On the status of representations and derivations[*]

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Abstract

I reargue the point here that current mixed theories of syntax that involve both derivations and representations are redundant and in principle less restrictive than their pure representational or pure derivational equivalents. Next I show that no pure derivational theory of narrow syntax exists. To be minimally adequate, derivational theories must be mixed, hence the arguments against mixed theories apply to these too. In addition to this point I argue that everything else being equal and with no additional stipulations added, derivational theories with the role of move are less restrictive than representational theories with the concept of chain. In the third section of the paper I consider the derivational explanation of the asymmetry of the notion of c-command and conclude that this explanation, like other explanations of c-command (and also like other derivational explanations) is not successful. I suggest instead that we should eliminate c-command from the grammar and replace it by simpler interacting notions.

1 Representations and derivations, – the status of the mixed theory
1.1 Restrictiveness and duplication

As set out in earlier work elegant syntax (ES) differs from the minimalist framework in several important respects. [1] I shall elaborate here some remarks made earlier on those features of this approach that relate to the so-called representational-derivational issue. I argued that since chain and move express the same type of relation, a theory that contains

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[1] Brody (1997a,b, 1998a,b). Although, in these works, I referred to the framework of elegant syntax as perfect syntax, the operative sense of perfection was invariably that of theoretical elegance. Hence the change of terminology.

both concepts is redundant, and therefore, at least in the setting of ES, wrong.[2] As has been also noted repeatedly, the issue is more general: there is an "architectural" redundancy in theories that assume that both representations and derivations play a role in the competence theory of narrow syntax.[3]

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[2] Brody (1995a, 1997a,b, 1998b). As has been noted before, in Brody (1995a) the argument against mixed theories (which include both the pure derivational and the pure representational alternatives) and the argument for the representation option (as opposed to the derivational one are clearly distinguished. A certain amount of confusion has been generated in subsequent literature by not always keeping these two points distinct.

[3] See esp. Brody (1995a, 2000a). For more recent discussions of the architectural duplication see Epstein et al. (1998) and Starke (2000). See also Hornstein (1998), who is apparently satisfied to keep the architectural redundancy, but wishes to eliminate the chain-move duplication, - a surprising position to hold without argument. Hornstein argued for eliminating chains rather than move by pointing out that in structures like (i) the interpretation on which *someone* binds (takes scope) over *his* and *every party* takes scope over *someone* is impossible.

(i) Someone seems to his friends to have attended every party

(ii) Someone seems to his friends to have every party someone attended every party

He claimed that this does not follow if chains are present at LF. In the chain structure in (ii) a chain member of *someone* in the lower clause (perhaps in spec-V) can be lower than a chain member of *everyone* (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 pronoun *his* (can take scope over *his friends*). 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 (ii), so it either scopes over *his* or under *every party* but never both.

In Brody (1999), I noted that the real argument here is actually for, and not against, retaining 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 (ii) from more than one of its chain positions since this will lead to infinite regress. *Someone* (from its higher position) will take scope over *every party* and *every party* will 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. So there may be an argument here for retaining chains on the grounds of avoiding redundancy; and also, given the undesirability of the mixed theory, for eliminating move.

I should note that Hornstein (2000) attempts to maintain the argument of Hornstein (1998) on the grounds that "this kind of reading is perfectly coherent and can be represented as [(iii)]" (p.5. in ms.)

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

But (iii) 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 (iii) *every report* scopes over *himself* but in (ii) *every party* does not scope over *his*. If it did, no problem would arise as (iii)

Let us note first that a general conceptual argument from simplicity in favour of a pure (representational or derivational) theory against a mixed one is weak or nonexistent. This is because it is in principle possible that derivational and representational principles are both necessary in syntax and that they hold in different domains, and/or distinguished also by other independently needed principles and properties, - ie. cluster in a modular fashion. Such clustering of properties with chains in one module and move in another does not seem to obtain in narrow syntax (the lexicon to LF-interface mapping), but this does not seem to be a necessary state of affairs, but rather an empirical fact about language. It may be that in wider domains, like the theory of mind for example, both derivational and representational components will be necessary. The important point here is that the argument from redundancy, against mixed theories of narrow syntax to be discussed below is not purely conceptual but is ultimately empirically based.[4]

Consider then representations and derivations in narrow syntax.[5] In principle there are two possibilities here (ignoring now possible but nonexistent mixed situations that involve

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shows. So (iii) is irrelevant, it does not represent the reading under discussion. All it does is gratuitously confuse an otherwise quite straightforward issue.

[4]A possible argument against the approach I'm taking here might be that it focuses narrowly on LF. When we take the full theory of expressions generated by the grammar this seems to include a derivational component: a mapping from narrow syntax to PF. Therefore the overall theory of grammar would be simpler if the theory of the lexicon-LF relation was also derivational. But we seem to know too little about spellout for this argument to carry much force. First, it is not clear that the spellout component is indeed derivational (i.e. sequential) and not just a one-step mapping. Secondly, even if they are derivational, we do not know if the principles of spellout are different or similar to those of narrow syntax. The general idea of syntax being a generative and spellout an interpretive component, would not make it unexpected that spellout principles have a different cluster of properties from the principles of narrow syntax. If this is the case, that would make at least the intuitive simple version of the simplicity argument inapplicable. More complex versions - like for example that the same principles apply differently in the two domains (the differences being due to the different properties of the elements to which they apply) - may still hold. But again, we seem to know too little about spellout to make any such point with more confidence than its negation.

[5] It is sometimes suggested that a representational approach simply translates a derivational approach and with the cost of involving more set theory. It is not clear how the amount of set theory involved is relevant to what is an empirical issue: which system is instantiated in the mind of the speaker. This is an empirical matter to which both empirical considerations and conceptual considerations of sharpening the concepts involved may be relevant, but the mathematical properties of the object postulated to exist will have to be whatever empirical research (with concepts adequate for the task) determines them to be. Once the set theoretical point is eliminated from the picture, as I think it should, it is clear that a priory we don't know if the derivational theory is a (perhaps misleading) translation of the representational approach or conversely.

both possibilities in a modular fashion). Either derivations and representations are (a) empirically distinguishable, or (b) they are not. Although it may have been sometimes argued that both of these situations obtain, it seems obvious that these two states of affairs are incompatible.

I return to (b) in the next subsection. Let us consider first the situation where we take (I think correctly, cf. section 2. below) the representational and the derivational theory to be empirically distinguishable. When the argument against the mixed theory was initially put forward there were essentially no attempts to construct analyses that relied on the existence of both derivations and representations. Given the lack of such arguments one obviously opts for either a fully derivational or a fully representational theory on general grounds of restrictiveness.

While there may now be some contributions in the literature that postulate both representations/chains and derivations/move and exploit one or another assumed (typically stipulated) difference between these, as far as I am aware there are essentially no strong arguments for postulating both concepts as part of narrow syntax.[6] Nobody has attempted

[6]Heycock (1995) was one early case where it was explicitly argued that both derivational and LF conditions are necessary. For critical discussion see Brody (1997b), Fox (1999). To take a somewhat random choice from relatively recent work that assumes and attempts to argue for a mixed theory, take first Nunes (1999), that argues that Move should be decomposed into copy (C), merge (M), form chain (FC) and chain reduction (CR). In fact M is not different from the usual merge operation that puts together phrase structures, CR is a spellout issue and C need not be separate from selecting from the lexicon the same thing twice. (The difference between the relation linking the two pronouns in “He said he left” and “He was seen (he)” does not have to do with different lexical access as is sometimes suggested). It is plausible to attribute that to FC having applied (or being able to apply legitimately) to the two pronouns in the second but not in the first structure. So only FC remains. In other words it is not clear that this approach really needs to be different from a representational account. It looks of course different: for Nunes C applies as part of a derivation. That a derivation exists and that C is part of it are thus additional assumptions.

In support of the assumption of keeping copy and (re)selection from the lexicon distinct Nunes refers to Chomsky’s (1995) argument from expletive construction where greater cost is assigned to move than to merge to rule out (i).

(i) *There seems a man to have left

Nunes suggests that correspondingly select and copy have different costs. But if copy is the same as select, the desirable simplifying assumption he is arguing against, then accessing the same element from the lexicon (or array/numeration) for a second time will be more expensive than accessing a new one. While this assumption is not particularly natural, it seems to be no less so than making copy more expensive than select. So the foundations of derivationalizing the account by adding Copy do not seem to be here. Note also that (i) may be excluded by independent reasons: for example that no lexical element, expletive or not, is ever permitted in the infinitival subject position that follows *seem*-type predicates. On accounts that exploit this fact, assigning different cost to different derivations would probably become altogether irrelevant.

In support of the assumption that derivation exists Nunes cites the following contrast:

(ii) "Which book did you review this paper without reading"

(iii) "Which book did you review without reading"

This is supposed to motivate derivations on the grounds that *which book* moved sideward from an island in (iii) before it became an island and then to the front while a similar non island-violating derivation is not possible in (ii). But there are no reasons why a largely similar alternative account could not be given in a representational vocabulary. In (ii) *which book* is separated from it's trace (theta position) by an island. In (iii) it is not. since there is a trace in object position of the matrix clause. The trace in the island causes no violation if the wh-phrase need only a single thematic trace to be subjacent to it. (see eg. Brody (1995) or Richards' (1997) principle of minimal compliance, a major and very interesting generalization of an idea in Brody (1995) as he notes, and which he refers to as "subjacency tax"). All this seems straightforward, and makes no direct reference to parasitic chains. It's not clear why the derivational approach is better. In fact, for there to be an argument for derivations here, it would be necessary to argue that something along these representational lines cannot be right, otherwise Nunes' account (and the derivational equipment it is supposed to motivate) is redundant and therefore undesirable.

Lechner (2000) proposes an interesting analysis of NP-comparatives where an empty operator raises to an intermediate spec-C position and the AP moves into the matrix:

(iv) Mary met [young-er men]$_i$ [$_{CP}$Op$_j$ than Peter met [$_{DegP}$ [$_{AP}$ young men] Deg t$_j$]]

He suggests an argument for a mixed theory based on the following observation: "empty operators in spec-CP of the than-XP [do] not interfere with AP-movement" (p.16): He observes that the two APs should not form a chain for thematic/semantic reasons. Hence he suggests that these APs are linked by a move operation that applied countercyclically to avoid the island effect induced by the empty operator. Note that countercyclic operations seem to be (a) quite problematic, as discussed in the body of this paper, and (b) they also seem to be beside the point if the relevant locality constraints (like on Lechner's assumptions the thematic requirements) apply only to chains. Furthermore no crossing problem would arise if the matrix AP and the empty operator are coindexed and the operator in turn is related to the whole degree phrase in the lower clause, --as in other similar constructions analyzed in terms of empty operator movement since the late seventies. Lechner provides arguments from principle C etc. that the structure does not involve pure deletion only but movement/chain but his evidence does not seem to distinguish between linking the AP to its matrix clause correspondent or to only to the operator at the edge of the embedded clause.

Pesetsky & Torrego (2000) provide an interesting and intricate new analysis of the *that*-t effect and various related matters. They argue for what they call "relativized extreme functionalism", which appears to be an approach near identical to Brody's (1997a) bare checking theory. (I think the colourful name is misleading, the issue involved in eliminating features that are in principle uninterpretable is one of restrictiveness and has little to do with functionalism.)

In bare checking theory all features must be interpreted in principle, but in a given sentence some occurrences of features may be in positions where their usual interpretation cannot be assigned to them, where interpretating them would not make sense. In such cases occurences of features of type T (say *wh* for example) in position(s) where they cannot be interpreted will have to merge (presumably via the chain and the spec-head relation) with another feature of type T that is in a position where interpreting it would make semantic sense. Pesetsky & Torrego's approach is not completely identical to bare checking theory because to show that the results achieved in the less restrictive framework, that apparently involves massive duplications (a property that is strange even in a minimalist setting, let alone ES) cannot be restated in a non-mixed system that avoids redundancy and lack of restrictiveness. There are also no attempts to argue that the assumed advantages outweigh the considerable burden of significantly weakening the grammar. It is clear that, even if focused arguments existed, for the claim that both derivations and representations must exist side by side within the language faculty and duplicate each other, these would have to be treated with extreme caution, since they would amount to a proposal to adopt a less restrictive grammar.[7] Everything else being equal, there are clearly more analytical

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they wish to retain the otherwise apparently dispensable operation of feature deletion (as it follows feature checking) in order to integrate into their system the anti *that*-t effects in sentences with topicalization like:

(v) Mary said *(that) John she liked

However it is not clear if such sentences should or can be integrated with other data they analyze. Anti *that*-trace effects constitute a much less clear class of facts than *that*-t effects. Maybe a pause in cases like (v), where the matrix verb does not select for *that*, suffices, suggesting perhaps an approach in terms of parsing. Pesetsky & Torrego themselves appear to almost come close to saying that anti-*that*-t effects might not exist. Nevertheless they attempt to extend their theory to cover such facts, but at the cost of a set of otherwise unnecessary and ad hoc assumptions that in turn seriously question the claim that these facts have genuinely been "integrated". It is necessary to reengineer their notion of locality into a much less appealing form, specifically to cover this case, it is necessary to retain the otherwise unnecessary operation of feature deletion, and it is even necessary to adopt a gamma marking type mechanism that distinguishes deletion of a feature from the feature being marked for deletion - the latter carried part way through the derivation.

It seems fair to say that even if we assume that the anti-*that*-t effects must be treated syntax internally, Pesetsky & Torrego have not successfully integrated these into their theory. Assuming that anti *that*-t effects need to be treated differently, all dubious theoretical adjustments and innovations just mentioned can be dispensed with. The argument for derivations that they consider to have provided then disappears together with the curious gamma-marking type distinction between marking for deletion at a one derivational stage and deleting at a later one. The representational theory and bare checking theory will do the work required elsewhere in Pesetsky & Torrego's paper and they will do so without the derivation-dependent and ad hoc additions.

[7] To make the point of restrictiveness more concrete, recall for example that (as noted in Brody (1997a)), Chomsky (1995) proposes a representational definition in addition to the derivational system of interface assembly (in effect an additional definition) of what counts as a well-formed syntactic object (cf. also Brody (1998a) for some discussion). Or take the additional distinction he makes between deletion (interface invisibility only) and erasure (essentially invisibility also for Move), where erasure occurs only if this would not violate the representational duplicate definition of well-formed syntactic object. Such duplications that exploit the derivational-representational duplication and distinctions that in turn might build on these additional duplications should probably have no place in a restrictive system of syntax and are indeed excluded in principle by avoiding the less restrictive mixed theory that makes them possible in the first place.

possibilities in a theory that has both representations and derivations with differing properties than in a system that only has one of these concepts.

I shall refer to these considerations as the argument from restrictiveness against the mixed theory of narrow syntax. Let me summarize this argument. Suppose that representations/chains and derivations/move have different properties. (This seems to be the case.) Then it's an empirical question which notion(-sets) are the right ones. Having both would weaken the theory in the sense of increasing the analytic options available (see note 7. above), hence very strong arguments would be needed to maintain that both sets are part of the theory of syntax. No strong argument appears to exist. Further, in addition to the problem of the unmotivated lack of restrictiveness, we would also have the problem of the unmotivated systematic (representational -derivational) duplications of concept-sets.

1.2 Principles of I-language

Suppose then, as is sometimes suggested, that arguments for a mixed theory are lacking because the issue they would address is effectively meaningless. Representations and derivations are just notational variants, they are simply different approaches to expressing the same notions and the same generalizations. Suppose that there were no empirical differences to distinguish the derivational and the representational views.

But, on such an assumption, a mixed theory like standard minimalism only becomes more strange. Putting aside the uninteresting case where notational variance means synonymy, two names for the same concept, let us look at the situation where we take derivation/move and representation/chain to be two different aspects, or two different ways of looking at, the same phenomena. Consider first a situation in physics that might be somewhat similar. The famous double slit experiment of quantum theory can be interpreted either in terms of probability waves or, in terms of a particle being able to traverse multiple trajectories before hitting a target.[8] The two interpretations do not result in distinguishable empirical predictions. (This is the case now, and may or may not remain so in the future). Assuming this fact, it would be a strange theory that postulates both multiple trajectories and probability waves, say mapping one into the other. It would be much like a theory whose ontology is committed to two entities, the evening star and the morning star in the context of the assumption that ultimately they are empirically indistinguishable. The standard minimalist framework mapping derivations into

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[8] Remotely - and, at least here, irrelevantly - resembling syntactic chains.

representations appears to be equally curious - especially so when viewed from the perspective of ES - that rules out in principle the option of attributing redundancy to the effect of selection or to evolutionary accidents.[9]

To repeat, on the assumption that representation/chain and derivation/move are just notational variants (ie. no empirical evidence distinguishes them), they are either just different names for the same notions or perhaps different but (at least currently) not empirically distinguishable notions. So one could suggest that the choice between them is not real, that one of them is just a way of looking at the other. In such a situation, it may be reasonable to look for some deeper notions that subsume the two competing ones. But to conclude from the assumption that, say, move captures the properties of chain that both chain and move are part of the grammar seems mistaken. If we talk about (some module of) I-language, and say that y is part of it, hence a real object, and, furthermore, that x is just an aspect of y, a way of looking at or treating y, this does not then seem to entail postulating x as a distinct element of the mind. Further evidence would be necessary for that, but by hypothesis this would be unavailable if the two notions cannot be distinguished empirically. I shall refer to this consideration below as the argument from I-language ontology. So this argument is meant to establish that the mixed theory cannot be defended even on the (empirically dubious) grounds of derivations and representations being notational variants. But the main argument against mixed theories remains the consideration based on restrictiveness and duplication: there is relatively little evidence for distinguishing derivations and representations, and not surprisingly there is essentially no serious evidence for adopting both.

2 Representations or derivations

2.1 Derivational theories and weak representationality

Suppose then that the rejection of mixed derivational-representational theories, mainly on grounds of empirically and conceptually unmotivated lack of restrictiveness is correct. Next comes the related but distinct and secondary issue of whether syntax is better thought of as a purely derivational theory (PDT) or a purely representational one (PRT). By a PRT of narrow syntax (or LF) I understand a theory that generates the interface level in the mathematical sense of generation. The theory consists of a set of constraints or principles

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[9] See Brody (1995, 1997a,b 1998b 1999a, 2000b), Epstein et al. (1998) for more discussion of the redundancy issue and related matters.

that determine well-formedness. I assume that the question of how to assemble the representation falls outside of the competence theory of grammar and is part of how the linguistic competence system is used, most plausibly it corresponds to the theory of parsing and sentence production.

A PDT is an ordered series of operations with input and output, where the input may only consist of terminals and the outputs of some other operations.[10] The following three way distinction will be useful: (i) a derivational theory is nonrepresentational if the derivational operations create opaque objects whose internal elements and composition is not accessible to any further rule or operation (ii) a derivational theory is weakly representational if derivational stages are transparent (i.e. representations), in the sense that material already assembled can be accessed by later principles. Finally, (iii) a derivational theory is strongly representational if it is weakly representational and there are constraints on the representations (weak sense) generated.

It is clear that derivational theories must be at least weakly representational. Take an object z, the result of merging x and y. At some later step move can only apply to y if z is a transparent rather than an opaque object since otherwise y would not be accessible or even visible for this operation. Notice that even if move is reduced to merge and an interpretive linking operation (in a way parallel to the theory of distributed chains of Brody (1998b, 1999a)) the same conclusion would still hold: the interpretive link between x and y could not be established if z was opaque. The derivational theory therefore is at the same time a (weakly) representational theory with multiple (weakly) representational stages instead of just one at the interface.[11]

So there can be no derivational theories that are fully nonrepresentational. The derivational theory will always be a mixed one to some extent. It would also seem to be almost necessarily a multi-representational theory. One might think that this sort of weak representationality does not matter, since the spirit of the theory remains derivational. I can see two problems with this sort scepticism about the argument. First, weakly representational derivational theories are clearly mixed theories and the I-language ontology argument above in section 1.2 applies to them just as much as to any other empirically unmotivated mixed theory. The fact that all derivational theories must be mixed then appears to already provide a good reason for rejecting derivational theories of all kinds.

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[10] Actual PDTs and PRTs may have other restrictions - relating, for example, to the number of branches of nodes etc.

[11] If chain-members are linked interpretively, and the status of z can switch from opaque during the derivation to transparent at LF, then the theory may not be multi-representational, but would still be mixed.

Secondly, consider the suggestion that weak representationality does not matter, because the crucial difference between the representational and derivational view is that the latter is not strongly representational, there are no representational constraints on the structures that the derivation assembles. Hence, these structures (although weakly representational) are still not levels of representation in some more important sense. But given that derivational theories are at least weakly representational, a derivational operation must have an input and an output both of which are at least weakly representational. Thus, such an operation is equivalent to a set of representation-pairs: a set of possible input-output pairs. It can equivalently be thought of as a bi-representational constraint. We have to understand a weakly representational derivational theory as having only such constraints on the representations as are put on them by the operations (sets of input-output pairs). It is clear, then, that the distinction between weakly and strongly representational derivational theory despite appearances does not really have to do with the derivational-representational distinction. What it really concerns is whether there are constraints that are additional to those captured by the postulated derivational steps (whether we view these as representational or derivational constraints). The answer to this question may be either negative or positive, both on the representational and on the derivational view. Currently of course the working hypothesis of most linguists working in this domain is that there are no such constraints. But whatever additional content strong representationality has over and above weak representationality, and whether this extra is or is not taken to be part of the grammar, does not crucially pertain to the derivational-representational distinction.

To provide a more concrete example for this point, take current ‘derivational’ theory with the operation merge, some applications of which are a suboperation of move. The input of merge is any two well-formed representation WR and WR’ (built from terminals and subtrees by merge) and the output WR’’ is WR augmented by WR’ in a way that merge specifies. Thus, in the general case, merge is in fact a tri-representational constraint. Where merge is a subpart of move it applies to an element WR’ of a tree WR and augments WR with a proper subpart of WR, WR’. What merge specifies is that WR and WR’ will be sisters in WR’’ and furthermore WR’’ inherits its label from WR or WR’. (In the case of move, always from WR for reasons independent of merge.). In terms of a multilevel representational theory this constraint is essentially equivalent to the requirement that at every level a (sub)tree ST’’ is well formed iff (a) it immediately dominates two well formed subtrees ST and ST’ each composed of terminals and other subtrees (in the case of move ST’ is properly dominated by ST) and (b) ST’’ carries the label of ST or ST’ (always ST in the case of move). Given this background, the question of whether there are any syntactic constraints that are additional to merge and move has little to do with representationality or derivationality of the system. We expect mostly on grounds of restrictiveness that there aren't any but if there are, they can be stated either in derivational or representational terms. Note in particular that a constraint on a single representation can always be phrased as a bi- or tri-representational constraint with no restriction on the input(s).

Thus the essence of representationality appears to be weak representationality. Strong representationality does not seem to add a property that genuinely distinguishes between derivational and representational approaches. The otherwise important distinction between weak and strong representationality matters in the present discussion only insofar as it was necessary to discuss why it very likely does not matter here. If it is true that the core concept of representationality is weak representationality then of course having shown that derivational theories must be weakly representational, the question of whether we should adopt derivational theories of narrow syntax again reduces to whether we should adopt mixed theories in this domain. As we have seen in the previous section, this we should probably not do.

2.2 Restrictiveness again

So current (apparently pure) derivational theory is equivalent to a restricted multi-representational theory that has only such conditions on representations that can be stated as conditions that hold on two adjacent levels. As we have seen it's in fact not clear that this really is a restriction with respect to a multi-level representation al theory, since a single level condition could be equivalent to a bi-level condition where the input may be any structure. The real difference between derivational and representational approaches is different. The representational theory is a single level theory, all representations/derivations except the 'final' representation, LF, are eliminated - so conditions can only hold here. This is clearly one obvious way to constrain the multirepresentational theory: assume the existence of only a single representation, the one corresponding to the final output of the derivational system. Henceforth, I refer by representational theory unambiguously to the single level representational approach. To emphasize the representational properties of derivational theories I shall use the term "multi-representational".

The derivational approach constrains the multi-representational theory differently, in a way that does not resolve the problems of the mixed theory. The derivational representational duplication now translates as the duplication between the final representation and the relevant aspects of all representations generated that carry the same information. Sisterhood and projection is duplicated at multiple levels by the effects of merge and chain by those of move.[12] The derivational theory ignores the problems of duplication and lack of restrictiveness, but suggests a different restriction. In this approach constraints like merge and move (which, as we have seen, are effectively equivalent to multilevel representational constraints) are individuated and are crucially required to operate in a sequential manner.

Perhaps there are aspects in which the sequential derivational theory is more restrictive than the unilevel representational theory in an empirically motivated way. As far as I know, this has never been argued and there is little to indicate that this might be the case. On the other hand, there is immediate evidence of this type for the unilevel representational theory. It's more restrictive than the derivational approach, since it disallows bleeding relations, which don't seem to occur in narrow syntax. In particular the effects of the cycle, an additional stipulation under the derivational system, follow automatically from the representational nature of the theory.

If there really were derivational components in syntax we would expect bleeding relations to occur with some regularity, and if syntax was fully derivational as is frequently suggested, bleeding relations should be commonplace. Derivational systems are eminently suitable to express the situation where one operation bleeds another rule or constraint. Consider cases where lack of bleeding of some constraint C can be detected as the fact that ungrammatical sentences (ruled out by C on one derivation) do not become grammatical on a different one where the context for C would not arise. Take for example the well known fact that the wh-island or the subject island constraint cannot be bled by a derivation that involves movement before the relevant configuration is created as eg. in (1) and (2).

(1) a. what did you wonder Mary bought (what) when ==>
  b. *what did you wonder when Mary bought (what) (when)

(2) a. who was bought [a picture of(who)]==>
  b. *Who was [a picture of (who)] bought ([a picture of (who)])

To deal with the descriptive problems, the usual restrictive assumption added to derivational framework has for a long time been the idea of the cycle in various incarnations. The derivations in (1) and (2) do not obey the cycle. Cyclic application of all rules and constraints removes this empirical problem, together with other similar ones. The

[12] In fact, I argued that neither categorial projection (Brody (1997b, 2000a)), nor the chain relation (Brody (1998b, 1999a)) should exist narrow syntax internally, but I put these matters aside here.

solution is less than satisfactory if proposed as an explanation of the lack of bleeding in derivational frameworks. While the cycle may be a simple and attractive construct, nevertheless, it is an additional stipulation that (as first observed in a somewhat different framework by Freidin (1978)) appears to be unnecessary on the representational view. Until the cycle is independently motivated, the representational theory has the advantage of being more restrictive than the derivational theory in an empirically motivated way. The derivational approach can achieve the same degree of restrictiveness and empirical adequacy only by invoking an additional descriptive stipulation.[13]

Epstein (1995) proposed that the cycle is a consequence of an appropriately defined notion of c-command, together with a PF ordering requirement. The intuitive idea is that a relation based on c-command must be defined between all terminals of the tree (to make possible the exhaustive ordering of the terminals at PF by the LCA) and c-command is defined in terms of merge (as holding in a particular way between the merged categories, see section 3.1 below). In a countercyclic operation applying to A, A will not, therefore, have this c-command based relation established with higher nodes in the tree. Such operations will thus be impossible.

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[13] Note that the examples in the text are not simply cases analysed representationally that are translatable derivationally without any gain or loss in understanding - something that often seems to be the case with arguments for derivations. The examples here illustrate the point that there are several derivations for a single representation, some of which need to be stipulatively exluded by some principle that is not entailed by the derivational nature of the grammar. So it does not matter for example if in (1b) *when* in the lower spec-C is in the (intermediate) trace position of *what* or there are two positions available here, one for each wh-phrase. The pure derivational theory that contains no traces/copies (if it did, it would encode earlier stages of the derivation into later representations) will not exlude exclude (1b) without some auxiliary assumptions that prohibits the countercyclic derivation.

Similarly in (2) it is not relevant that the subject island constraint apparently has to hold of subjects only. This is not a stipulation that is additional to what would be necessary to exlude the structure in a derivational framework. Derivationally the assumption translates as the constraint holding only for extraction from subjects. This much is necessary so that the structure be excluded on the cyclic derivation, but does not suffice to rule out by itself the countercyclic derivation. On the representational approach, the representational statement of the subject island does not need to be similarly supplemented by (some equivalent of) the cycle.

Consider a different line of attack. On the representational approach we need to ensure that the trace/copy inside the subject is part of the chain that includes *who* in spec-C and the trace/copy inside the object. But again this is not an extra statement that would correspond to the stipulation of the cycle on the derivational view. If one A-position copy of *who* would be a trace and the other would not be, then the two copies of the subject to object chain of *pictures of who* would not satisfy the identity requirement on chain members that corresponds to the identity requirement of move, which is “copy (identity) and delete” on the derivational view. But properties of move in the derivational theory do not ensure the ungrammaticality of the countercyclic derivation, while given a representational approach, the corresponding properties of chain do.

As noted in Brody (1997a), the account based on PF ordering does not rule out however all violations of the cycle. Since traces are invisible at PF and therefore do not need to be ordered, countercyclic movement or merger of A followed by cyclic raising of A is still incorrectly allowed. The approach allows also lowering rules if followed by cyclic raising - highlighting another aspect in which the derivational theory is less restrictive than the representational.

In the representational theory chains are neutral with respect to lowering, raising, and round trip (lowering followed by raising into the same position) derivations. These distinctions, by now, rather clearly seem empirically unmotivated. Although they could be stipulatively grafted onto a representational theory, the basic concepts of this approach unlike that of the derivational theory do not naturally provide for these unnecessary distinctions.

The reliance of the Epstein's explanation of the cycle on the LCA is also questionable. The status of the LCA as an external stipulation on an otherwise overgenerating derivational system raises the same issues as the cycle. Surely we should prefer a theory in which the basic building blocks of hierarchical relations simply did not permit the types of structures that in standard frameworks we need the LCA to rule out. See Brody (1997b), (1999a) for a theory with this property and also Kayne's (2000) recent work.

In addition to these considerations there is an even more crucial problem with deriving the cycle from (an appropriately constructed) c-command: the notion of c-command has a complexity presented by its asymmetrical nature, so it is probably even more problematic than the cycle that it is called for to explain. See Brody (1997b), (2000a) and below.[18]

3 C-command
3.1 Derivational definition

Epstein pointed out in an influential paper (1995), see also Epstein et al. (1998), that in the cyclic derivational framework of the minimalist approach, c-command can be defined as in (3):[14]

(3) x c-commands all and only the terms of a category y with which x was paired by merge or by move in the course of the derivation

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[14] Sections 3.1 and 3.2 correspond with minor changes to sections of Brody (1997b). For the purposes at hand, "term" can be taken as a synonym of "constituent".

He compared (3) with Reinhart's representational definition, which I restate in (4):

(4) x c-commands y iff
  (a) the first branching node dominating x dominates y, and
  (b) x does not dominate y, and
  (c) x does not equal y

Epstein claimed that the derivational definition in (3) answers certain questions concerning properties of the relation that are “unanswerable given the representational definition of c-command” (p.19 in the ms.). Before looking at this claim, notice that (4) can be made more easily comparable to (3) if it is restated as (5) in a form parallel to (3)[15]:

(5) x c-commands all and only the terms of its sister

He suggests that (2) explains that (a) x appears to c-command whatever the **first** (and not fifth nth etc.) branching node dominating x dominates since “this is the projected node created by pairing of x and y...” Furthermore, x does not c-command (b) the first branching node dominating x, (c) nodes dominated by x and (d) x itself in each case the reason being that x was not paired with the category in question by merge or move during the derivation.

But the derivational definition in (3) appears to give us neither more nor less insight into why these properties characterize c-command than the representational definition in (5). We can say without any loss (or gain) in understanding that x appears to c-command whatever the **first** (and not fifth nth etc.) branching node dominating x dominates since ‘this is the node that dominates (all and only) the terms of x and those of its sister y’. Similarly instead of saying that x does not c-command itself, the nodes dominating it and the nodes it dominates because x was not paired with these we can say without any apparent loss of insight that x does not c-command these because these are not its sisters.

Epstein suggests also that the fact that c-command makes reference to branching can be explained in a framework where “Structure Building (Merge and Move) consists of Pairing, hence it invariably generates binary *branching*.” Again, this point is in fact neutral with respect to the issue of whether syntax should be constructed as a

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[15] Or, if binary branching was not assumed then:
  (5’) x c-commands all and only the terms of its sisters
Note that sisterhood is taken not to be reflexive in (5)/(5’).

representational or derivational system. The assumption that pairing by merge and move is always binary is an additional assumption. There is nothing in the notion of concatenation that would force this operation to always be binary. The syntactic concatenation could in principle operate on any number of elements. This would allow also the unary operation alongside the binary, ternary etc. options. But, just like the concatenation operation can be restricted to be binary, correspondingly, the branching of trees can be restricted to the binary option, ensuring the same result in representational terms: the elimination of non-branching nodes (along with the elimination of other n-ary branching for n not= 2).

Additionally, Epstein argues that the representational definition of c-command is inconsistent with the independently motivated hypothesis of the invisibility of intermediate projections.[16] He considers the example of the category that is the sister to a VP-internal VP-spec subject - I will refer to this as V'. If V' is invisible for the computation of c-command relations then the elements contained in it (the verb and its complement) will c-command the subject and also the categories the subject contains. This is undesirable. On the other hand, Epstein suggests that the situation is different if c-command relations are determined derivationally by (3). Then assuming that the intermediate projection V' can ultimately neither c-command nor be c-commanded (ie. if its c-command relations established by (3) are eliminated) then the subject will asymmetrically c-command the verb and its complement as required by Kayne's LCA. Notice that if V' is fully visible to c-command relations then the subject and V' will symmetrically c-command each other, creating problems for the antisymmetry hypothesis.

Given the assumption of antisymmetry, it seems necessary to assume that V' or, more generally, intermediate projections (or lower adjunction segments) are visible for the computation of c-command relation, but cannot themselves c-command or be commanded. There is nothing however in this state of affairs that would be "incompatible" with a representational view.

Consider, instead, the weaker claim that this behaviour of intermediate projections can be naturally attributed to the assumption that at the point in the derivation where a category becomes an intermediate projection (ie. once it projects further) its c-command relations become invisible (it neither c-commands nor can it be c-commanded) but nevertheless during the earlier derivation it has already participated in determining c-command by other nodes (it counts for the calculation of c-command by these).

[16] In Brody (1998a), I argued that the best hypothesis to explain the invisibility of intermediate projections (for chain theory) is that they do not exist. See also note 12 for references to a later hypothesis ("telescope") that subsumes this one.

The problem with this line of argument is that the interpretation of "becoming invisible" is not antecedently given, it is not any more natural to understand invisibility as entailing only the loss of ability to c-command and be c-commanded than to understand it as the loss of any c-command related role (including the role in the calculation of c-command relations between other nodes). Thus, again, the advantage of the derivational approach is only apparent. The statement that intermediate nodes participate in the calculation of c-command relations by other nodes but they do not participate in c-command relations themselves is not improved upon by saying that this latter property arises at a point in a derivation where the nodes become intermediate nodes /project further.[17]

3.2 Derivational explanation?

The various definitions of c-command - as Epstein notes in connection with his cyclic derivational version - do not explain why c-command exists, they just state its properties. The question remains why certain - or perhaps all - syntactic relations are restricted by c-command? Why cannot categories establish the relation with any other category in the tree? And if the set categories with which a given element can establish a (relevant) relation is to be restricted, why is it restricted precisely in the way the definition of c-command states, rather than in one of the infinitely many other imaginable ways?

Epstein offered an explanation within the cyclic derivational framework he adopted. This is based on two assumptions that he refers to as (a) the first law/the unconnected tree law and (b) the law of pre-existence. The unconnected tree law states that a syntactic relation can only hold between elements that are members of the same tree and excludes relations between elements of unconnected trees. "Derivationally construed", as in (6) (p.25.), it disallows relations between elements that at any point in the derivation were members of different unconnected subtrees.

[17] The problem of intermediate projections will not even arise in the framework of mirror theory referred to in note 12, where no categorial projection exists.

(6) [Epstein's (27)] T_1 can enter into c-command (perhaps more generally, *syntactic*) relation with T_2 only if there exists NO DERIVATIONAL POINT at which:

i) T_1 is a term of K_1 (not=T_1) and

ii) T_2 is a term of K_2 (not=T_2) and

iii) there is no K_3 such that K_1 and K_2 are terms of K_3

Given the cycle, the condition in (6) prevents sideways c-command between two elements x and y. In all such configurations cyclicity allows only derivations in which two unconnected subtrees have been formed at some stage that properly contain x and y respectively.[18]

Notice that "derivationally construed" actually adds another assumption to the unconnected tree law, namely that lack of (c-command) relation at any derivational level freezes and cannot be overridden later:

(7) If there was no (c-command) relation at any given point in the derivation between terms x,y, there cannot be a relation later.

(7) still allows x to have a relation to/c-command y where y c-commands x, since in such a configuration no unconnected subtrees that contain both x and y have been formed.[19] Epstein excludes this configuration by his principle of derivational "pre-existence" (8), that disallows x c-commanding y on the grounds that y was not present when x was introduced.

(8) x cannot bear a relation to y when y is nonexistent

Given the assumption that the lack of a relation at a derivational point cannot be remedied at a later stage, i.e. (7), (8) entails the exclusion of what we might call upward or reverse c-command.

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[18] Note that presupposing the cycle in the explanation of c-command and c-command in the explanation of the cycle (cf. section 2.2 above) makes the explanation of both of these notions circular) in addition to the other problems discussed in the text.

[19] More precisely no two unconnected subtrees have been formed that respectively properly include x and y.

On closer examination, the condition in (6) does not actually explain however the impossibility of sideways relations. The intuitive content of the condition is that two categories unconnected at any point in the derivation cannot enter into a (c-command) relation. But in fact all merged/moved categories were unconnected before merger, still all can c-command the appropriate nodes. In order to allow categories to c-command at all, it is necessary to add the stipulation in (6i,ii) that "K not= T", i.e. that the top node of an unconnected tree does not count as an unconnected element. But this means that "K not=T" in fact just encodes the difference between c-command and lack of it and instead of an explanation we have only another way of stating the c-command configuration.

Epstein comments on the "K not=T" restriction by noting about the top nodes (to be related by merge/move) of the unconnected trees, ie. about K_1 K_2, that "each equals a root node, neither has undergone Merge or Move, hence each is (like a lexical entry) not 'yet' a participant in syntactic relations" (p.26.).[20, 21] In other words, the two instances of the "K not=T" stipulation in (6i) and (6ii) can be exchanged for an additional fourth subclause as in (6'):[22]

(6') T_1 can enter into c-command (perhaps more generally, *syntactic*) relation with T_2 only if there exists NO DERIVATIONAL POINT at which:
- i) T_1 is a term of K_1 and
- ii) T_2 is a term of K_2 and
- iii) there is no K_3 such that K_1 and K_2 are terms of K_3 and
- iv) merge/move has already applied to T_1 and T_2

The intuition (6') expresses is that two terms that are integrated into some subtree by merge/move cannot form a relation if at any point in the derivation **after they have been so integrated** they are unconnected, i.e. they are members of distinct subtrees. With the addition of (6'iv), (6') states that if applying merge/move to two elements x, y does not result in a subtree of which both are terms, then x does not c-command y. So, inverting the conditional, if x c-commands y then merge/move applying to x and y must have resulted in

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[20] More precisely, K1 and K2 have not yet undergone merge or the merge part of move.

[21] Notice that "syntactic relation" here must mean: not yet part of the tree, and not as before, c-command.

[22] Again, read "merge part of move" for "move" in (6').

a subtree that includes both. In other words, either x or y must have been merged with some tree that included the other.

The explanation of the definition in (3) involves then breaking it up into two parts: x c-commands y if neither of the following two situations obtains: (a) there is no derivational point at which x,y have been integrated into unconnected structures and (b) there is no derivational point at which x is present/integrated but y is not. We can now bring the two parts of the account (6') and (8) together again, since in both cases what is crucial, is that there is a derivational point at which a (sub)tree exists into which x is integrated but y is not. But, whether or not we make this improvement, the account provides no evidence for derivations, since it can again be easily restated in representational terms.

Instead of referring to a derivational point at which there is a (sub)tree into which x is integrated but y is not, we can say that x cannot c-command y if in the single syntactic representation there is a subtree which properly contains (ie. contains but is not equal to) x but not y. Instead of rationalizing that all derivational stages must be checked for x-y connection and where no c-command holds there was one at which x was in a (sub)tree that did not contain y, we can presume that all subtrees in the representation must be checked for x-y connection and we have no c-command where we find one in which they are unconnected. (Note also that the representational version is in fact preferable, if the bottom to top derivation and the cycle have no independent motivation (cf. Brody 1997a and the text above), since the derivational account needs to assume these. Furthermore, the easy translatability of the account into non-cyclic representational terms provides some additional evidence against these constructs.) But until we have an explanation of why a relation cannot be established at a later derivational stage that connects the relevant subtrees that were unconnected earlier (or, in representational terms, why the connection must hold in all subtrees), it will remain debatable for both the representational and the derivational versions to what extent the account explains and not just rephrases Reinhart's definition.

In contrast to the clear exposition of the nonexplanatory nature of the definition in (3) in Epstein's paper, this definition is itself sometimes taken to provide a sufficient explanation of c-command. Thus for example Groat (1995) states that, while c-command is arbitrary as a representational definition, "it is explainable as a property of the derivation." Take a configuration like (9), where Z c-commands A B C, A, B does not c-command Z.

(9) Z+$[_C$ A B]

According to Groat this "follows straightforwardly if the relations formed by [merge] are in fact properties of the operation. Z is merged, hence Z is in relation with [$_C$ A B]. A B were not merged with Z, hence they are not in relation with Z".

But notice that we need to decide if merge/move applies to trees or to categories. If the former then in (9) Z merges with C, hence Z does not c-command A and B. If the latter then say [$_Z$ D E] merges with [$_C$ A B], and D and E are incorrectly predicted to c-command A and B. In neither case do we get the desired result. We can of course stipulate c-command again, by saying for example that it is always a category that merges with a tree.

3.3. Domination

The core of the c-command problem is the arbitrary asymmetric conjunction in its definition: x c-commands y iff the following two conditions of somewhat different nature obtain: (a) there is a z that immediately dominates x and (b) z dominates y. It is crucial, but unexplained, that the two subclauses make use of different notions of domination. None of the attempted explanations some of which I just reviewed in the previous section are able to explain this asymmetry.[23] Consider a different approach (Brody 1997b, 1999a). Instead of trying to explain the strange properties of c-command let us assume that no such strange properties exist because, despite appearances, no notion of c-command is part of syntax or more generally of the grammar. Cases where c-command appears to be useful are cases of accidental interplay between two in principle unrelated notions, one of which is domination.

How about the other notion? In standard frameworks this must sometimes be the specifier-head relation and sometimes the head-complement relation. I shall only consider here the specifier-head relation because in the ES representation provided by mirror theory (Brody 1997b, 1999b, 2000a) the head-complement relation reduces to domination. (In mirror theory heads and the associated phrases are not distinguished in the syntactic representation, hence c-command by a head H reduces to domination by H.)

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[23] It is often suggested that c-command follows from the way semantics works but proponents of this view typically do not raise the question of why the semantics they assume has to work in the way that the strange asymmetry of the notion of c-command/scope comes into existence, why this relation must be what it is. So in effect such accounts often restate c-command in semantics but do not attempt to explain its surprising property. In fact as far as I am aware, all attempted explanations in syntax or semantics so far simply define c-command differently and stipulate the asymmetry differently rather than explain it.

Consider a typical condition that refers to c-command like e.g. principle C of the binding theory. Suppose that spec-head agreement has the effect of the head inheriting/sharing the referential/thematic features of its specifier. Then instead of requiring that an R-expression not be c-commanded by a coreferential category we can prohibit the configuration where the R-expression is dominated by an Agr node carrying the same reference.

Similarly the requirement that chain members c-command each other can be straightforwardly restated in terms of domination. Again I ignore head chains here, since in mirror theory their members will be in a strict domination relation with each other. Consider chains that are constructed on potentially larger structures (phrasal chains in standard terms). Assume that the members of these chains always occupy spec positions. Let us think of the heads associated via spec-head relations with the spec positions occupied by the chain members as themselves constituting a chain, call it r(estricted)-chain. (Note that an r-chain is a chain whose members are heads, but it has nothing to do with the head chains expressing the head-chain/movement relation. In mirror theory head chains in this latter sense reduce to morphology and do not exist narrow syntax internally.) It is the domination relation that must hold then between members of r-chains. Additionally and independently we require that r-chains members to have identical or nondistinct specifiers. This is natural since the heads participating in the chain are by virtue of that fact at least in some respects identical, so they will naturally require identical (or at least nondistinct cf. Brody 1997b, 1998b) spec's.[24]

4 Summary

The representational framework seems more restricted than the derivational one in that there are many derivations for a single representations but not conversely. I argued on the empirical grounds of bleeding relations that some of the derivations need to be eliminated to reach descriptive adequacy. Additional asumptions are necessary in the derivational framework that are not entailed by the hypothesis that syntax is derivational. As we have seen the corresponding problems do not arise in the representational framework where the correct consequences follow directly from the representational nature of the system. Additionally I provided arguments against mixed derivational-representational theories of

[24] In Brody (1999a), some empirical advantages of this view are sketched. Additionally, the substitution of domination for c-command may solve the antisymmetry problem of the well motivated instances of c-command from the right, cf. Brody (1997b, 2000a), Brody & Szabolcsi (2000). The latter work elaborates also the idea that semantic scope is similarly a matter of domination.

the kind where derivations and representations essentially duplicate each other's work. I showed that no observationally adequate pure derivational theory can exist, on closer examination derivational theories are mixed theories with derivational-representational duplications, hence arguments against mixed theories hold also against apparently pure derivational theories.

In the second part of the paper, I argued that the derivational explanation of (the asymmetry in) c-command (like all other attempted explanations) is unsuccessful, hence no indirect argument for a derivational approach can be based on it. I suggested that the explanation may be so difficult to find because the complex notion is epiphenomenal only and does not exist within the grammar. I suggested an alternative approach, developed in more detail elsewhere, according to which syntactic principles refer to simple domination instead of c-command and other independently necessary principles unconnected to domination that are involved with spec-head agreement ensure that reference to domination instead of c-command is sufficient.

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