention in Hornstein (2003). Likewise, Landau (1999, 2000, 2003) refuted the common assumption that interrogative complements fall under NOC. It was shown that these complements

'The chair believes to have gathered in vain last night'

I realize that the temptation to explain PC in purely semantic terms is hard to resist, yet partial explanations, unlike partial control, cannot be coerced.

exhibit every OC property, including partial (but not long-distance) control. This is of course highly problematic for the raising analysis of OC, which is blocked in islands. Again, Hornstein (2003) contains no response to this issue.

The one concession Hornstein makes is to accept that PRO in NOC may bear more resemblance to a logophor than to a pronoun. He notes (p. 51) that the implications of this distinction are not evident, however, in light of the uncertainty surrounding the syntax and semantics of logophors. Those comments are well taken. I also agree with fn. 98, where it is said that Landau (1999) was not explicit about the “competition” between OC and NOC. In particular, the question why OC holds whenever it can was not addressed. I agree that some economy metric is needed: Try to establish control syntactically (by Agree/Move) before you resort to pragmatics (logophoric/pronominal coreference).

6. Case Percolation

In Landau (2003) I pointed out that languages with case concord reveal a crucial contrast between raising and control constructions. When the embedded predicate assigns inherent (quirky) case to its subject, this case shows up on the matrix subject in raising, but not on the matrix controller in control. The latter bears the regular local case (e.g., nominative), while PRO bears the quirky case (revealed on an agreeing element, like a secondary predicate, or floating quantifier). Icelandic, for example, displays the following pattern (Sigurðsson 1991), illustrated below with dative quirky case (FQ=floating quantifier).

(21) Icelandic

- a. *Raising*
DP₁.DAT ...V ... [t₁ ... V ... FQ.DAT]
- b. *Control*
DP₁.NOM ...V ... [PRO₁ ... V ... FQ.DAT]

Icelandic is not unique; Russian, Hungarian and Greek behave similarly (see Landau (2004) and the references therein).¹⁰ This pattern strongly suggests that one chain is involved in raising and two in control, contra the reductionist analysis.

¹⁰ I am abstracting away from the case split internal to Russian between subject control (without an overt C), where PRO inherits nominative case from the controller, and the "elsewhere" situation, where PRO is locally assigned dative case. Only the latter option is analogous to Icelandic. For relevant discussion, see Comrie (1974), Neidle (1988), Franks (1990), Franks & Hornstein (1992), Babby (1998).

Hornstein (2003, ex. 41b) brings up interesting data that challenge this picture. In Chilean Spanish, the quirky dative case licensed by the embedded (psychological) predicate may show up on the controller.¹¹

- (22) a. Marta le quiere gustar a Juan.
 Marta cl.DAT wants to-please to Juan
 'Marta wants for Juan to like her' (Marta wants to be liked by Juan)
- b. A Juan le quiere gustar Marta.
 to Juan cl.DAT wants to-please Marta
 'Juan wants to like Marta'

In (22b), *a Juan* receives the two experiencer θ -roles, but crucially, its dative case is assigned by the embedded verb *gustar* (*querer* does not assign dative case). Hornstein notes that this pattern is the expected one under the "Control=Raising" view, admitting that the Icelandic pattern remains a problem (fn. 32).

The actual force of this (admittedly intriguing) datum, however, is questionable. Gonzalez (1990) makes clear that this type of "unusual inversion" is severely restricted, "occurring only with the Equi-Clause-Reduction predicates *querer* 'want' and *tratar* 'try'" (p. 101). In fact, it is most common with *querer*, while some speakers also accept it with *tratar*. Other predicates disallow "dative percolation".

- (23) A Marta le trataron/*desearon/*intentaron de gustar los gatos,
 to Marta cl.DAT tried/ *desired/ *tried of to-please the cats
 pero le produjeron alegria.
 but cl.DAT produced allergy
 'Marta tried/*desired/*tried to like cats, but they produced allergy on her'

Furthermore, Bošković (1994: fn. 35) notes that the status of the judgments in (22) is quite contentious; some Chilean Spanish speakers reject (22b), while others find no semantic difference between (22a-b). Indeed, Gonzalez notes that when the dative argument is a clitic, the sentence becomes ambiguous. Given that Spanish allows postverbal subjects, it is not clear why the first reading of (24) is missing from (22b).

¹¹ See Gonzalez (1988, 1990), cited in Bošković (1994).

- (24) Te quiero gustar.
 cl.DAT want. 1sg to-please
 'I want you to like me'
 'You want to like me'

The puzzle for Hornstein's analysis is why this construction is so limited in distribution (at most two restructuring verbs, for some speakers in some dialects). Given the purely syntactic mechanism of A-movement, nothing seems to block the raising of a quirky dative DP to the external argument position of *any* matrix verb. This option is not even expected to be restricted to restructuring verbs, as all OC verbs trigger raising in Hornstein's theory.

A further puzzle (already recognized by Gonzalez, fn. 7) is the non-iterative nature of dative percolation.

- (25) *A Marta le quieren tratar de gustar los gatos.
 to Marta cl.DAT wanted to-try of to-please the cats
 'Marta wants to try to like cats'

As the English translation indicates, OC dependencies can be chained; so can A-movements. If A-movement derives (22b), then the fact that it cannot apply successive-cyclically to derive (25) is unexpected.

These observations suffice to undermine the claim that (22b) demonstrates movement into a θ -position. Still, we may ask how such examples are derived. I will offer two possibilities, leaving the choice between them open.

A first possibility is to treat *querer* as a modal of sorts, along the lines suggested by Wurmbrand (2003) for *wollen* 'want' in German. Wurmbrand observes that the verb *want* in many languages exhibits "quasi-modal" properties. Modals are raising predicates, that assign no θ -role to their subject. The semantic import of the modal – obligation, necessity, permission etc. – is rooted in lexical entailments interacting with context, not in θ -theory. If indeed *querer* is a modal element, the alternation in (22) is of a familiar sort, on a par with other modal alternations.

- (26) a. John may visit Mary.
 b. Mary may be visited by John.
 c. There may be no more than one visit per week.

On that account, the sense that *Marta* is the wanter in (22a) and *Juan* is in (22b) is not due to a different distribution of θ -roles, but rather due to the association of the matrix subject with the lexical entailments of the modal (desire in (22), permission in (26)).¹² (22b) is monoclausal; *A Juan* raises to a nonthematic subject position, preserving its inherent case, the standard behavior of (derived) quirky subjects. Since a clause may contain at most a single modal, example (25) is impossible.

A second possibility, perhaps more daring, is to acknowledge that *querer* is a standard subject control verb, which assigns its own external θ -role. The challenge, on this account, is to explain how the external argument of *querer* comes to bear the dative case associated with the embedded verb.

(27) Chilean Spanish

Control

DP₁.DAT ... V ... [PRO₁ ... V_{psych} ...]

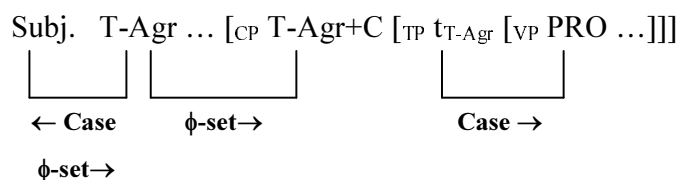
Consider the Agree-based mechanism illustrated in (9b). Establishing control requires checking (valuation) of ϕ -features between the matrix v/T and the embedded T-Agr. To the extent that structural case is a reflex of ϕ -features, it is potentially checked in this process. As discussed in section 4.4, there is every reason to believe that PRO bears case. The question then is – how is the case of PRO acquired? More technically, how is the case feature of PRO valued?

Several options suggest themselves. The normal way would be parallel to case checking in finite clauses. The infinitival T-Agr contains a valued case feature, and by Agreeing with PRO, values the latter's case. The actual value could vary across languages: in Icelandic/Greek it is nominative, in Russian/Hungarian it is dative. On this scenario, the matrix functional head effecting control (say, T) only values the ϕ -features of PRO, not its case. Below I illustrate the relevant valuations with their direction.¹³

¹² *Want* normally disallows expletive subjects, but Wurmbrand (2003) shows that under certain circumstances this restriction is lifted. I thank Susi Wurmbrand for discussion of these issues.

¹³ The ϕ -features of PRO come to be valued after those of the embedded T-Agr are; I omit this part from (28).

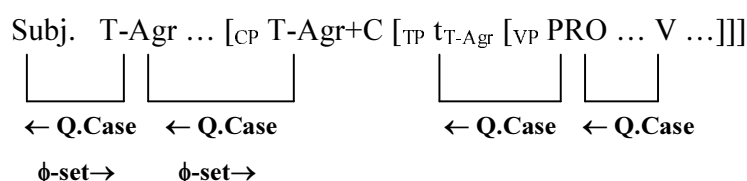
(28) Case Independence



This is what happens in the normal case, even if no direct evidence for the actual case value of PRO is available in the language; the only special feature of languages like Russian and Icelandic in this regard is the availability of case concord, which reveals the case of PRO. Scenario (28) also applies in Icelandic when the embedded predicate assigns quirky case, the only difference being that the case feature of PRO is valued by the lexical predicate, not by T-Agr.

Consider now the peculiar case of Chilean Spanish (27), where the embedded predicate appears to value the case feature of the controller. I suggest that this may come about whenever the case feature of the matrix functional head (here, T-Agr) is optionally unvalued. In this situation, the value of PRO's case feature (determined by the embedded predicate) may actually “percolate up” to the controller via the matrix T-Agr, since the latter cannot provide its own value. The valuation scheme is the following.¹⁴

(29) Case Percolation



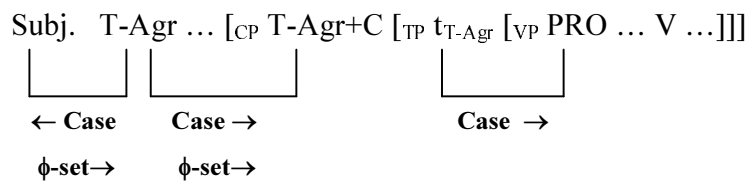
The difference between Icelandic and Chilean Spanish, then, boils down to this: In Icelandic, finite T-Agr obligatorily bears a valued nominative case feature. In Chilean Spanish, it is either valued nominative or unvalued. Notice that the locus of this difference – a property of a functional head – makes it a natural parameter.

The logic of this system predicts another possibility: The case feature of the embedded T-Agr is unvalued, and as a result PRO inherits its case from the controller

¹⁴ Notice that the case feature of the *embedded* T-Agr may be valued in (29), but still “overwritten” by the quirky case of PRO, as usual in such contexts.

(more precisely, from the matrix functional head that values the case of the controller). Indeed, as Cecchetto & Oniga (2004) report, this is the situation in Latin: Subject controlled PRO bears nominative case, object controlled PRO bears accusative case. Latin is the mirror image of Chilean Spanish, as can be seen below.¹⁵

(30) Case transmission



Putting all these data together, we can appreciate the typological space afforded by the Agree-based approach to OC. Depending on which, if any, of the functional heads Agreeing with the controller and with PRO are unvalued for case, the case of PRO is independent from, transmitted from, or percolates to the case of the controller. In contrast, we predict that in genuine raising, the trace position would be either caseless, or marked with quirky case. This would *look* like case transmission or percolation, respectively. Technically, though, the raising chain is case valued only once, whereas the OC dependency is valued twice. What is excluded in principle is case-independence in a raising chain; this follows from the traditional assumption that A-chains bear a unique case.

Under the reductionist view, however, the last prediction is lost. As far as their case patterns are concerned, raising and OC should not differ, since both involve an A-chain with a single case value. For this reason, the Icelandic/Russian/Hungarian pattern is problematic for Hornstein's theory. By contrast, The Chilean Spanish facts, which are extremely idiosyncratic to begin with, are harmless to the Agree-based theory, and in fact follow from its design.¹⁶

¹⁵ Apparently Icelandic also exhibits this pattern, alongside case-independence (see Andrews 1976, Thráinsson 1979).

¹⁶ If the Chilean Spanish facts reduce to an isolated property of *querer*, independent evidence will be needed to exemplify the model of Case Percolation in (29). If none shows up, we may conclude that UG blocks this option (say, by stipulating that the case feature of finite T-Agr is always valued).

7. Backward Control

The claim that backward control exists in natural language is perhaps the most interesting contribution of the reductionist camp to the debate on the nature of OC. According to this camp, backward control is nothing but covert movement of the “controller” DP to its matrix *thematic* position; if OC is A-movement and A-movement can be covert, then backward control is an inevitable possibility. PRO-based approaches, in contrast, cannot explain how PRO can be licensed and interpreted in a position higher than the controller’s.

In this section I will not attempt any analysis of backward control. Instead, I will lay out some skeptical thoughts about the force of the conclusions warranted by the evidence that is currently available. I believe that this skepticism is in place, and I hope that it will prompt further research into this important topic.

Hornstein cites two studies arguing for backward control: Farrell (1995) and Polinsky & Potsdam (2002). Let us consider them in turn.

Farrell (1995) discusses causative complements to *fazer* (‘make’) and *mandar* (‘have’; literally, ‘send’) in Brazilian Portuguese (BP), illustrated below.

- (31) a. A mulher fez o nenê dormir.
‘The woman made the baby sleep’
- b. Eu mandei o sapateiro concertar esse sapato.
‘I had the cobbler fix these shoes’

These infinitival complements, according to Farrell, have an ECM syntax and an object control semantics. Like object control complements, they do not show voice transparency under passivization of the embedded verb; the causee is subject to selectional restrictions – it cannot be a clause or an expletive; furthermore, with *mandar*, it must be animate.

At the same time, the overt causee seems to occupy the embedded subject position (the one reserved for PRO under normal object control). It can be realized as a subject (1st person) pronoun (32), which is otherwise excluded from object positions, and can follow the embedded verb when the latter is unaccusative, a standard property of subjects in BP.

- (33) A professora mandou/fez eu apagar o quadro.
the teacher had/made I.NOM erase the board
‘The teacher had/made me erase the board’

Hornstein concludes from these facts that the BP periphrastic causative construction displays backward control: The causee raises covertly to the thematic position of the matrix object, establishing object control. It is curious, though, that Farrell himself, although using the same terminology of “backward control”, draws different conclusions from his own data. Farrell argues that the matrix object position is in fact never realized in the syntax. Following Jackendoff (1990), he views Lexical Conceptual Structure as the level where control is established. In fact, he takes his data to show that any *syntactic* approach to control is misguided.

Let me first say that I find the BP data fascinating, although more puzzling than what either Farrell or Hornstein seem to acknowledge. First, notice that the causee *can* be realized as an object clitic on the main verb.

- (34) O professor os fez estudar mais.
The professor cl-3.Masc.pl made study more
‘The professor made them study more’

Farrell explains that the causee may either get nominative case from the embedded Infl, as in (33), or accusative case from the matrix verb, as in (34). He points out (and following him, Hornstein does) that unlike standard object control verbs (34b), *fazer* (or *mandar*) cannot be passivized in the causative construction (34a).

- (35) a. * O nenê foi feito dormir.
‘The baby was made sleep’
b. Os alunos foram forçados a estudarem mais.
‘The students were forced to study more’

Farrell and Hornstein take this fact to show that the causee does not occupy a matrix object position. But does it really show that? Recall that (34) has shown that an ECM analysis is possible for these constructions. If so, it is far from clear what blocks (35a). If the causee is an embedded ECM subject, matrix passivization should withdraw its accusative case and allow raising to the matrix subject position. Notice that this should be possible whether the causee stops at the matrix object position, to pick up a θ -role (as Hornstein would have it), or not (as Farrell would). In fact, passivization of causatives is

unavailable in many languages ((35a) is impossible in English and French too). Little follows from this poorly-understood observation about the surface position of the causee.

In sum, contrary to Hornstein, I do not consider these facts as “evidence that the thematic complement is not a syntactic object of the matrix in overt syntax” (p. 57). In fact, they strike me as a real puzzle for everyone.

A much more persuasive case for backward control, I believe, is made by Polinsky & Potsdam (2002). Through a series of tests P&P show that the ergative argument in constructions like (36) in Tsez occupies the embedded subject position, although associated with both the lower and the higher subject θ -roles (II/III are noun class agreement markers).

- (37) Δ_1 [kid-ba₁ ziya b-išra] y-oq-si.
II.ABS girl.II-ERG cow.III.ABS III-feed.INF II-begin-PAST.EVID
‘The girl began to feed the cow’

As I said, I find most of P&P’s arguments compelling, so I will only comment on the weaker aspects of their analysis.

First, perhaps the most striking feature of backward control is its rarity. In Tsez, only two verbs display it; the numbers hardly exceed five in other languages. Most commonly, the backward control verbs are aspectuals (*begin*, *continue*, *stop*, etc.), which also have a standard raising guise. This suspicious overlap is recognized by P&P, and is attributed to the tendency for such verbs to undergo V-to-T raising. In their analysis, V in T may satisfy the EPP, allowing the “controller” to remain in its embedded position. Yet the ability of a verb to satisfy EPP is a stipulated lexical property – a [+D] feature. Furthermore, it is not clear why backward control exists, but backward raising does not (as far as we know).

Another puzzling issue is the status of the matrix null controller (Δ in (37)) with respect to case. On the one hand, P&P (p. 258) explain the apparently exceptional agreement of *-oqa* ‘begin’ with the ergative argument (agreement in noun class is always with the absolutive argument) by positing a coindexed null matrix absolutive argument which is the true trigger of agreement. On the other hand, the actual analysis P&P end up with (see their ex. 65) deprives the control verb *-oqa* ‘begin’ of its case assigning capacity. The lack of absolutive case in the matrix clause is in fact crucial: It explains the very effect of OC. If an independent DP were merged as the external argument of *-oqa*, its case feature would remain unchecked. Thus, the only DP that can check the matrix θ -

role is the embedded subject, which has already checked its ergative case feature downstairs. This is done by covert raising of this DP to the matrix clause.

It seems to me that P&P run into a contradiction here. To explain the agreement facts, they must allow absolutive case in the matrix clause. To explain OC, they must exclude it. Conceivably, they could acknowledge that the LF A-chain of 'the girl' in (37) bears two distinct cases, not an unprecedented situation (see Bejar & Massam 1999). This, however, would rob them of the account for why a second DP cannot be merged in the matrix clause (e.g., **The boy.ABS [the girl.ERG the cow.ABS to-feed] began*). That the very effect of OC hangs on a problematic technical assumption, rather than on a natural semantic analysis of *begin*, is a testimony, in my view, to the limitations inherent to the movement approach to OC.

Having said that, I wish to stress that the evidence for backward control (more so in Tsez than in Brazilian Portuguese) is a real challenge to standard theories of OC, perhaps the most challenging phenomenon presented so far by proponents of the movement approach.

8. Finite Control

Hornstein (2003, section 1.8.2) describes OC into finite clauses in Brazilian Portuguese (BP), as below.

(38) O Joao₁ disse [que ec₁ comprou um carro novo].

'John₁ said that he₁ bought a new car'

BP has lost referential pro-drop, and appears to allow referential null subjects in finite clauses only under OC. Hornstein suggests that here too, A-movement applies to produce OC. Importantly, he assumes that case marking of the embedded subject is optional (a fact related to the simplification of the verbal paradigm). When this position is caseless, the embedded subject may raise to the matrix clause, where it checks its case (and an additional θ -feature). BP also allows raising out of finite complements.¹⁷

Finite control is in fact more common than normally assumed. It is found throughout the Balkan family, where the infinitive was lost and replaced by the

¹⁷ Hornstein remarks that "... it is interesting that these BP clauses tolerate both control and super-raising, suggesting that both raising and control are reflexes of the same operation" (p. 60). Clearly, though, nothing of the sort is suggested. Finite clauses are likewise transparent to both *wh*-movement and pronominal binding, yet no underlying common operation is suggested by that.

subjunctive. Other languages that exhibit finite control are Hebrew, Kannada, Persian and Dogrib (see Landau 2004 for a comprehensive description and analysis).

I am in complete agreement with Hornstein that the controlled empty category in finite clauses is the classical OC PRO (for him, an A-trace). What I disagree with is the alleged lack of case for this element. The BP evidence cited by Hornstein is neutral, but other languages make it clear that OC PRO in finite clauses bears the standard case that any lexical subject would bear in that position.

The examples below illustrate *low* case marking of OC PRO in finite complements.

(39) *Greek* (Philippaki-Warbuton & Catsimali 1999)

- a. Anangasan tin Eleni [PRO na milisi afti i idhja].
 forced.3pl the Eleni.ACC PRO.NOM PRT speak.3sg she herself.NOM
 ‘They forced Helen to speak herself’

Hungarian (J. Horvath, p.c.)

- b. János bízatta Marit [hogy PRO maga/*magát
 John.NOM urged.3sg.DEF Mary.ACC that PRO.NOM herself.NOM/*ACC
 beszéljen az orvossal].
 talk.SUBJ.3sg the doctor-with
 ‘John urged Mary to talk to the doctor herself’

Notice that the emphatic reflexives bear nominative case. Such emphatic elements display case-concord – they agree in case with the DP they modify. Thus, we can conclude that PRO bears nominative case in (39) – *despite* the fact that the controller is marked accusative. This situation is parallel to what we have seen in Icelandic and Russian OC infinitives: The controller and PRO bear distinct cases (see section 6). On a theory that divorces the distribution of PRO from case, like Landau (2004), this phenomenon presents no mystery. If, however, lack of case at the base position is a prerequisite for A-movement, and OC is but A-movement, then data such as (39) are extremely puzzling.

9. PRO-gate

The last section in Hornstein (2003) argues that analysing OC as A-movement provides an elegant account for an old chestnut in the study of control – the PRO-gate. I believe that this is the weakest part of Hornstein's paper, containing few if any empirically valid

claims. Therefore, it is important to clarify the factual nature of PRO-gate in particular and subject gerunds in general.

The PRO-gate effect is illustrated in (40); a PRO subject in a gerundive subject clause allows a pronoun to be A-bar bound even to left of a variable (the *wh*-trace), in contrast to a lexical subject in the same position, which gives rise to the familiar WCO violation.

- (40) a. Who_i did [PRO_i kissing his_i mother] upset t_i?
b. * Who_i did [Mary's/his_i/him_i kissing his_i mother] upset t_i?

Following Kiguchi (2000) and Kiguchi & Hornstein (2001), Hornstein makes the following argument.

- (41) a. A-movement circumvents WCO (i.e., a pronoun bound by an A-trace will not violate WCO).
b. PRO in OC is an A-trace.
c. PRO in subject gerunds displays OC.
d. Therefore, PRO in subject gerunds circumvents WCO (= PRO-gate).

(41a) is well-established (e.g., *Who_i t_i seemed to his_i wife t_i to be clever?*). (41b) was precisely the issue under debate in Landau (2003); for the sake of the present argument, let us grant it. The novel claim, then, is (41c). If true, then (41) tracks a valid deduction, and PRO-gate is indeed explained.

However, (41c) is false: PRO in subject gerunds falls under NOC. In what follows I show why Hornstein's claim to the contrary is unsupported.

To show that PRO in subject gerunds requires an antecedent, Hornstein cites (42).

- (42) a. * Shaving himself impressed Mary.
b. * Shaving himself made it seem cold outside.

These examples show little out of context. It has been widely documented (e.g., Grinder 1970, Kuno 1975, Williams 1992, Bresnan 1982, Manzini 1983, Lebeaux 1984, Kawasaki 1993, Landau 1999, 2000) that a local object DP is not an obligatory controller for PRO in a subject gerund. Indeed, Kiguchi & Hornstein (2001) admit that previous discourse may allow extrasentential reference for such PRO, citing (43a). Furthermore, (42a) *is* possible in a strongly deictic context, where the speaker points to (say) John,

saying “Shaving himself...”. Compare (43b), where the 1st/2nd person PRO is easily accessible in any context.

- (43) a. John₁ put on a bushy hat. PRO₁ having shaved himself₁ earlier made it seem very cold.
b. PRO₁ Shaving myself₁/yourself₁ impressed Mary.

So the first property of OC already fails to hold of PRO in subject gerunds: It does not require a local grammatical antecedent.

Next, Hornstein claims that split control is blocked.

- (44) *John₁ said that PRO₁₊₂ shaving themselves upset everyone₂.

Again, the example is misleading. Control into subject gerunds is subject to logophoricity constraints, as recognized in the studies cited above. Those constraints are quite complex, involving both syntactic prominence, animacy, discourse salience and mental perspective. Provided that both controllers qualify as logophoric antecedents, control can be split between them. Example (45a) is provided by Grinder (1970); (45b) is my own minimal modification of (44), which makes it grammatical.

- (45) a. That [PRO₁₊₂ covering themselves with mud] disturbed Spiro₁ amused Dick₂.
b. John₁ told Bill₂ that PRO₁₊₂ shaving themselves would upset everyone.

Unlike OC environments, where split control is indeed uncommon (see section 3), NOC environments allow it rather liberally, contra Hornstein's description.

Hornstein also claims that PRO in subject gerunds only permits a *de se* interpretation and a sloppy reading when the controller is modified by *only* (see Hornstein's ex. 134, 135). However, the judgments on such examples are very subtle, and are easily reversed with contextual manipulation. Consider the following scenario.

- (46) Fred has recently barely survived a ferocious assault by an alligator that was videotaped live. Fred lost his memory of this entire event, and he now watches the videotape. Describing Fred's reaction to the videotape, we say:
- a. Being assaulted by an alligator amused Fred.
b. Fred was amused to have been assaulted by an alligator.

Speakers judge that in this scenario (46a) makes sense (even if it is morally regrettable) but (46b) must imply that Fred is a suicidal freak. In other words, (a) is compatible with Fred's not being aware that he himself is the victim of the assault, but (b) is not. The implication is that PRO in subject gerunds can be interpreted *de re*, unlike PRO in OC complements, which is necessarily interpreted *de se*. Again, subject gerunds behave like NOC.

As to *only*-sentences, the strict reading is possible given the right context. (47a) can imply that only Bill was amused by *his* flirting around (his wife didn't find it all that amusing); the same reading is available in the stripping constructions (47b,c).

- (47) a. Flirting around amused only Bill.
- b. Flirting around amused Bill, but not his wife.
- c. Flirting around amused Bill, but offended his wife.

I should mention that there is some speaker variation here, but clearly the majority allows the strict reading. Nothing like that is observed in classical OC (into complements).

Hornstein's fourth claim is that the antecedent of PRO must be the most prominent DP available and cannot be buried inside another DP.

- (48) a. * PRO₁ shaving himself made Mary believe John₁.
- b. * PRO₁ shaving himself upset John's₁ mother.

The examples are again partial. Notice that *Mary* in (48a) is a potential logophoric antecedent (subject of a mental attitude) while *John* isn't. If NOC PRO is a logophor, this fact alone – and *not* locality – would rule out control by *John*. Whenever two DPs follow the gerund, it is indeed easier to take the closer one as a controller. This is no doubt due to the fact that this DP will be more prominent on the logophoric scale (its own perspective “embedding” that of the more distant DP). Processing limitations (favoring shorter dependencies) might also be at work. But this is just a strong tendency, not a grammatical constraint. Example (49), where long distance control skips a potential controller, is provided by Richardson (1986).

- (49) [PRO₁ storming out of the room that way after losing the game] convinced everyone that John₁ is very immature.

Another way of showing that proximity in NOC isn't a syntactic constraint is by using previous discourse to highlight the lower DP, thus making it the prominent logophoric center (see Kawasaki 1993 for evidence that "topicality" affects NOC). Compare (48a), where locality supposedly rules out control by *John* – with the following.

- (50) a. John₁ finally got what he wanted. PRO₁ shaving himself made Mary believe him₁.
b. John₁ knew that [PRO₁ shaving himself] made Mary believe him₁.

Furthermore, a local DP can perfectly be skipped if it is an expletive.

- (51) a. [PRO₁ getting himself a new pair of trekking shoes] made it look like John₁ was about to leave on a journey.
b. * [PRO₁ getting himself a new pair of trekking shoes] made Mary realize John₁ was about to leave on a journey.

This non-syntactic contrast is mysterious if the controller is moved from the position of PRO by sideward movement. For (51a-b), the decision whether sideward movement is allowed into the most embedded subject position would crucially depend on the identity of DPs merging *after* that point, in a higher cycle (namely, *it* or *Mary*). This look-ahead is precisely what is banned in Hornstein's strongly derivational theory.

(48b) does not represent a general fact either. Hornstein ignores familiar cases (discussed by Chomsky 1981, 1986, and Landau 1999, 2000) where a controller can be embedded inside a DP. (52a) is from Chomsky (1981).

- (52) a. PRO₁ finishing his work on time is important to John's₁ development/*friends.
b. [PRO₁ perjuring himself] tarnished John's₁ reputation.
c. [PRO₁ reciting these silly lines] insulted John's₁ intelligence/*sister.

The contrast between the good versions of (52a,c) and the bad ones (including (48b)) is that the controller in the former is embedded inside DPs which are not in themselves potential controllers. Moreover, nouns like *reputation*, *development*, *intelligence* etc. all designate some attribute of the personality of their possessor; Landau (1999, 2000) dubs them "logophoric extensions". It is a fact – and crucially, not a *syntactic* fact – that these nouns are "transparent" to NOC, whereas individual-referring nouns are not. I cannot see how the (sideward) movement approach can make sense of the data in (49)-(52).

Thus, on every single criterion, in contradiction to Hornstein's claims, PRO in subject gerunds displays NOC.¹⁸

Hornstein points out that whenever sideward movement is blocked – e.g., when the gerund is inside an island – NOC should emerge and with it, WCO effects (see his ex. 138, 139). The fact that NOC "emerges" is of course not surprising – it emerges already in the basic paradigm, as shown above, when the gerund is not inside an island. As to the WCO effects – the judgments are not clear-cut at all. Native speakers find the following example acceptable (i.e., PRO-gate applies even to gerunds inside islands).

(53) The fact that PRO₁ losing his₁ life is a distinct possibility frightens every soldier₁.

Finally – perhaps the deadliest blow to this analysis – PRO-gate effects are attested even in environments of arbitrary control (J. Bobaljik, p.c.). The judgments below were confirmed with several native speakers.

- (54) a. PRO_{arb} calling him₁ an idiot would upset any/ ?every professor₁.
b. PRO_{arb} overcooking his₁ lunch would make any/ ?every pupil₁ cry.

Observe that the deduction in (41) presupposes that for PRO to act as a gate, it must be locally controlled by the operator that binds the pronoun; only if it is so controlled can it be even contemplated that PRO is a residue of prior A-movement of the operator. But clearly, arbitrary PRO is not controlled by any local DP, hence could not be an A-trace. The fact that it still displays the PRO-gate effect eliminates the entire motivation for the analysis.

To summarize, Hornstein's argument that PRO-gate can be explained by sideward A-movement is seriously flawed. First, PRO in subject gerunds systematically displays NOC, as a careful consideration of the data reveals. On Hornstein's analysis, NOC PRO is a small *pro*, hence it is incorrectly predicted to trigger WCO violations. But even if PRO in subject gerunds had been a genuine OC PRO, the fact is that OC is not a necessary condition for PRO-gate. The PRO-gate puzzle appears more damaging than corroborating to the movement approach to control.

¹⁸ It should be stressed, again, that this conclusion is supported by a vast literature, cited above. Boeckxs & Hornstein (2004) discuss some peculiarities of the specific examples given by Landau (2003), failing to address the general issue. The complex, *non-pronominal* behavior of NOC PRO has been documented in other languages as well (see Babby & Franks 1998 on Russian, Lyngfelt 1999 on Swedish).

10. Wh-infinitives

Another case of OC being misclassified as NOC involves infinitival *wh*-complements like (55a).

- (55) a. John wondered what to do.
b. ? What did John say how to cook?

Hornstein (1999, 2001), following the mainstream tradition, assumed that these fall under NOC; he then derived this effect from the islandhood of *wh*-complements. Specifically, he claimed that since movement cannot cross a *wh*-island, a "last-resort" *pro* is inserted in the embedded subject position, yielding NOC.

Landau (2000, 2003) has shown that the common conception is false – *wh*-complements display OC, specifically, of the partial control type. Thus, they resist long-distance control and truly arbitrary control, force sloppy readings under VP-ellipsis etc. Landau (2003) concluded that "Hornstein must either deny the islandhood of interrogative complements or abandon the link between islandhood and NOC" (p. 483).

Not surprisingly, Boeckx & Hornstein (2004) opt for the former option. They now claim that *wh*-infinitives are "very weak islands" – see (55b) (their judgment); furthermore, they block *wh*-movement due to Relativized Minimality, but whether or not they should block A-movement (which supposedly underlies OC) is unclear.

I find this hedging somewhat elusive; the current position of B&H is very hard to test, especially since the status examples like (55b) is debateable. Still, one can put to test the speculation that infinitival *wh*-islands are invisible to A-movement. Notice that the verb *inquire* is compatible with a *wh*-complement, and with the expletive *there* (in the passive voice).

- (56) a. Someone inquired how to welcome Malay visitors.
b. There were Malay visitors inquired.

However, the two constructions cannot be combined – compare (57a) and (57b).

- (57) a. * There were inquired how to be Malay visitors welcomed.
b. There were likely to be Malay visitors welcomed.

The plausible derivation for (57a,b) would have the expletive merge in the embedded subject position and raise to the matrix one. Then the matrix T establishes Agree with the embedded associate (witness the plural agreement). Alternatively, the expletive may directly merge in the matrix subject position, followed by long-distance Agree.

Crucially, whatever the cross-clausal link is – Agree or Move + Agree – it is of the A-type. And also crucially, this link is allowed to cross a raising infinitive (57b) but not a *wh*-infinitive (57a). Thus, the evidence at hand suggests that *wh*-infinitives *are* islands (and strong ones - (57a) is completely impossible) to A-movement. B&H's speculation cannot be maintained, leaving the initial puzzle unanswered: How can movement (of the subject) escape a *wh*-infinitive for the purposes of OC, but no other movement type may do so?¹⁹

Note, in comparison, that the Agree-based approach to OC (and specifically, to PC) faces no such difficulties. Under this approach, Agree targets that C head of the *wh*-complement, which is accessible to the higher phase by assumption (see the Phase Impenetrability Condition of Chomsky 2000). The fact that the specifier of the complement is filled by a *wh*-phrase is immaterial, since the OC dependency consists of ϕ -features. Nothing else needs to be said about these constructions.

11. Control Across Passive

In Landau (2003) I observed that Hornstein (1999) has no principled way to rule out (58a), given that both (58b,c) are possible. The same type of A-movement allegedly involved in the latter should also be able to apply in the former. I have also shown that Hornstein's (2000) solution is empirically inadequate – predicting, in fact, (58b) to be as bad as (58a).

- (58) a. * John was hoped to leave.
b. John was persuaded to leave.
c. John was expected to leave.

¹⁹ Obviously, solutions can be devised; for example, B&H may suggest that θ -driven movement is subject to different islands than other movements. This will correctly distinguish OC from both expletive raising and *wh*-movement. But the issue is not whether solutions can be devised – they always can – but rather whether they can be substantiated on the basis of independent evidence. Many of B&H's suggestions – e.g. regarding the spellout of binders, the islandhood of *wh*-infinitives, the functional (?) status of raising verbs, the effect of θ -features on equi-distance, etc. – are just that: suggestions, unaccompanied by argument.

Boeckx & Hornstein (2004) retackle the issue, proposing a new solution. They claim that the problem with (58a) is the general inability of *hope* to passivize with a (non-expletive) subject. Thus, they claim, only verbs that license a DP object can passivize; (58a) is ungrammatical because (59a) is, and (58b) is grammatical because (59b) is.

- (60) a. * A victory was hoped.
b. John was persuaded.

B&H's condition may be a necessary condition on English passive formation, but its relevance to the point at stake is unclear. Importantly, it misses the important generalization (so-called "Visser's Generalization") that (58a) only illustrates: *No* subject control verb may promote the embedded subject to the matrix subject position via passivization - not even verbs that *do* passivize their DP objects.

- (61) a. The decision was regretted/hated.
b. They regretted/hated to have passed the decision.
c. * They were regretted/hated to have passed the decision.

This generalization is obscured, in English, by the availability of "passive ECM" variants of subject control verbs. Thus, it might appear that the pair in (62) confirms B&H's hypothesis:

- (63) a. The program was decided.
b. John was decided to be our representative.

However, (63b) is a passive ECM, not a passive control construction.²⁰ This is made evident by its tense restrictions, typical of ECM and absent in control.

- (64) a. John decided to apologize to Mary tomorrow.
b. * John was decided to apologize to Mary tomorrow.

Most languages lack this option, and do not even allow (63b). Furthermore, in languages where unergative verbs form impersonal passives (e.g., Dutch), it seems that Case-

²⁰ Why the active source of these passive ECM constructions is ungrammatical is an old puzzle (see Postal 1974, Pesetsky 1992, Bošković 1997).

licensing a DP object is not a necessary condition on passive; nonetheless, sentences like (58a)/(64b) are consistently ruled out. As far as we know – this is a universal phenomenon. The ad-hoc solution of B&H (like its predecessor in Hornstein (2000)) fails to address this general problem.²¹

12. Sidward Movement from Complements

In Landau (2003) I noted that the option of "sideward movement" – used by Hornstein to explain OC into adjuncts – overgenerates examples like (65).

(65) *John's₁ friends prefer [t₁ to behave himself].

If OC is movement, and sideward movement is allowed, nothing seems to block movement of the embedded subject into [Spec,DP] of a matrix argument, generating nonexisting interpretations.

B&H propose two possible ways to deal with this problem: (i) The DP *John's friends* is derived from a small clause headed by *John*, which functions as a predicate. Semantically, *John* cannot be both a predicate (of *friends*) and an argument (of *behave himself*), hence (65) is uninterpretable; (ii) Possessive DPs are adjuncts, at least in cases like *John's friends*. Movement to adjoined positions is blocked (violating Greed), hence (65) is underivable.

B&H do not offer any independent evidence for either of those sketchy proposals; therefore, I will not even try to consider whether such evidence exists, and if so, what weight it might carry. Instead I will show that here again, B&H's solution is narrowly ad-hoc, confusing one example (which may be solved locally) with a general pattern (which calls for serious rethinking).

Both options (i) and (ii) above can be ruled out if we make sure that the "raised" genitive phrase is a genuine argument of its head noun. Such is the case in (66).

- (66) a. *John's₁ examination of the patient convinced Mary [t₁ to applaud himself].
b. *John's₁ examining the patient convinced Mary [t₁ to applaud himself].

²¹ In Landau (2003) I suggested that (58a) is blocked by the Ban on Improper Movement. In recent terms, this would be covered by the Phase Impenetrability Condition, which makes [Spec,TP] inaccessible under a phasal CP. A simpler view, however, would be that a lexical subject (or its copy) is not licensed to begin with in the infinitival complement of control verbs, because it would fail to check off certain uninterpretable features that characterize T and C in these clauses (see Landau 2004 for details).

Examination and *examining* in (66) are complex event nominals in the sense of Grimshaw (1990), taking an internal agent, namely *John*. There is no reason whatsoever to analyse *John* here either as a predicate or as an adjunct. The fact that the ungrammatical pattern is constant across (65) and (66) indicates that B&H's proposal does not go to the heart of the problem.

13. Novel Raising-Control Contrasts

While the debate about the nature of OC is largely informed by well-known facts, one should not assume that all the relevant facts have been uncovered, even in this well-trodden terrain. The empirical adequacy of the competing views should be continually measured against new discoveries.²² In this section I will briefly discuss two such discoveries, made by Postal (2004, Ch. 2).²³

Postal discusses predicates like *be the matter/wrong with*, whose subject position is antipronominal.

- (67) a. Something₁ is the matter with my transmission, but that sort of thing/*it₁ is not the matter with his.
b. * He said something₁ was wrong with her values, and it₁ was wrong with them.

Interestingly, these two predicates may occur inside raising complements, but not inside controlled clauses.

- (68) a. Lots of things seem to be the matter with your transmission.
b. Such a thing is bound to be wrong with someone's liver.
- (69) a. * Lots of things can be the matter with your transmission without being the matter with mine.
b. * That can be be detectable without being wrong with your liver.

The contrast is explained if PRO is a null pronominal (or reflexive) of sorts, whereas the trace of a raised DP is simply a silent copy of this DP, retaining all its distributional

²² Recently, findings from ERP studies revealed a systematic difference between the syntactic processing of raising and control constructions (Featherston et. al. 2000), in line with the conclusions of Landau (2003) and this paper.

²³ Postal argues for a third one, namely, that subjects of middle predicates can be raised, but not controlled. However, the actual scope, and ultimate source, of this putative contrast are less obvious to me.

properties. Notice that according to Hornstein's theory, OC into adjuncts like (69) is achieved via NP movement, hence the contrast with (68) is unexplained.

A second observation pointing to the pronominal nature of PRO involves antecedence relations between certain DPs and their metonyms. Observe first that pronominal antecedence tolerates certain metonymous shifts but not others.

- (70) a. I am parked on 26th Street (= my car is parked on 26th Street).
b. Microsoft went up (= Microsoft's stock's price went up).

- (71) a. John_i claimed that he_i was parked on 26th Street.
b. *Microsoft_i claimed that it_i would go up.

Thus, for whatever reason, a pronoun may be metonymous to its antecedent in the case of a car-possessor relation, but not in the case of a company-stock's price relation.²⁴ Crucially, now, the same contrast is preserved in control, but not in raising.

- (72) a. John plans to be parked on 26th Street.
b. *Microsoft_i plans to go up.

- (73) a. John seems/is likely to be parked on 26th Street.
b. Microsoft_i seems/is likely to go up.

The fact that (72b) patterns with (71b) and not with (73b) strongly suggests that the null subject of the control complement is more akin to a pronoun than to a copy of NP movement. Postal further shows that a "stock price" PRO *can* be controlled, provided its antecedent is construed as a stock price as well.

- (74) Microsoft went up today after going down yesterday.

Therefore, the problem with (72b) is specifically the metonymous *shift*, which is independently shown to be restricted for pronouns (as well as reflexives, e.g., **Microsoft believes itself to have gone up*). A raising analysis of OC will be hard-pressed to make sense of this pattern of data.

²⁴ Notice that there is nothing wrong about treating companies as agents or attitude-holders; compare (71b) with *Microsoft_i claimed that its_i stock price would go up*.

14. Conclusion

In this paper I attempted to assess the current status of two accounts of control: The movement analysis and the Agree-based analysis. Although situated within the same framework, each account comes with its own baggage of auxiliary assumptions; quite often, the empirical success of the accounts relies on those assumptions, rather than on first principles. It is now worth considering where each account stands.

Consider first the issues raised in Hornstein (2003). As I have argued above, few of the empirical challenges to the movement analysis are genuinely solved. MDP-violations, partial control, the proper delineation of NOC and case-independence in OC reveal serious shortcomings of the analysis. The PRO-gate phenomenon, I maintained, lies entirely beyond the explanatory capacity of Hornstein's theory (in its present form). Moreover, the responses of Boeckx & Hornstein (2004) to the issues raised by Landau (2003) leave much to be desired. On many points – blocking passivization of subject control verbs, blocking sideward movement from complements, the treatment of implicit control and of OC into *wh*-infinitives - the solutions BH provide are extremely narrow in scope, failing to address the deeper generalizations.

Finally, novel facts, that came to light recently, point to a referential distinction between PRO and NP-trace, which is also congenial to the traditional view but extremely puzzling under the raising analysis.

Remarking on the current success of movement-based analyses, Hornstein (2003) writes that “what I find interesting is not that they indeed have empirical weaknesses, but that there are not more of them” (p. 63). That is, indeed, a question of judgment; I find the above list of problems quite worrisome.

Compare now the Agree-based approach. First, I argued above that this analysis is free of many of the ailments attributed to it by Hornstein; in some cases (i.e., semantic plurality in PC) the objections have no force; in others (e.g., T-to-C) minimal modifications, of the sort developed in Landau (2004), remove the difficulties; and yet in others (e.g., finite control), a closer consideration of the full picture reveals a clear advantage for the Agree-based analysis over the movement analysis.

The Agree-based analysis of OC is of course not without problems. As far as I can see, two such problems emerge from the debate. First, the existence of split control. This, I think, is an outstanding problem for both sides of the debate, whether acknowledged as such or not. Second, the phenomenon of backward control. If not misanalysed (see the skeptical comments in section 7), then backward control represents a challenge to any

non-movement approach to OC. Hopefully, more research into this phenomenon will be available to inform future theoretical attempts to deal with it.

Thus, on the empirical front – which is the decisive one, at least for me – it seems fair to say that the movement analysis of OC still has a long way to go to reach the empirical adequacy of its rivals, the Agree-based analysis included. The conclusions of Landau (2003) do not call for revision.

On the meta-theoretical front, I suppose that Hornstein still believes that the balance is reversed. However, our perspective now is clearer than it was a few years ago. The theoretical apparatus constructed in Hornstein (1999) was indeed very tight and elegant. The only problem was – it could barely begin to explain the wealth of facts involved in control. Hornstein (2003) and Boeckx & Hornstein (2004) are (laudably) much more empirically responsive, addressing a variety of factual issues. The inevitable price is, however, a considerable enrichment of the sparse theoretical toolbox afforded by Hornstein (1999). Novel mechanisms abound, none of which follows from the bare foundations of the movement analysis: Logophoric PRO is allowed in NOC, a new spellout condition is imposed on binders, a meaning postulate for PC is proposed, stipulations about case in finite control are introduced, speculations about *wh*-islands are entertained, etc.

All of these are, of course, natural developments. No empirically responsible theory can hope to do with the unreasonable a priori strictures imposed in Hornstein (1999). But now, I think, the claim for theoretical elegance and economy, repeated in Hornstein (2003)) and Boeckx & Hornstein (2004), is far less convincing. Supplemented by quite a number of auxiliary assumptions, the movement analysis of OC does not seem *obviously* simpler (more elegant, less redundant etc.) than its rivals. The fact is – no one can tell for sure. Meta-theoretical comparisons, so revered in certain linguistic circles, can be easily pulled towards whatever direction suits one's taste; worse, they can never be objectively settled. Which leaves us with the good old adjudicator – empirical adequacy. And his verdict, I believe, is quite clear in this case.

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