ONE MORE TIME

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Hornstein (1998) argued like Brody (1995) that a theory with both chain and move is redundant, one of them should be eliminated. He presented two arguments that the concept of A-chains should not be part of the grammar. In my comments (Brody 1999) I showed that the arguments for this way of eliminating the redundancy are flawed and that their background is dubious. Hornstein in his reply (2000) disagrees, but as set out below, without valid argument. His reply further discredits in various ways the position I reacted to.



1. The Non-argument

Hornstein 1998 argued for eliminating chains rather than move by pointing out that in a sentence like (1) with a hypothesized structure essentially like (2), the interpretation on which *someone* binds (takes scope over) *himself* and *every report* takes scope over *someone* is impossible.


(1) Someone seems to himself to be reviewing every report.

(2) [Someone] seems to **himself** to be [every report ]  [someone] reviewing

[every report]


He claimed that this does not follow if chains are present at LF. In the chain structure in (2) a chain member of *someone* in the lower clause (perhaps in spec-V) can be lower than a chain member of *every report* (also in the lower clause, perhaps in spec-AgrO); and at the same time the higher (audible) chain member of *someone* (in matrix subject position) can bind the anaphor *himself* (can

take scope over it).  On the other hand, Hornstein argued, if at LF only a single chain member can be present then this interpretation is never available, *someone* is either in the higher or the lower of its possible positions indicated in (2), so it either scopes over *himself* or under *every report* but never both.

In Brody 1999 I noted that there is in fact no real argument here against the concept of chains. Deleting chain members is an operation that ensures this result in a redundant fashion, which follows straightforwardly from minimal assumptions about the interpretation of scope. In particular, it will be impossible to interpret the scope of a quantifier in (2) from more than one of its chain positions since this will lead to infinite regress: in (2): *someone* (from its higher position, from where it is supposed to bind *himself*) would have to take scope over *every report* and *every report* would simultaneously have to take scope over *someone* (in *someone*'s lower position).  In fact even a single quantifier chain is contradictory if the quantifier is taken to scope from more than one position: the quantifier will both scope and not scope over itself. Thus the unacceptability of the relevant reading appears to be a consequence of interpretive considerations. There may at best be an argument here for retaining chains rather than for dispensing with them on the grounds of avoiding redundancy; -- and also, given the undesirability of the mixed theory that assumed both chain and move[1], for eliminating move.

Hornstein 2000 attempts to maintain the argument of Hornstein 1998 claiming that "this kind of reading is perfectly coherent and can be represented as [(3)]"(p.132).


(3) Every report$_x$ [someone$_y$ [y seems to himself  to be reviewing x]]


But (3) does not have "this kind of reading" the reading which, as Hornstein correctly describes in his immediately preceding sentence, is such that "the chain headed by *every report* cannot scope over

the one headed by *himself*, as no part of the former chain c-commands any part of the latter." In (3) *every report* scopes over *himself* but it does not do so in (2). If it did, no problem would arise as (3) shows. So the coherent (3) cannot represent the essentially incoherent reading under discussion.

Hornstein's next sentence introduces another puzzling self-contradiction. Here instead of (incorrectly) claiming that (3) is a possible interpretation of the structure in (2), he says that "[t]he problem is that [(2)] cannot be interpreted as [(3)]..." and suggests again that such an interpretation would be possible (undesirably) if chains were made use of at LF. But this is a strange point. Given Hornstein's assumption (which I share here for the sake of argument), that quantifier scope is essentially clause-bound, the interpretation indicated in (3) is obviously not accessible for the structure in (2). But whether it is or is not is irrelevant, since the coherent (3), as we have just seen, does not represent the reading of (2) where relative scopes of the quantifiers are determined in positions inside the lower clause and one of the quantifiers is then expected to take scope also over an element in the higher clause. Why **this** is not possible has to do with infinite regress, something that the material that Hornstein reacts to covered in detail.

"Apparently Brody (1999) misunderstood the problem with examples like [(1)]" -- according to Hornstein. "Chains that interleave are not in unique c-command relations as part of each c-commands the other. This in combination with [the assumption that a chain C can scope over another just in case some part of C c-commands some part of the other chain] has the effect of allowing an expression $E_1$ to scope over an expression $E_2$ without scoping over the expressions that $E_2$ itself scopes over" , -- an "awkward possibility" (p.132). (For example in (2), reproduced here for convenience with some annotations as (2'), *every report* ($E_1$) can scope over *someone* ($E_2$) without scoping over the expression *himself* that *someone* scopes over.)


(2') [Someone] seems to **himself** to be [every report ] [someone] reviewing

|     |     |     |     |
|-----|-----|-----|-----|
| $E_2$ | $E_3$ | $E_1$ | $E_2$ |
| P'' |     | P | P' |

[every report]

All right. One more time. Brody 1999 pointed out precisely that this problem, "the awkward possibility" is not going to arise. For example in (2') , if *every report* ($E_1$) can scope over *someone* ($E_2$) then *someone* cannot scope over the expression *himself* for the independent reason that this would lead to infinite regress.

In more excruciating detail: $E_1$ (eg. *every report* in (2')) can scope over $E_2$ (*somebody* in (2')) only if $E_1$ has a chain position P that c-commands some member of $E_2$'s chain in P'. (Given Hornstein's assumption about clause boundedness of scope, P , --hence also P'--must be in the lower clause in (2').) For interpretive reasons (of avoiding infinite regress) this entails that within this particular interpretation of the sentence, $E_2$('s chain) cannot take scope from any position P'' that is higher than P. Indeed it is entailed more generally that $E_2$('s chain) cannot take scope from any position other than P', since, as noted earlier, this would lead to contradiction. So again, no "awkward possibility" will arise: since P ($E_1$'s scope position) c-commands P' ($E_2$'s scope position), there cannot be an element $E_3$ (like *himself* in (2')) such that $E_2$ (from P') but not $E_1$ (from P, where P as just shown, must c-command P') c-commands $E_3$. Hence no such interpretation needs to be excluded redundantly, by eliminating chains as Hornstein proposes. [2],[3]

2.Control.

In Brody 1999 I pointed out that "[t]here are difficulties with considering [Hornstein's treatment of the difference between control and raising] as an argument against chains. ...[It] is better interpreted

as an attempted defence of his alternative approach to control...." (p.221)  I referred here to the contrast between (4) and (5);--*someone* can scope under *every report* in (4) but not in (5).[4]


(4) Someone seems (to John) to be reviewing every report

(5) Someone hoped to review every report


I pointed out that standard treatments of raising and control account for this data, given the assumption that reconstruction is chain-internal and the control structure involves two chains not one, like raising. Hence if  there is a way to treat the data under Hornstein's assumptions that reject A-chains, such a fact by itself is at best a defence of,  but not an argument for, his assumptions.

Hornstein 2000 refers to this remark of mine and appears to agree:  "Brody correctly observes that this in itself, does not provide an additional argument against chains (p.221). However, it was not intended as such." But then he explains: "Rather ...its main purpose is to thwart an otherwise obvious argument for the technical indispensability of chains." (p.135). So it's not quite clear what he has in mind here. Although explicitely he reacts to my comment about his argument based on (4) and (5), when he agrees that he has provided no argument against chains, he seems to refer not to his argument based on (4) and (5), but to his assumptions (especially that the bearers of theta roles are not chains) that are involved in this argument. In effect he seems to grant only that his assumptions (no chains, theta roles are features) do not give evidence for themselves.

But the point I made was that, contrary to what he wrote, his argument based on (4) and (5) did not give evidence for his assumptions. To this he has not reacted, except perhaps by not remembering that he intended to give an argument[5] and implicitely towards the end of his reply where he rejects his argument and the data on which it is based (on this see section 3. below).

So Hornstein 2000 does not attempt to defend the argument in Hornstein 1998 from control phenomena against A-chains, and suggests that he has not made this argument. His arguments against A-chains clearly reduce then to the (non)argument reviewed in section 1. above. I see no reason to conclude any differently than I did in my earlier reply: even if their often dubious background assumptions are accepted, both of these attempts to argue for the elimination of A-chains are flawed.

Let me turn to the remaining issue of the status of Hornstein's background assumptions, concentrating on those that Hornstein's reply attempts to deal with.[6] Although I shall consider his comments about his assumptions about control, strictly speaking these cease to be relevant since he now claims not to have provided any argument from control against A-chains in his 1998 paper. (He attempts to construct a different, but I believe equally untenable, argument to the same effect towards the end of his reply, see section 3. below.).

Brody (1999) pointed out also that the similarities in antecedent choice between NP-chains, obligatory control PRO and local lexical anaphors can be treated either in terms of an interpretive construal rule relevant for all three or in terms of reducing the latter two to the A-chain/A-move relation. To quote:

"Hornstein ([1998]) argues for the latter solution on the grounds that it captures the similarities in antecedent choice. But for quite some time now, the problem has not been to find a way to capture the similarities but to decide which of the two available ways of doing so is right (if either). Since it is enough to consider OC PRO to be the same type of element as an NP-trace, say an anaphor, for binding-control theory (cf. eg. Brody 1985), it is not necessary to fully assimilate OC PRO (and local lexical anaphors) to chain/move relations. In order to establish a case for OC PRO being just an NP-trace, it would be necessary to argue that this way of capturing the similarities in antecedent choice is preferable to its alternative(s)" (p.217).

Hornstein (2000) does not question this overtly. However he still seems to occasionally misunderstand the point. At some junctures he seems to think that this is about the difference "that Brody appears to prefer a reduction in terms of construal processes whereas I [=Hornstein] prefer a reduction in terms of movement" (p.135) But the point is not about what he or I or anyone might prefer. It is about Hornstein's (1998) apparent assumption carried over to parts of his reply, that there being similarities in antecedent choice between control and raising entails that they must be treated as aspects of the same entity (movement), --instead of the similarities analysed as covering more than one type of structure on the basis of some underlying common property.[7]

Also, Hornstein's reply continues to talk about "most of the distinctive properties" and "core properties" of the structures and objects to the comment in my reply that the relevant properties reduce to antecedent choice: "Contrary to Brody's suggestions this involves considerably more than antecedent choice...it accounts for example for ...split antecedents,...de se interpretations,...sloppy readings" (p.135) But Hornstein does not explain on what grounds he wishes to construe the notion of antecedent choice in such a way that these matters do not fall under it. The point remains: obligatory control and A-chains share properties relating to antecedent choice but do not share others, like for example thematic or Case structure. Capturing their similarities by taking them to be the same object needs more argument than just observing that they are partially similar.[8]

Interestingly, a paragraph later Hornstein appears to realize that the evidence he has given for the particular account of the similarities he proposes does not suffice and here he begins to try to provide some relevant considerations. He now suggests that "there is a short conceptual reason for preferring a movement approach, all things being equal, especially in the context of assumptions outlined by Chomsky". This is that the relation between the antecedent and the PRO/anaphor is not simply formal, the referential value of PRO/anaphor being dependent on that of the antecedent. "However this makes little sense for NP-traces" (pp.135-136). So the similarity, which involves this

formal relation should be captured within the syntactic system ("derivationally" for Hornstein) where formal similarities between semantically diverse expressions are expected. "... one thing is clear: such a formal relation will resist interpretation as a Bare Output (interface) Condition" (p136).

But A-chain type relations involve theta role transmission, an assumption that Hornstein shares, for it is crucially assumed by the alternative A-chainless treatment he puts forward. So we can take the theta role of the head of the chain[9] to bind the theta role of (or the theta role to be assigned to) the chain-root. It is a small step from here to assume with Williams (1994) that control also involves binding of theta roles. Thus one way to avoid the problem is to assume that theta binding and theta transmission are subject to the same locality requirements (possibly with predication being involved in the account). Theta roles are interpretively relevant entitites hence a requirement that involves binding relations between them should not be problematic in principle. At the same time we can continue to assume that control unlike raising involves two distinct A-chains thereby avoiding the Case and theta theoretical problems elsewhere that the reduction of obligatory control to A-chain type relations would raise. (I discussed some of these in my first reply. See also right below for some more discussion of two of them.)

Hornstein also considers certain parts of two of the arguments I raised in connection with his treatment of obligatory control and local anaphors. One issue revolved around the status of verbs like *expect*. Hornstein's (1998) proposals relating to these matters led him to suggest that in a sentence like (6) *expect* has the option of not assigning accusative Case in spite of it having a thematic subject position, in contradiction with Burzio's well known generalization. I noted in my reply that this assumption would incorrectly predict that (7) is grammatical. I noted also that ECM verbs that violate Burzio's generalization in this way do not exist and that this fact follows immediately if no such verbs exist at all, as in the framework of Chomsky 1982, or that of Brody 1993. Hornstein 2000 did not react to this part of the problem.

I noted next that even assuming the existence of such an ECM verb the question would still remain why the chain reaching the matrix subject forced by the lack of accusative can never involve a single non-root theta position only? "Why is a structure like [(8)], where *expect* is a hypothetical verb with no accusative and a subject theta position, always ungrammatical? Why can't a structure like [(8)] ever express what [(9)] does; why can't (a verb that fits into this construction) exist?" (p.220 in Brody 1999).

(6) John expected  e  to leave

(7) *John expects.

(8) *John expects t to seem/be obvious that S

(9) John expects it to seem/be obvious that S

Hornstein (2000) refers to his forthcoming work and also to earlier unpublished work at the University of Maryland and also by Epstein and Seely, where the assumption that the EPP holds for all sentences is rejected.[10]  He considers this to solve the problem:  "Then sentences like [(8)] are not derivable as there is no licit subject position to merge into in these sorts of infinitival complements" (pp.136-127, footnote omitted).

But first, while it is true that on Epstein and Seely's theory there would be no subject position in the intermediate A-chain trace position in (10),

(10) *John is certain to seem/ be obvious that S

it appears that under their approach there would be a subject position in the infinitival spec-T embedded under *expect.* Epstein and Seely 2000 are explicit: "...the EPP as D-checking is redundant

with Case and Agreement checking in tense and control clauses". (8) unlike (10) has a matrix control verb and the subject of its complement clause (unlike the subject in a raising clause) has been argued to check with Inflection. So it's not immediately obvious how a theory that eliminates intermediate A-traces (A-traces in spec-to positions) in raising, by dropping the EPP, can be extended to control verbs and Hornstein does not elaborate.

Second, ignoring various technical and theoretical problems with the proposal, even if it could be made to work,  its relevance to the present issue would be dubious. Suppose that there is no trace in the embedded subject position. This does not change the question nor does it help to answer it. The question still is "Why is a structure like [(8)]," --now without the trace in the embedded subject position--"where *expect* is a hypothetical verb with no accusative and a subject theta position, always ungrammatical? Why can't a structure like [(8)]" --with or without the trace in spec-to -- "ever express what [(9)] does; why can't (a verb that fits into) this construction exist?"

Hornstein continues by this sentence "Other problems that Brody cites are more interesting" (p.137). As we have just seen, he has not found a solution, --to the problem that is less "interesting" for him for reasons that he does not state and which do not seem obvious.

Hornstein next turns to a subpart of the objection in Brody 1999, that if raising and control are the same A-movement operation then why do they appear to hold under different structural conditions? "It is not clear why (11) is ungrammatical if the distribution of OC PRO and NP-trace is in principle the same and PRO is just an NP-trace in a chain that happens to involve multiple θ-positions". In other words what is the difference between *attempt* and *believe* that makes them behave differently from the other in raising/control contexts [(11)/(12) vs (13)/(14)]? (Brody 1999, p. 219)

(11) *John was attempted t to leave

(12) John attempted PRO  to leave

(13) John was believed t to have left

(14) *John believed t to have left


Hornstein's reply ignores the general issue, but comments on one aspect, namely the question of why (11) is ungrammatical. His suggestion is this: "Assume that some operation can void the CP phase derivationally and say that this is prevented from occuring in passive verbs"[11] (p.137). He then says that "the main difference" between this assumption and "the more conventional wisdom" is that his assumption "makes barrier removal sensitive to the voice of the matrix verb" whereas the Principles and Parameters view does not.  He concludes "I do not see this as sufficient reason for prefering the conventional view" (p.138).

However it is probably more felicitous to phrase the issue like this: does the theory where control equals raising necessitate additional ad hoc conditions? (Hornstein admits that his suggestion is "technical" in nature.) And the point remains that under this "technical" proposal for a single aspect of the issues here, an additional possibility (sensitivity of the presence of the barrier to voice) needs to be introduced that is not instead but **in addition to** the standard condition of sensitivity of the presence of the barrier to the lexical class of the matrix verb.[12]


3. A new non-argument.

Hornstein (2000) withdraws the data and the analysis of the interaction of scope and control in Hornstein 1998. "It seems that the kind of ambiguities present in raising constructions like [(4)] occur in control structures like [(5)] as well"


(4) Someone seems (to John) to be reviewing every report

(5) Someone hoped to review every report

He then goes on to suggest that this state of affairs "streamlines" his proposal but "constitutes a puzzle, no doubt surmountable, for Brody's general approach" (p.140). These remarks are based not only on the belief that in (5) *someone* can have scope under *every report* but also that this must be treated in terms of reconstruction/lowering, --which Hornstein plausibly takes to be possible for movement/chain type relations but not for control, if in the latter antecedent and anaphor both have their own chains.

Suppose for the sake of the argument that the new understanding of the data is a better idealization than the old. But the assumption that inverse scope is achieved here by lowering cannot be right. The matrix verb is also a scopal entity and the matrix subject quantifier must take scope over it which it could not do if it was interpreted inside the embedded clause. "A unicorn wants to leave" does not mean that it is wanted/there is a desire for a unicorn to leave".[13] So if the new understanding of the data is correct the analysis must involve positioning both quantifiers higher than the matrix verb. This is a straightforward matter if Hornstein's A-movement analysis of quantifier scope is not adopted but appears to constitute direct counterevidence for Hornstein' approach. The point is supported by the further observation that if in (5) for example *every report* takes scope over *someone* then the universal also necessarily takes scope over the matrix verb.

Hornstein concludes his discussion of control by saying that I (=Brody) have "many things to say about control". However, he considers that "they do not add up to a good reason for backing away from the movement theory of control (footnote omitted, see note 14 for discussion)". As it happens, I did not say that they did, all I claimed was that "the approach to control that H[ornstein] advocates is seriously problematic," and that he "appears to provide no real grounds for questioning" the standard analysis. On the former point however, there appears to be agreement

between us, --indeed Hornstein now rejects the analysis he gave for a quite different one. Additionally the new analysis does not seem tenable. If we assumed it nevertheless, it would then inherit also most of the problems of the old one, which, as we just have seen, Hornstein's reply failed to resolve.[14]


## 4. Representations

I started my (1999) reply to Hornstein's paper by pointing out that his desire to eliminate the chain-move duplication fits well into the program set out in Brody 1995[15]: the construction of a theory without representational-derivational duplications. But I noted also that he does not attempt to resolve the more general issue of which chain vs move is only a subcase: the dubious architectural redundancies of the standard minimalist framework in which derivations and (LF) representations duplicate each other, the same information being carried by both.

Hornstein in his reply distinguishes LF as a "level" where a level is a "point in a derivation at which gramatical conditions, distinct from Bare Output Conditions, are stated on phrase markers" and LF as a "representational output" yielded by grammatical operations "eg., phrase markers that are handed over for interpretation to other cognitive faculties". He says that on the first construal he "agree[s] that [postulating such a level] needs argumentation" but not on the second.

First the question I have been raising for some time now is whether the representational or the derivational component should be eliminated to avoid the chain-move and the merge -result of merge[16] duplication. Hornstein, like some other authors, is apparently not willing to be engaged in a debate that questions the grammar internal existence of derivations and is only prepared to discuss issues about the necessity and appropriate understanding of representations. I find treating the existence of derivations (the sequentiality of the definition of LF) as an unquestionable truth strange in a scientific setting.

Second, the distinction Hornstein (again, among others) makes between LF as a "level" and LF as a "representational output" seems to me somewhat tenuous. After all, other than stipulation, for what reason should a representation not be a "level" if conditions of type A (conditions that follow from legibility considerations, whatever that exactly means), apply to it, but necessarily a level if conditions of type not-A apply?

Third, and most important here, the distinction he makes is in fact irrelevant to the issue I am raising. The terminological question of what we call a level matters little, and I'm happy to use "level" and "representational output" here the way Hornstein characterizes these.[17] In these terms, the point I have been making for some time refers to the notion of "representational output".

It is clear intuitively and is easy to show[18] that in a derivational system each merge and move operation must involve more than one representation (its input(s) and its output) and is in fact equivalent to a multilevel constraint. The information that move carries, and the information that a corresponding chain provides duplicate each other. Similarly sisterhood and mother-daughter (immediate domination) relations are duplicated by merge. It seems methodologically desirable to eliminate these unnecessary duplications. It is only possible to do this in a purely representational theory without derivations. A derivational theory cannot avoid representations (see note 18. above)

A derivational system is in fact equivalent to a multirepresentational system, the representational alternative simply drops all intermediate representations as unnecessary and keeps only the interface representation (whether this is a level or not in the sense just characterized being immaterial here). It's clear that there are aspects in which this approach is more restrictive. On the other hand, it remains to be seen how a multirepresentational approach, where appropriate translations of the relevant representational configurations (chain and sisterhood+immediate domination) hold also between adjacent "representational outputs" in a derivation, could be more restrictive than an approach that eliminates all except one representation, the one at the interface.

In particular, the restriction that there are no constraints on LF, other than a general structural condition (whether this is a sequential algorithm or a representational statement) and legibility conditions, can be part of both the derivational or the representational theory of the structural LF condition. In spite of frequently made assumptions to the contrary, this hypothesis may or may not be incorporated in both approaches and thus there is no general reason to think that it can help to choose between them.

An example of the greater restrictiveness of the (single) representational approach is that for some LF representations more than one derivation may correspond (eg. a cyclic and a countercyclic derivation) but the additional possibilites provided by the weaker derivational system never seem to be genuinely necessary. Differently put, the less restrictive derivational system needs to be supplemented with some constraint that has the effect of the cycle while cyclicity appears to be automatically ensured by the representational theory, without additional stipulation. Note that this is not a methodological point in any sense, but a summary of an argument based on empirical facts. See Brody 2000 for more discussion.[19]


5. Conclusion

Hornstein (2000) also says towards the end that he "believe[s] that many (if not all) the suggestions put forward in Hornstein 1998 are incorrect" and repeats his aim in the reply which "has been to show that the arguments presented [in Brody 1999] are of little relevance to the program of eliminating chains as fundamental grammatical constructs". This is perplexing and, as we have seen, unsubstantiated rhetorics. However, the fact that what were presented as arguments in Hornstein's 1998 paper for a specific claim ( "The paper presents a theory that dispenses with chains" (p.99.), "chains are superfluous constructs" (p.100) ) have now been tamed into "a program" should be welcomed. (As noted earlier (Brody 1999) Hornstein's program is a partial attempt to eliminate a

systematic redundancy stressed in Brody 1995 and to do so in one of the two obvious ways outlined (though not the one actually taken) there. But we should keep in mind that Hornstein (1998, 2000) gave no valid arguments for his 'program'. Of the two flawed main arguments he made in his 1998 paper, in Hornstein 2000 he claimed not to have made one and attempted unsuccessfully to defend the other by adding some apparently irrelevant and self-contradictory remarks. As for the wider derivational-representational issue, although his contributions discussed here can be viewed to point to certain aspects in which the representational approach might be superior,  he has not in fact provided evidence or valid argument pertaining to the choice between these approaches.

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[1] For a recent discussion of the derivation vs representation issue see Brody 2000.

[2] Hornstein's intuition is correct when he states that "Brody (p.212) dislikes that Hornstein (1998) argues against A-chains, has little to say about A'-chains and yet concludes that chains do not exist" (2000, note 10, p133.). On the cited page I remark on a couple of formulations in Hornstein (1998), which are potentially capable of misleading readers who are not experts of this particular area, --misleading them to think that Hornstein's (1998) conclusions are more generally motivated and more conceptually inevitable than they in fact are. Although I did not say all of this explicitly, I am surprised that there may be any doubt as to what I meant to draw attention to. Hornstein attributes my dislike for this type of rhetorics to a position he imagines I take (on the basis of no evidence as far as I'm aware) on the differences between A- and A'-chains. This he takes to be roughly that of mainstream Principles and Parameters theory as e.g. in Chomsky 1982, against which he proceeds to defend his position (with assumptions that were in fact largely characteristic already of Williams 1986), --all of this a red herring in the present context.

[3] Hornstein discusses further whether his discussion relies on dispensing with QR, an issue I did not address. In this context he suggests that "what makes the argument run" is "[s]imply the distinction between chains as the units of scope interpretation versus terms as the units". As we have seen the "argument" doesn't "run", and additionally talk about terms instead of chains would create otherwise avoidable redundancy. Here he takes note 11 on p. 215 in Brody 1999 to indicate that I might wish to find some kind of a compromise retaining terms as interpretable at the interface and also chains as syntactic units. Note 11. is beside the point, however. As is made explicit there, it talks about binding (essentially coindexation inside a scope domain) and not simply scope. The suggestion there that a chain can bind from more then one position is in any case not equivalent to what Hornstein appears to read into it, namely that "at the C-I interface terms are the units of interpretation rather than chains" (p.133). Although I'm not aware of having given the slightest indication of it, Hornstein contemplates further that I may wish to reject the "assumption that the units of interpretation at the interface determine the units of syntactic manipulation ", which he rather menacingly warns "has a venerable minimalist pedigree" and requests me to "be up-front" about this. Alas, it's enough hard work to be up-front about the assumptions I do make.

[4] (4) and (5) correspond to (6b) and (18) respectively in Hornstein 1998

[5] I shall just quote from the conclusion of section 3. of Hornstein 1998, where the argument about (4) and (5) is made:
"The above argues that chains are inadequate interpretive objects and thus θ-roles should be analyzed as features..." (p.110) In Hornstein 2000 he claims (see the quote from p.135 in the text) that his main point in the 1998 paper was to reject the "simple" (p.134) argument against eliminating chains, that there would then remain no way to capture theta role transmisssion. This statement seems incorrect. To repeat: Demolishing a straw man argument (that nobody I know would want to spend time defending) against one's assumptions (Hornstein's recollection of what Hornstein 1998 did) is not equivalent to an argument that one's assumptions are right (that Hornstein 1998 attempted unsuccessfully to provide).

[6] There are many other problems, see my first reply and references cited there.

[7] In the top paragraph on p. 135 Hornstein repeats twice that the main "payoff" and "power" of the assumption of treating obligatory control and raising in a unified manner is that "the core properties " "follow without further stipulation". Thus the motivation for treating the similarity in one way rather than the other is still based on the fact that there is a similarity, --just as in Hornstein's 1998 paper.

[8] Again quoting Hornstein 2000: "...local lexical anaphors have properties very similar to those of OC [obligatory control] PRO. This suggests reducing one to the other" (p.135). This is again a case of the same fallacy that is discussed in the text and in note 7. above.

[9] I follow Hornstein in abstracting away from issues relating to expletives in this discussion.

[10] The idea that intermediate A-chain traces could be eliminated was put forward and discussed also in Brody 1997.

[11] Hornstein in fact states that his approach "involves exploiting Chomsky's 1998 notion of a phase" (p.137). It seems to me that if we leave out the the words *phase* and *derivationally* we lose nothing except unnecessary complications. Hornstein does not provide a reason to think that it is relevant for his point if CP is or is not a phase and whether it is "deleted" derivationally or just not present in the final representation in the relevant passive structures.

[12] Concerning another problem I raised (1999, p.217-218), relating to a lost generalization over locality effects that are obeyed by both obligatory and nonobligatory control structures and to the dubious pronominal status of non-obligatory control PRO in Hornstein's approach, he now remarks that my comments "are rather short and categorical" (note 22, p. 138-139). I actually cited some appropriate references (Manzini 1983, Brody 1985 and others) where these matters were discussed in more detail some time ago, and as Hornstein (2000) states elsewhere, "[h]ere is not the place to restate analyses and arguments that have been discussed in various other places" (his note 12, p.134). Apart from this surprising objection, Hornstein also says in his note 22, that "I [=Hornstein] disagree with Brody that these [OC and

NOC structures] show entirely parallel properties". He can hardly disgree with me here however, since again I never said or thought what he attributes to me. He continues: "though there are points of intersection". But that was all that my point about lost generalizations required. Hornstein then enumerates cases where NOC structures behave differently from OC structures. In fact, apart from the fourth case whose relevance is questionable, these structures were discussed in the works I cited, where an appropriate generalization and an account (though not an explanation) was proposed for them as well. Hornstein points out correctly that "the classical "theory" of control, is less of a theory and more a series of lexical stipulations". But of course I'm not interested in defending the classical approach, I have only been interested in finding out if Hornstein's proposals constitute a genuine improvement and/or a promising direction.

[13] As noted by David Pesetsky who read this part of Hornstein's reply on the train before I did.

[14] In his footnote 29 (p.141) Hornstein repeats that having rejected the EPP there is no correspondent of (obligatory) PRO in spec-to position. As discussed above it's not immediately clear how this follows. He draws the further conclusion: "There is no […]'analogue' for OC PRO." Again even if the trace in spec-to is not assumed, why the same issue does not arise for the trace in the spec-V theta position is not explained.

[15] The relevant parts were circulated in 1991.

[16] This terminology for what I have been calling the architectural problem is due to Starke 2000.

[17] Typically "level" and "representation" are used just conversely in minimalist works. In my first reply I used "level" more neutrally, for both senses. For more discussion of the level vs representation issue under the heading of weak and strong representationality, see Brody 2000.

[18] For example for move to operate across some bigger unit U, at the point where move is triggered U must be transparent (ie. a representation) to make the target of move visible. See Brody 2000.

[19] Hornstein (2000, note 28, p.140-141) asks why lowering rules should not exist, and indeed in a derivational framework, I'm not sure what could be the right direction to search for an answer. In representational terms (i.e. if move is eliminated and chain is retained) however, as noted in Brody 1995, 1998, the equivalent of lowering can be excluded, -- by ruling out feature lowering, more precisely a feature F being duplicated, and the duplicate being licenced by F, in a position lower than F. Hornstein however sees "nothing conceptually odd about having lowering rules" He asks "why should going up be conceptually better than going down?" And even "There is something methodologically suspect" about defining such an asymmetry into UG" etc. Avoiding details, the prohibition of feature lowering brings to mind semantic compositionality, from which we might expect this prohibition to follow in a representational framework. Additional assumptions with an uncler status would seem to be necessary to carrry over such an explanation to a derivational setting, to rule out lowering.

In the same note Hornstein states that "Brody ([1999] p.223) asserts that there is good evidence for an LF cycle" and that he disagrees. Again I did not quite say that. Given my representational approach I don't believe even in the existence of a covert syntactic component. What I said was that "The assumption that the extension condition refers only to overt syntax is also highly problematic" but this in the context of Hornstein's assumptions, which explicitly include the hypothesis of a covert component. On this see Brody 1997, one of the references I gave in this connection. But even in the derivational context my statement does not entail the existence of an LF cycle, again, see Brody 1997. Hornstein's disagreement appears to be based on misunderstanding.

In the previous note (Hornstein 2000, note 27, p. 140) he objects that "Brody's theory, like Chomsky's retains a residue of D-structure, coded in Brody's case in the condition on chains". Recall first that the major argument against a level of D-structure (both in Brody (1991-)1993, and in Chomsky (1993-)1995) was that the theta criterion is true almost by defintion at LF, hence it seems wrong to duplicate it by stipulation at an independent level. Chomsky's later assumption, that merge in theta position is only possible for (and is required by) arguments is indeed problematic then as it stands: this will generally happen at a stage in the derivation that has not yet reached LF. The condition I am sugggesting, that only the root position of a chain may be thematic --or the improved statement in Brody 1998 that does not refer to the root, but from which this statement largely follows (given the prohibition of downward percolation)-- involves no such conceptual problem. First all this happens at LF, so the argument against D-structure does not apply. The fact that the earlier version of the condition picked out chain roots that under certain assumptions correspond to what were D-structure positions does not change the situation, --these are different concepts understood to be part of LF. But this issue is academic given the later formulation which does not need to refer to chain roots. Hence I see no valid sense in which this theory retains a D-structure level.