

Remarks and Replies

Reply to “Control Is Not Movement”

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In this reply we examine Culicover and Jackendoff’s (2001) arguments against syntactic treatments of control, and against Hornstein 1999 in particular. We focus on three of their core arguments: (a) the syntactocentric view of control; (b) the control pattern found with *promise*; and (c) the violability of the Minimal Distance Principle. In all cases we contend that Culicover and Jackendoff’s claims fail to undermine Hornstein’s proposal.

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1 Introduction

In a recent contribution to this journal Culicover and Jackendoff (2001; hereafter C&J) take issue with syntactic treatments of control; in particular, they argue against Hornstein’s (1999) proposal that control be regarded as a special instance of raising.

In the present reply we would like to highlight some of C&J’s core claims and contend that they fail to undermine Hornstein’s proposal.

We focus here on three issues: (a) C&J’s argument against a syntactocentric account of control, (b) their argument against a reduction of control to movement based on *promise* and the (apparent) violability of the Minimal Distance Principle imposed on control; and (c) asymmetries between nouns and verbs in control possibilities.

2 Syntactocentrism

Let us start with the most important theoretical point. Although C&J take Hornstein 1999 as their object of criticism, this was merely a target of convenience. The movement approach to control developed in Hornstein 1999 and 2002 is but one species of a far larger genus: the view that control properties largely reflect structural properties of the syntax. C&J’s objections are not limited to movement theories of control but extend to virtually every syntactic treatment. In place of syntactic structural restrictions on control, C&J propose that control be coded in terms of

We would like to thank Amy Weinberg and the editors at *LI* for a variety of helpful comments. We would also like to thank Ray Jackendoff for comments on an earlier draft. Though we are quite sure that our reply will not satisfy him, his comments provoked us to recast some of our points.

thematic structure. However, what they provide is essentially a list of controllers coded by thematic role: some verbs are agent control verbs, others are patient control verbs, still others are source control verbs, and so on. This reduces the theory of control to a lexical catalogue. Consequently, on methodological grounds one should adopt their view only as a last resort.¹ Note that this does not mean that C&J are wrong. Maybe a list (perhaps structured by some local redundancy rules dependent on thematic characterizations of the sort C&J explore) is the best one can do. However, because in our view syntactic treatments of control, starting with Rosenbaum 1967, have led to numerous insights, we favor approaches that remain within the syntactocentric tradition of which Hornstein 1999 is but one example.²

It would be useful here to review what we take to be some of the main virtues of the syntactocentric approach. In this section we concentrate on two features: adjunct control and the fact that controllees are syntactic subjects.

First, as documented in Hornstein 2001, adjunct control cases like (1) exhibit *all* the properties of obligatory control: the controlled element must have an antecedent; the antecedent must be local; it must c-command the controlled element; obligatory control allows only sloppy readings under ellipsis; it disallows split antecedents; and it has only the *de se* interpretation.³

(1) John saw Mary [before leaving the party].

This is totally unexpected under C&J's view, for adjuncts are by definition unselected, athematic material.⁴ That they show obligatory control properties is a serious problem for an approach to

¹ Ray Jackendoff (personal communication) denies that C&J's solution to the control problem 'hang[s] on random lexical classes.' Rather, he states that it follows from the meaning of the control verbs at issue. We do not claim that C&J's work is without interest. However, where they see principled classifications, we see coding of the facts in other terms; in other words, we would describe their efforts as coding rather than explaining the facts at issue.

² It is worth repeating here that Hornstein 1999 and 2001 represent just one of many recent attempts to think of control in terms of movement. Other recent proposals include O'Neill 1995, Martin 1996, and Manzini and Roussou 2001. The earliest version of such an approach is Bowers 1973.

³ For reasons of space we do not provide the supporting data here. These can be found in Hornstein 1999, 2002: 46–49.

⁴ C&J discuss adjuncts and their control properties and conclude that 'some syntactic constraint seems unavoidable' (p. 502). Where we differ from C&J is in our belief that this observation has serious theoretical ramifications. This is why. It seems that the syntactic constraints required for adjuncts extend quite easily to accommodate cases of complement control (as Rosenbaum (1967) originally observed). If so, thematically based approaches to complement control like C&J's *when added to the syntactocentric ones required for adjunct control* introduce substantial redundancy and add little in the way of explanatory power. In short, *if*, as C&J observe, adjuncts require syntactocentric devices and *if* these devices extend naturally to the core complement cases, then the empirical relevance of thematically based accounts of control is substantially curtailed.

It should be noted that C&J's discussion focuses on purpose clauses. In this discussion they seem to suggest that the thematically based approach might extend even to these cases, as purpose clauses show some 'nonsyntactic influence' (p. 503). We are not quite sure what C&J intend here. No one would deny that extrasyntactic factors play a role in interpreting control constructions, and all can agree with C&J's claim that '[s]ince *in order to* denotes a purpose, it must be controlled by an individual capable of having a purpose' (p. 503). However, it is unclear to us what this 'deeper semantic influence' has to do with *thematic* structure. We take the latter to be some (perhaps elaborated) form of argument structure, not a cover term for whatever effects meaning may have on some linguistic phenomenon. The problem with adjunction is that it is not related in any obvious way to *thematic* structure and yet it displays the same properties as thematically dependent cases of complement control. We take this fact to be very significant. Why, after all, if control is largely thematic and only incidentally syntactic, should complements (domains that are thematically accessible) and adjuncts (domains that are thematically inaccessible) display identical properties? This said, the reader should not think that C&J are unaware of the challenges that adjuncts pose.

control that sees it as essentially a form of thematic dependence (i.e., an approach like C&J's). However, as Rosenbaum (1970) noted early on, if control is sensitive to a structural principle akin to the Minimal Distance Principle, this is what we expect.

Hornstein (1999, 2002) presents an alternative analysis of adjunct control (in terms of inter-areal movement), one that adheres to the spirit of Rosenbaum's (1970) proposal in being syntactocentric. As such, in contrast to a thematically based theory such as C&J's, it is comfortable with the fact that obligatory control phenomena appear in domains beyond the reach of thematic dependencies.

One last point. Note that the choice of controller in (many of) these cases is not thematically restricted in any way.⁵ In (2), for example, *John* need not be a thematic dependent of the predicate modified by the adjunct. Instead, the adjunct can be interpreted as modifying the matrix clause (i.e., the believing), and *John*, its *syntactic* subject, is understood to be the controller.

(2) John was believed to be likely to be elected before giving that awful speech.

Let us now turn to a second virtue of syntactocentric theories of control. Any adequate theory of control must address at least two questions: what sorts of elements can be controllees (the distribution-of-PRO problem)? and what elements are controllers (the interpretation-of-PRO problem)? So far as we can tell, most linguists would agree that the first problem is largely a syntactic one (see, e.g., C&J 2001:sec. 2). There are various ways of implementing this syntactically (null Case, locality, selection, etc.), but given that controllees are always subjects with no apparent thematic restrictions, there has been unanimous agreement that the distribution of PRO is ultimately a syntactic issue.

As noted, C&J agree with this, for they couple their thematic theory of the *controller* with a selection account of the *controllee* and adopt the more or less conventional description that thematically driven controller selection extends only into nonfinite complements, namely, gerunds and infinitives (see C&J 2001:sec. 2).⁶ What C&J do not take into account, we believe, is the degree to which solving the distribution-of-PRO problem also solves the interpretation-of-PRO problem.⁷ A virtue of the movement theory of control, perhaps its most alluring property, is that it completely accounts for the *distribution* of (obligatory) controlled PRO. In effect, one finds

⁵ C&J note a semantic restriction on purpose clauses, though not necessarily a thematic one; see footnote 4.

⁶ There appear to be cases of obligatory control that do not fit this textbook description. For discussion of a particularly interesting case see Rodrigues 2000 and Ferreira 2000.

⁷ So far as we can tell, for C&J these two problems are unrelated. This is the traditional view, which a movement approach to control challenges. Note that separating these two issues raises an interesting question for theories like C&J's: why is it that control clauses have the structures they do? Why doesn't control reach into finite complements, for example, or into nonsubject positions? Is there really anything that would semantically restrict such dependencies from the broad thematic perspective that C&J adopt? To make matters concrete: if *persuade* requires object control for thematic reasons in (i), why does it not impose similar interpretive restrictions on the pronoun in (ii)?

(i) John₂ persuaded Mary₁ PRO_{1/*2} to leave.

(ii) John₂ persuaded Mary₁ that she₁/he₂ should leave.

Of course, one could stipulate that thematic control only applies if there are control complements. However, this is to redescribe the facts, not to explain them. If (obligatory controlled) PROs are essentially NP-traces, these questions have a principled answer—a virtue of a movement-based approach to control.

such PROs in precisely the configurations allowed by movement.⁸ PROs occur in the subject position of nonfinite clauses for the same reason that NP-traces occur in these positions in raising constructions. The reason that control cannot extend into objects or indirect objects of transitive verbs is that movement from these positions (over intervening subjects) is prohibited (whether by minimality or some other condition is of no consequence here). In effect, once one assumes that controlled PRO is actually an NP-trace, then *why* it appears *where* it does is straightforwardly accounted for. However, and this is the important point, once one takes this sort of approach to the distribution problem, one has committed hostages to the interpretation problem: the antecedent (controller) is the DP that has moved from the trace position.

None of this implies that the movement theory is correct. However, it does highlight something that C&J's reply does not sufficiently focus on (in our view): that the movement theory has a ready account for the distribution problem (a problem, recall, that most linguists would agree to be syntactically based and that all approaches must address) and that this lends considerable *independent* support for this sort of analysis.⁹

Last point. Just as C&J do not doubt that syntax is required to account for *some* of the properties of control (i.e., adjunct control and the distribution of PRO), so we do not deny that thematic information is relevant to control. In fact, the checking of an additional θ -feature is precisely what under Hornstein's theory distinguishes control from raising. The question is not whether thematic properties and semantic constraints play a role, but how they do so in the context of a theory that accounts for both the distribution and the interpretation of PRO. An objection to C&J's account, from our perspective, is that it does not take into account how much the two problems are interrelated. Once this is acknowledged, the scope for a thematic theory of the kind they suggest should, in our view, be severely restricted.

In short, while we do not deny that semantic considerations are likely part of an overall account of control, we would argue that there is more to the phenomenon of control than a list of thematically coded predicates capable of entering the "control frame."

3 *Promise*

Among the insights gathered over the years about control is that it is subject to a Minimal Distance Principle that demands that the controller be the closest NP to the controlled position (Rosenbaum 1970). This generalization follows immediately from a movement approach to control, since movement is constrained by a shortest/closest requirement; see (3). In (3) the trace of DP₁ is too remote from its antecedent, in violation of minimality, be it phrased as Shortest Move, Shortest Attract, or the Minimal Link Condition. Thus, Rosenbaum's (1970) principle (i.e., axiom) of Minimal Distance is derived (i.e., is a theorem) in the context of a movement theory of control.

⁸ In what follows we concentrate on the case of intraarboreal movement. However, we think that a very strong case for interarboreal movement is precisely the fact that the distribution of obligatory controlled PRO within adjuncts is the same as its distribution within complements.

⁹ We also believe that the movement approach provides the most *principled* account for the distribution of PRO, at least when compared with current favorites such as selectional stipulation (e.g., C&J 2001) or null Case (e.g., Chomsky and Lasnik 1993).

(3) [_{TP} DP₁ [_{VP} V DP₂ [_{TP} PRO (= t₁). . .]]]

Although we take this to be a virtue, C&J (see also Landau 1999) raise it as an argument against movement theories in general and Hornstein 1999 in particular. To do so, they capitalize on the old observation that *promise* and a handful of semantically related verbs like *vow* and *commit* require subject control.

(4) John promised Mary [‘‘John’’/*‘‘Mary’’ to leave].

By virtue of being transitive verbs, such predicates are expected to manifest object control given the Minimal Distance Principle/Shortest Move. In short, the methodological virtue of deriving Rosenbaum’s generalization (highlighted above) is actually a serious (and deep) flaw given the existence of subject control verbs like *promise*, or so C&J argue.

We agree with C&J that *prima facie*, *promise*-type predicates pose an apparently serious problem for a movement approach to control, since this approach grounds the Minimal Distance Principle in something as deep as locality conditions on movement. However, we believe that the properties of *promise*, when considered in their entirety, argue in favor of theories that incorporate some version of Rosenbaum’s minimal distance condition (and so, a fortiori, in favor of theories that can derive this principle) and against those, like C&J’s, that do not adopt (or require) a principle like Rosenbaum’s and (over?) regularize the properties of *promise*. Let us explain why.

As early as 1969 Carol Chomsky noted that children show a significant delay in acquiring the control properties of *promise*. This, she argued, suggests that the pattern seen in (4) is marked. Following her observations, we can ask what it is about *promise* that leads to its late acquisition.¹⁰ At the very least, the logic of markedness implies that the control found in *promise* constructions should not be taken as the central case of control. In fact, it suggests that *promise* calls for special theoretical treatment.

This is very good news for any syntactocentric approach to control that incorporates something like the Minimal Distance Principle (MDP). A movement-based approach to control offers a basis for explaining the acquisition facts discovered by Chomsky (1969), as within such an approach the MDP follows from Shortest Move/Attract. The reason that *promise* is marked (and hence acquired late) is that it appears to violate Shortest Move! Of course, if the MDP (and so, Shortest Move) is irrelevant to control as C&J suggest, a big empirical problem arises: how to account for the marked status of *promise*. Thus, correctly understood, the refractory behavior of *promise* argues both in favor of the movement theory and against C&J’s alternative.

¹⁰ In fact, it appears that some adults never master the subject control properties of *promise*. Courtenay (1998) notes that in an informal survey, 52 of 84 native speakers of English rejected a subject control reading of (i) and treated *Kris* as the one who would buy the cat food.

(i) I promised Kris to buy the cat food.

If these numbers are representative, then well over half the native speakers of English do not allow a subject control reading for *promise*. If accurate, this observation clearly strengthens the point we make in the text.

In sum, no matter how one eventually treats *promise*, one must make sure that it's not just another regular control predicate. *Promise*, and the apparent violation of the MDP that it entails, therefore turns out to be an argument in favor of a movement-based approach to control.

For the purposes of this reply, then, it is enough to note with Carol Chomsky that *promise* is odd and that taking it to be the core phenomenon of control is unwarranted.¹¹ Having said this, we want to suggest a concrete way in which *promise* may be special (our suggestion draws on Hornstein 2001, 2002).

We would like to propose that what makes *promise* special is the existence of a (possibly optional) null preposition heading the object. It is the existence of such a null preposition (P_{null}) that is hard for the child to detect. Note that P can be overt with some semantically related predicates, as in (5b), which lends some plausibility to (5a).

- (5) a. John promised [P_{null} Mary] [to leave early].
 b. John vowed/committed [to Mary] [to leave].

Note that once P_{null} is posited, *promise* complies with the MDP/Shortest Move under some view of locality.¹² In essence, P_{null} renders *promise* similar to raising predicates with an experiencer, like those in (6) and (7) (where a null preposition has often been appealed to).¹³

- (6) John seemed to Mary t to be tired.
 (7) John struck [P_{null} Mary] t as tired.

This is not the place to delve into the details of movement and locality conditions required to render raising licit in (6) and (7) (see Boeckx 1999, 2002, for review). Suffice it to say that P_{null} eliminates the object as a potential intervener for movement.

¹¹ This point is important. The cursory proposal offered below provides a mechanism that, if correct, would allow subject control with *promise* without violating the MDP. However, our reply to C&J's comments is completely adequate even if this proposal is incorrect. In fact, as far as the logic of our point is concerned, it would be perfectly satisfactory to adopt the mechanism for exceptions detailed in Lakoff 1971. C&J (2001:498 fn. 9) state that such solutions "are no longer considered appropriate, and with good reason." However, this statement is incorrect. A Lakoff-style approach to *promise* is superior to one that treats it as regular (as, for example, C&J suggest their approach would do) given the irregular acquisition profile that it displays. This does not imply that a Lakoff-style treatment is correct. It may well be that what allows *promise* to have subject control where it does for those speakers amounts to more than lexical stipulation. However, this is an empirical matter, and there is nothing in principle wrong with a Lakoff-style approach to these sorts of marked cases.

¹² In particular, it will allow the subject of the embedded clause to raise across the object of the preposition without violating Shortest Move/Attract. Note that this does not imply that DP objects of prepositions cannot be controllers.

¹³ It is interesting to note in this respect that the grammatical status of the French equivalent to (6), given in (i), is far from clear. Like English speakers asked to make judgments about sentences with *promise*, native French speakers appear to have conflicting intuitions about (i) (see also Chomsky 1995:388 n. 79).

- (i) Jean semblait à Marie être fatigué.
 Jean seemed to Marie to-be tired

4 Nominals

With the status of *promise* clarified, we now turn to the third, and in our view most interesting and challenging set of issues raised by C&J against a movement-based (or any syntactocentric) approach to control: the case of control in nominals. Given limitations of space (and competence), we will not offer reanalyses of C&J's proposals. Our aim is more modest: to suggest some general strategies for approaching the data that C&J outline and to soften C&J's conclusion that such data clearly imply the inadequacy of syntactocentric approaches.

Before we get into any details, it is worth noting that C&J appear to agree that structural approaches (and maybe even a movement approach) to control may well be (more or less) adequate (modulo the *promise* cases) for the standard control configurations involving verbs. They believe, however, that this approach cannot be extended to analogous cases within nominals. Their main point seems to be that the well-rehearsed syntactic differences between nouns and verbs need not get in the way of generalizations regarding control if syntax-centered approaches to control are replaced with thematic ones, as the latter exploit relations that are stable across verbs and their nominal counterparts. Thus, what makes a thematically based theory superior to a syntactocentric one is that it can abstract away from the obvious syntactic differences between nouns and verbs and still capture their common control properties.

We believe there is at least one potential problem with this view of thematic structure. The differences between nouns and verbs might extend beyond syntax to the thematic domain as well. Thus, the thematic behavior of nouns is not identical to that of verbs.¹⁴ For example, it is well known that *by*-phrases within nominalizations enforce an agentive reading that is absent from their verbal counterparts.

- (8) a. The house was encircled by the trees.
 b. the encirclement of the house by the trees

(8a) has two readings. In the more familiar one, the trees are in a certain position with respect to the house. In the other, the trees have done something; they have moved to surround the house. Curiously, (8b) only has the latter reading. This nominal denotes the latter event, not the former state. The complement of the *by*-phrase within the noun must be agentive. The one inside the verb need not be.

Nouns and verbs display other thematically related differences as well. Consider two more. First, as is well known, the complements of nouns are (more or less) optional, whereas those of verbs are not. This is one of the facts about nominals that has led many to conclude that the "arguments" of a noun are in fact adjuncts, rather than complements (see, e.g., Grimshaw 1991, Zubizarreta 1987). This suggests that the thematic relations (or the realizations of thematic relations) within nouns are different from those found with verbs. But if this is the case, then a

¹⁴ It might be more accurate to say that the realization of thematic information in nouns is quite different from that in verbs.

thematically based theory should say why it is that this difference makes no difference when it comes to control. After all, many thematic properties found with verbs are not found with their nominal counterparts. Why are control properties immune from this difference?

This question becomes more pressing when we consider a second disanalogy between verbs and their nominal counterparts. Idioms are not found in the latter. Consider the data in (9).

- (9) a. John kicked the bucket.
 b. John's kicking of the bucket
 c. John let the cat out of the bag.
 d. John's letting the cat out of the bag
 e. John's letting of the cat out of the bag

(9a,c,d) all allow an idiomatic interpretation. (9b,e) permit only literal readings. It seems that the thematic properties characteristic of verbs do not carry over wholesale into their nominalizations. It is not clear why this happens.¹⁵ However, there must be some difference; otherwise, we would expect perfect symmetry between verbs and their nominal counterparts.

In sum, the presupposition that C&J tacitly exploit (namely, that thematic properties and their realizations are invariant across verbs and nouns) is more complex than might first appear and so does not obviously support the explanatory load they wish it to carry.¹⁶

With these caveats registered, let's consider some examples.

- (10) a. John's attempt [to win]
 b. John's desire [to leave]
 c. John's plan [to write]

The challenge for a movement-based theory of control is to reconcile the possibility of control in nominals with the absence of raising in nominals. Witness (11).

- (11) *John's appearance [to be tired]

If control is raising, how can control take place, but not raising?

¹⁵ One possibility that comes to mind—namely, that Case is the relevant difference—is likely incorrect. Note that even in nominals that have no overt complements (and so where Case is irrelevant), idioms are banned. (i) can mean that Harry has succeeded. (ii), the nominal, does not refer to Harry's success.

- (i) Harry has finally arrived.
 (ii) Harry's long-awaited arrival

This suggests that the problem is not simply Case-based, but relates in some unknown way to thematic issues. We are grateful to Hagit Borer for the *arrive/arrival* example.

¹⁶ One might simply stipulate that control is exclusively dependent on thematic structure and that is *why* the syntactic difference between nouns and verbs leaves control properties unaffected. However, this is not entirely correct even in C&J's framework: for example, control is operative only in certain syntactic configurations. But suppose it were true. Such a claim would not explain why control properties, in contrast to other thematic properties, are unaffected by the syntactic frames that express them. Why is control so special?

We see two ways to tackle the problem. One is to deny the existence of control in (10). If such cases do not fall under the purview of control, they pose no problem for an approach like Hornstein's (1999). The other is to bite the bullet and claim that there is movement (and control) in (10), and find an explanation for why standard raising is blocked in (11). We briefly outline each possibility.

Let us start with the "negative" approach to (10): denial of the control facts. The essence of our suggestion goes back to Williams's (1980) argument that control in nominals is a case of nonobligatory control. Thus, Williams observes that the controlled element within nominals does not require a syntactic antecedent. Witness (12).

(12) any attempt [to leave]

In addition, control in nominals supports arbitrary readings, as in (13a), in sharp contrast with control in the verbal domain (13b).

- (13) a. any attempt to conceal oneself
 b. *It was attempted to conceal oneself.

Hornstein (2002) also notes that split antecedents are possible with control in nominals, but not in sentential domains.

- (14) a. John approved Bill's initial/regular attempts to sneak each other/themselves into the party.
 b. *John approved of Bill's initially/regularly attempting to sneak each other/themselves into the party.
 c. *John said that Bill attempted to sneak each other/themselves into the party.

Finally, control in nominals allows for strict readings under ellipsis, unlike control in the sentential domain. (That is, (15) can be understood as talking about Bill's attempt to get John to sneak himself into the party.)

(15) John's attempt to sneak himself into the party was not as clever as Bill's.

(16) John tried to win and Bill did too.

The facts just reviewed suggest that we are dealing with instances of nonobligatory control inside nominals. If correct, this conclusion voids C&J's argument against Hornstein's (1999) account. For the latter, nonobligatory control is a nonmovement dependency involving a null pronominal element akin to *pro*. In other words, the control facts in nominals do not show the characteristics that motivated a movement approach to control.

However, we still lack an explanation for the strong tendency of the controlled elements in (10) to take a syntactic antecedent (to behave like an obligatorily controlled PRO). Hornstein (2002) speculates that this tendency may follow from interpretive peculiarities of genitive subjects inside DPs (for discussion see Hornstein 2002).

Now consider another alternative approach. Suppose we view the strong tendency for the controlled elements in (10) to take a syntactic antecedent as indicating that obligatory control is

available inside nominals. The peculiarity of nominals, then, would be that they allow for both obligatory and nonobligatory control (for reasons yet to be elucidated). This would mean that some kind of movement is possible inside nominals. To be tenable, the analysis must also explain why raising is not permitted in (10).

The answer to this apparent paradox may lie in the driving force for movement and the features available inside nominals. Suppose that A-movement is Case-driven, a possibility entertained in many circles. It is widely believed, since Chomsky 1986, that Case inside nominals is inherent. If we construe inherent Case as θ -related (see Chomsky 1986; in minimalist parlance, as a reflex of θ -feature checking), the possibility arises for A-movement to take place inside nominals only if it is θ -related. This is precisely the difference between standard raising and control (as raising). In the latter case movement takes place to a θ -position (followed by movement to Spec,TP in the sentential domain, a functional layer that is standardly assumed to be absent in nominals). By contrast, standard raising is never θ -driven, hence is absent from nominals. In sum, the absence of structural Case in nominals blocks standard raising,¹⁷ but movement as control may take place, since θ -features (and inherent Case) are available.¹⁷

We have (at most) alluded to two possible approaches to the control facts within nominals. We believe that each can be developed more fully and might suffice to accommodate the sorts of facts that C&J have highlighted.¹⁸ However, the judicious reader should not conclude that we have answered C&J's provocative points. Doing so would require fully developing a theory of control within nominals, and we have not done that. The main point we want to make here is that control within nominals is not necessarily incompatible with a movement-based approach to control. One may choose to cast doubt on whether the facts have a bearing on obligatory control (and movement), or one may capitalize on the featural difference between the nominal and verbal domains to reconcile control as movement with the traditional claim that raising is absent from nominals. Either way there are still many facts to be explained and C&J's remarks serve as a timely reminder of the complexities involved.

This said, we believe that the control properties found within nominals are part of a more general research agenda that attempts to determine the similarities and differences between nouns and verbs. There is a growing consensus in the literature that nominals bear some structural affinity with their verbal counterparts (an intuition we share); but the extent to which they diverge is not entirely clear, and why they do so still remains mysterious. It is not implausible to think that once those differences are understood, the nature of control inside nominals will be revealed. If so, it would be premature, in our view, to reject a movement-centered (or any other syntactocentric) approach to control on the basis of current, very incomplete understanding of nominalizations.

¹⁷ Hornstein (2001) offers a version of this sort of theory but couched in terms of sideward movement. It derives the possibility of control and the absence of raising inside nominals in terms of greed.

¹⁸ To repeat, see Hornstein 2002 for some elaboration of the former proposal.

5 Conclusion

We would like to conclude by emphasizing that the movement-based approach to control, like any other hypothesis, is bound to meet with difficulties. Hornstein (2002) reviews the major obstacles that have been noted in the literature. We have concentrated here on arguments offered by C&J. We have argued that many of the objections they raise do not stand up to closer scrutiny. Some of them, we have argued, provide arguments in favor of a movement (and syntactocentric) approach. Others, we have suggested, are not obviously incompatible with it. We think that structural (syntactocentric) approaches to control in general and the movement approach in particular have led to considerable insight and that the objections advanced by C&J are not sufficiently compelling to warrant abandoning this approach yet.

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