No derivation without representation*

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> *Efficiency is increased as effort is decreased, as though the former approaches infinity as the latter approaches zero, and in the ideal case, which is obviously the impossible promise of Taoism, one should be able by doing nothing to achieve everything*.
>
> —Arthur C. Danto, *Mysticism and Morality*

0.0 A spectre is haunting Minimalism—the spectre of Representationalism. Many (most?) Minimalists seem to agree that Derivationalism is the Way: that in building a syntactic object "step-by-step", everything (syntactically?) useful and important simply falls out of this derivation, and the object being so perfectly and stepwisely built itself has no (syntactic) efficacy.[1] Yet the object, the representation, apparently persists, only now as a ghostly remnant, unable to have effects (in the syntax?).

I hereby suggest that this incapacitation is theoretically premature.

The overall point—and the argument—is conceptual / theoretical. The argument is not that *within* the narrow syntax / computational component / derivation (whatever this may mean exactly) the objects/representations are <u>necessarily</u> potent, but rather that even on the Derivationalist conception they *are*, and that if they are, it should be *odd* (and surprising) that they have *no* effects ever anywhere. Perhaps the narrow syntax / computational component / derivation can get away without talking (much?) about the objects. But whereof one does not speak, does this *thereby* not exist? This seems not merely unparallel in expression, but unlikely in fact.

The chapter itself has two main parts. The first goes over some fairly general considerations about derivations before descending into some more specific discussion of some Derivationalists. Then the focus shifts to a series of appealingly intricate and internecine comments on C-command. A point on which I recurringly harp is that C-command and Representationalism fit together *so very* well. A concomitant point is that a purported major success of Derivationalism in this domain is rather more problematic than often supposed. What is not argued for is the usefulness of C-command. Rather, the argument is that if C-command is judged useful and desirable, then this finding is discommodious for the alleged supernumerary status of representations.

1.0 *Don't stop 'till you get enough*

Let's pretend. Let's pretend there's a Numeration and let's pretend there's Merge.[2]

Now suppose we are deriving the sentence in (1).[3]

(1) The shirt on the floor looks very dirty.

Whatever else might be true, it seems inescapable that Merge has to put [shirt on the floor] together as a unit unto itself. That is, [shirt] can't be Merged with [looks very dirty] and then [on the floor] Merged with that, (nor, for that matter, can [on the floor] be Merged with [looks very dirty] and [shirt] Merged with that). At least, not if Merge is "always at the root" and obeys Extension, and there's strict cyclicity and other Good Things like that.[4] The point here is that Merge is going to have to derive, and the grammar allow for, (complex) structures that are not immediately composed one with another.[5]

Put it this way: if the derivation has got to the point of [looks very dirty], no matter what comes out of the Numeration next [the] or [shirt] or [on] or [the] or [floor], this cannot be Merged with [looks very dirty] if the derivation is going to proceed successfully. Instead, these bits will have to be Merged with one another, then that result Merged with [looks very dirty]. After that, for various Greedy-type reasons[6], these structures will (have to) compose.

And?

Well, this does have *some* implications. One concerns binary branching. If there are going to be little structures that are not (immediately) composed, then it is not clear why all branching must be binary. Merge really cannot be the reason by itself, unless one stipulates that it be what Chametzky (2000) calls Noahistic (two-at-time); once there can be more than two syntactic objects available to Merge, it is an open question whether they <u>must</u> combine two-at-a-time. Perhaps there are independent reasons for Noahism; there had better be, for those who require that it be true.[7]

A different question is this. Why should all these objects compose into one object anyway? Greedy ("purposeful") reasons were assumed above. Good. But what about if there are no such reasons in a particular case? Is that possible? Why not? Suppose, for example, that the Numeration had only [Kim] [leave] [Pat] [arrive] in it. Again, why not? Suppose the first two Merge with each other, and the second two with each other—is there any reason this cannot happen? At this point, there would presumably be no (further) Greedy reasons to Merge more. And what would be wrong? Is there *any* strictly syntactic reason why this would not be a successful derivation?

There is a role for our earlier discussion here: we know that it is independently necessary for there to be syntactic objects around (in a derivation or workspace or …) that are not immediately Merged with one another. So it cannot be that *this* derivation is illicit just because there are objects that have not composed. Aha! The answer, then, *must* be that a derivation cannot end with such uncomposed objects around. But why not? Recall that objects composed on account of Greedy-type requirements of one or the other. But recall, also, that at this point there are no further such requirements, and our question is: what could be wrong with that here? Surely *all* derivations that succeed have the property of lacking further such requirements at their end. Why cannot *this* be the end of the derivation?

The suggestion is that there is nothing in the (narrow) syntax that requires such final composition. But it is not really possible to have [Kim leave] [Pat arrive] be a successful syntactic derivation, is it? *Is it*? Beats me. But it certainly *seems* that that if you have only a derivation and composing driven by (local) syntactic Greed, then you will end up here, like it or not.

So, if you do not want to go there, something else is necessary.[8] Maybe an extra something in the syntax that says: "Oh, and, by the way, all the stuff in Numeration, it has to compose into one Big Object". Or maybe it is not strictly in the syntax, but is "imposed" from the interface, as one says, but still with the same effect: "If you want to cross this line, you have to do it as one Big Object". Psychometricians and their confreres like to talk about "face validity"; maybe Minimalists have to talk about "interface validity", and to have that, syntax has to present a single Big Object to the world.

It is not clear anyone really disagrees with this, when put this way.[9] For example, Epstein etal (1998) talk about this idea, calling it The First Law of Syntax., and they are Derivationalists. But then they want to "construe it derivationally".[10]

But, still, there is something. Various Derivationalists (Chomsky (2001), Epstein and associates, Uriagereka (1999)) want, in various ways, to disenable the final (big) object (as noted at the outset). There is no single "interface" as such, but rather periodic / cyclic / phasic / whateveric smallish packages of syntax that are sent-off and closed-off throughout the derivation. This gets to be fairly complex stuff, and different in its various guises. There are two underlying ideas that seem to be fairly simply stateable, however. One might go this way for a negative reason, or one might do so for a positive reason (or both). The negative reason is a general anti-representationalism: having eliminated *some* levels (viz., DS, SS), now the right and true Minimalist thing to do is to eliminate *all* levels (viz., LF, or its descendent).[11] The positive reason is derivational advocacy: that it would be better for a derivation as a derivation to have such a property, and having it results in there being no role for the Big Object (though there is no prior commitment to eliminating an LF-like level).[12] On the first motivation, the apparent continued requirement for a Big Object seems a matter of regret, while on the latter more one of indifference. On either, it might well be a matter for puzzlement, perhaps even embarrassment.

And so, to reiterate an idea from the preamble: it just seems passing strange that there <u>must</u> be this Big Object, and that it must <u>not</u> be allowed to do more than <u>just</u> be. A system that appears to specially and specifically require an epiphenomenon seems, for that reason, peculiar.

And just to be clear that I'm not imagining things, or still just pretending, it does seem that this is a view that is held. Thus, Epstein & Seely (2006: 178-9, fn.6) write: "It is important to note in this regard that in an optimal derivational model, it shouldn't be merely non-explanatory to define relations on trees or representations. It should be formally impossible to do so."

I, at least, need to pause a bit here.

What can "It should be formally impossible to do so" mean? Can it be that the representations lack any or adequate information to formally define relations? I can't really imagine how that could be. So, presuming there is *something* that could be used to define relations, apparently the optimal derivational theory is, qua optimal, unable to access or use this something. I suppose this is coherent, and I guess it might even be true, and maybe not just by stipulation. But what I fail to see is why this is *desirable*. That is, if the "optimal derivational theory" just does build representations and the representations are (among other things) "information structures", and yet the optimal derivational theory is *in principle* debarred from accessing these structures, why should one *want* the—or only the—optimal derivational theory?

As Epstein & Seely themselves write "Thus, in the rule-based Minimalist approach, iterative application of well-defined transformational rules is assumed….Thus, it would be odd indeed to pay no attention to the form of the rules, *intermediate representations*, and the mode of iterative rule application." (2006: 6; emphasis added). If attention must be paid, well how so, given the optimal derivational theory? And why only to the intermediate representations? Epstein & Seely are explicit that the "end-of-the-line" LF representation or "the final LF representation" has "no special status" and that their model "is a satisfactory alternative…only if all points in the derivation are treated alike" (2006: 180). But surely we are talking symmetric predicates here, ones that cut both ways. We have not merely due process (viz., Derivationalism) but also equal protection: if it would be "odd indeed to pay no attention to…intermediate representations" then it must also be *odd indeed to pay no attention to…the final [LF] representation* given that "all points in the derivation are treated alike" and that "the final LF representation" has "no special status". Or so it would seem.

At various points in their writings, Epstein & Seely assert that Derivationalism is explanatory. I agree, at least sometimes (for example, Chametzky 2000: 155). But they also assert that Representationalism is (always?) nonexplanatory. Here I do disagree, both generally and more particularly.

Generally: I don't know where or how or why the next explanatory whatever is. Nor do they. Nor does anyone else. So there.

Particularly: They write (Epstein & Seely 2006: 7; italics added): "For us, if you define relations on (*or appeal in any other way directly to the macrostructure*) tree representations, you have failed to explain their properties." While one might agree with them that "definitions in general do not explain", the italicized parenthetical in the quote goes rather beyond this point. Other than a commitment to Derivationalism, what could motivate such a blanket statement? And as for definitions, if they themselves are not explanatory, still they have a *role* in explanations—for example, might one not use a definition of, let us say, a "derivation" in constructing an explanatory theory of, let us say, something syntactic (see (4) and (5) below)? But why then is it impossible for a definition that appeals in "any other way directly to the macrostructure" to play an essential role in constructing an explanatory theory of something syntactic? How on earth could one know that this is *in principle* impossible?[13] [14]

One final point before we turn to C-command. Brody (2002) has analyzed derivational and representational theories. He argues—demonstrates, it seems fair to say—that "current (apparently pure) derivational theory is equivalent to a restricted multirepresentational theory…." (2002: 25) That is, there is no pure derivational theory without representation(s).[15] It appears that Epstein & Seely's only response to this is to argue that "representational theories with enriched derivation-encoding representational mechanisms, e.g., trace theory, are thus really 'just' a kind of derivational theory …but, we would suggest, the wrong kind." (2006: 8; see 2002: 6-8) But while this seems to be "just" playing with words, it actually illustrates something about the utility of definitions. If you define "derivation" in a usefully strict way, then the sorts of theories Epstein & Seely dislike are <u>not</u> derivational ones, not even of "the wrong kind". If the sorts of phenomena that exist and the kinds of information required to analyze them are most insightfully captured in derivational theories, then other sorts of theories that analyze these phenomena by *encoding* this information do not *thereby* become "derivational". Rather, they will be theories that, just because they are <u>not</u> derivational, are not the best (kind of) theory. I really do not see any point to Epstein & Seely's assertion.

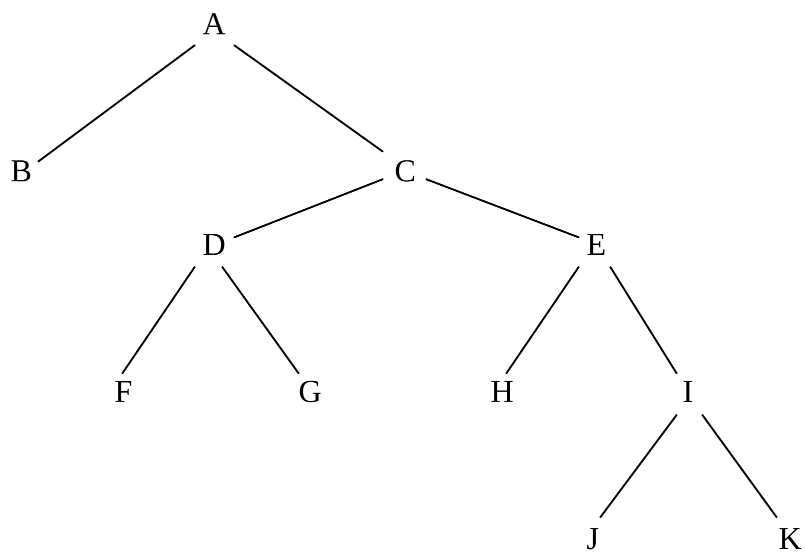
Let's sum up before moving on. Derivations seem to require representations. Trying to say otherwise does not seem to make much sense, and actually saying that representations are there, but in principle impossible to see or use seems to be even worse. Syntax seems to require a Big Object, but (some) syntacticians seem to have Big Objections. It might be nice to understand why a Big Object seems necessary. We turn, therefore, to C-command.

2.0 *If you could see C-command like I can see C-command*

Some 25 years ago, John Richardson had *the* fundamental insight into C-command. Richardson & Chametzky (1985, R&C hereafter) then tried to start an explanatory inquiry into C-command.[16] In the event, nothing much followed from this[17], so Chametzky (1996, 2000) gave it another shot. This has received a little more response, about which presently. First, though, we can refresh our memories about what R&C were trying to do.

The usual question with respect to C-command is “Does node X C-command node Y”? R&C invert this, “taking the point of view of the C-commandee”, asking “What nodes are the C-commanders of some node X”? This has an immediate, and salutary, consequence: the C-commanders of some node X are all and only the nodes which are sisters of all the nodes which dominate X (dominance reflexive). See (2), and (3), G as our “target node”.

(2)



(3) For any node A, the C-commanders of A are all the sisters of every node which dominates A (dominance reflexive).

For G, this returns the set {F, E, B}. This is, evidently, the correct set. C-command is a generalization of the sister relation, complementary to, and parasitic on, dominance.

In R&C it is assumed that Phrase Markers (PM) are totally ordered by the combination of dominance and precedence, and that the Exclusivity Condition holds, so that any pair of nodes is related by either dominance or precedence, but never both. This means that R&C understand C-command to condition linguistic relations between nodes in a precedence relation (because not in a dominance relation). These days, precedence as a syntactic relation and Exclusivity have fallen rather far out of favor.[18] However, on account of those assumptions, R&C call the set of C-commanders the “minimal string” for a given node. Chametzky (1996) renamed it the “minimal factorization”, and that will be asked to do some heavy lifting below.

Anyway, in either case here is what is going on. There is at least one formal relation characterizing an object such as (2), viz., dominance. But there is nothing specifically or peculiarly linguistic about that relation or that object. There are *also* substantive linguistic relations among nodes in a dominance relation, viz. ones having to do with projection (X-bar theory, or its remnants/descendents perhaps). What then about nodes not in a dominance relation? What about substantive linguistic relations among *these* nodes? Well, this is what C-command does: it provides the set of nodes which are not in a dominance relation with some given node and with which that node can be in some substantive linguistic relation or other.[19]

I'm going to go on a bit about this. The minimal string/factorization is minimal in that there is no other set of nodes that both is smaller than (has fewer members, a lesser cardinality) than it and, when unioned with the set containing just the target node, provides a complete, nonredundant constituent analysis of the PM. C-command is the minimal string/factorization of a PM with respect to a target node. There are, of course, other sets of nodes that don't stand in a dominance relation with a target node. Why single out this one?

How about the set of all nodes not in a dominance relation with, say, our target G: {B,F,E,H,I,J,K}. But such a set will generally just ignore the fact that a PM is hierarchically structured, in that it contains nodes which are constituents of other member nodes. If dominance really is basic to syntax, to the structuring of syntactic objects, then we ought to be surprised, and chagrined, to find it utterly ignored in this way: if our inquiries suggest that such a set (relation), which is entirely indifferent to the dominance-induced structure, is the most useful one, then our original commitment to dominance is thereby undermined.

Going to the other extreme, how about the smallest set of nodes relatable to a target by dominance but not in a dominance relation to it, viz., the set of a target's sisters? This is a good set, but, in a sense, too good: it's just too constrained for our initial goal of finding a set of candidate nodes in the PM for further substantive relations with a given node. Grammar seems to have an "incest-only taboo": it is just *obviously false* that a given node can only have substantive linguistic relations with its sister(s).[20] What we really want is a set that both is relatable to our target by means of dominance, though there is no dominance relation between the target and any set member, and which utilizes the full PM while respecting the hierarchical structure induced by dominance.

Looking at (2), there are really only three candidates with respect to G: {B,F,E}, {B,F,H,I}, and {B,G,H,J,K}. Only these sets provide complete, nonredundant analyses of the PM with respect to G. {B,F,H,I} is just an arbitrary analysis, but the other two are distinguished: {B,F,E} is minimal and {B,G,H,J,K} is maximal. But the maximal set has a now familiar problem: it denies the relevance of the full hierarchical structure induced by dominance: it is just he set of (pre)terminals not in a dominance relation with the target G. In fact, it is arguably worse on this score than the set of all nodes not in a dominance relation with G. That set *ignores* the dominance induced structure, but at least redundantly contains all the nodes; this set *denies* the structure, entirely leaving out the nodes indicating hierarchical structure. In other words, the minimal string/factorization set is the only nonarbitrary set which requires and respects the full branching hierarchical structure induced by dominance on a PM.

The chapter in Chametzky (1996) that said all this is called the "The explanation of C-command". It's still an appropriate title. If this ain't explanation, well, it'll have to do, until the real thing comes along.[21] One final bit of stage-setting, and then we can see whether the real thing *has* come along, as we compare R&C with the "derivational explanation of C-command".

Brody (2002: 27-33; 2003: 195-99) has analyzed—eviscerated might be better—the derivational explanation of C-command in some detail. I shall pick over the bones myself in a bit, but do not rehearse Brody's performance here. Instead, I want to draw attention to what he calls "the core of the c-command problem" (2002: 32; 2003: 198):

> 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.

Let's now recall our own (3)

(3) For any node A, the C-commanders of A are all the sisters of every node which dominates A (dominance reflexive).

Notice that we don't have two subclauses or two notions of domination. All we have is dominance, giving us the generalization of the sister relation. This is because RC takes the point of view of the C-commandee. So, it appears that the core of the C-command problem just goes away. But maybe not. Maybe Brody would say that there are still two different relations there in (3): dominance and sisterhood. And that's *still* unprincipled and arbitrary, even if worded so as not to be exactly an asymmetric conjunction. But also notice this: if syntactic objects are hierarchically structured, then there's *no way* to avoid either the dominance relation or sisterhood. There is nothing any more basic than these when dealing with hierarchical structure. So, if there is going to *be* another relation, these are what you'd really, really want it to come directly from; and if you can't have even *that* relation with *that* provenance, then what *can* you have?[22]

There's something here worth talking about. It seems that if there is going to be a C-command relation, it will have to have more than one aspect to it: what could it mean to say that there is C-command, but it's *only* dominance. Or *only* sisterhood? To be a *new* relation, it has to have something about it that is *different*, after all. So, what Brody appears to be objecting to, really, is just there being a *new* relation. And, indeed, when he gets around to his own proposal, it turns out to be precisely that: there isn't any C-command. There is, instead, "the accidental interplay between two (in principle unrelated) notions, one of which is domination." The other is the specifier-head relation or the head-complement relation. (Brody 2002: 32-33; 2003: 198, 226-27). Maybe Brody is right about this. I don't know; that's his agenda. But let's be clear that his objection to C-command is really no objection once we understand C-command as in (3). C-command is a generalization of the sister relation, and that's as *unnew* as a new relation can get. Maybe it's unnecessary. But it's <u>not</u> illegitimate.

Now we can ask whether R&C's "representational view of C-command" has been overtaken and surpassed by the "derivational explanation of C-command" of Epstein et al (1998). This is germane to our larger concerns because if it has <u>not</u> been, then there is, apparently, at least *this* much work for a representation still to do.

3.0 *If you build it, will they C-command?*

EGKK = *A derivational approach to syntactic relations* (Epstein et al 1998*)*

TPM = *A theory of phrase markers and the extended base* (Chametzky 1996*)*

EGKK have two criticisms of TPM (p.174).[23] The first is that the maximal factorization of a PM **does** require the full branching hierarchical structure of the PM, contra TPM. The second is that the concept “minimal factorization” is basically ad hoc.

For the first, they are wrong. At least, they are wrong given what is intended, though perhaps not clearly enough conveyed, in TPM. The argument there is that the maximal factorization is just the set of (pre)terminals not dominated by the target node (TPM: 31). The point is that this set does not require that there be any hierarchical structure in a sentence; it is compatible with a “flat structure” in which all these (pre)terminals are daughters of the mother node of the representation. EGKK (p. 174) write “the factorization of any phrase-marker requires the existence of a phrase-marker to factorize, in which *all* nodes are in a Dominance relation with (at least) the mother node of the representation.” Fine. But the point about the maximal set is that it is consistent with their parenthetical “(at least)” being *at most*, and if the maximal set is what is called for analytically, this can be seen as evidence that sentences are <u>not</u> hierarchically structured, because, as noted, this set is consistent with the lack of such structure. EGKK seem to be already assuming that there is hierarchical branching structure; this is a perfectly good assumption, maybe a true one. But it is <u>not</u> one which the maximal set requires or necessarily leads one to embrace. This is the point of the claim in TPM. So, while there is a sense in which EGKK are right—the maximal factorization does require a PM—it is a sense that misses the point—PMs might, in general, have no hierarchical branching structure as far as the maximal factorization is concerned. The second objection is that

> the notion of a factorization is needed to explain the naturalness of the representational definition of C-command, but to the best of our knowledge for nothing else. Though a factorization is easily defined, its definition serves only to facilitate a particular outlook on C-command; no notion of factorization is required independently. (EGKK: 174)

This is their big complaint. Let's grant that it *is* big. Let's also grant it for now, and look instead at EGKK's derivational approach.

They define—notice—three things: (4) a derivation and dominance (pp. 167-8, their (3)), and (5) C-command (p.170, their (5)) The important point is that the definitions for Dominance and C-command are identical, except that "input" appears in (5i) where "output" appears in (4bi). This exact parallelism is quite striking.

(4) a. Definition of Derivation

A *derivation* D is a pair $<O,M>$, where

i. O is set of operations $\{o_1, o_2, \ldots o_n\}$ (Merge and Select) on a set S of lexical items in a Numeration, and terms formed by those operations,

and ii. M is a set of pairs of the form $< o_i, o_j>$, meaning o_i "must follow" o_j. D is transitive, irreflexive, and antisymmetric (a quasi order).

b. Definition of Dominance

Given a derivation D = $<O,M>$, let X, Y $\in$ S.

Then X *dominates* Y iff

i. X is the output of some $o_i \in$ O. (*we consider outputs*) and ii. X is not in a relation with Y in any

proper subderivation D' of D (*the relation is "new"*)

and iii. Y is member of some $o_j \in O$ such that $< o_i, o_j > \in M$ (*the terms are in a relation*

only if the operations are)

(5) Definition of C-command

Given a derivation $D = <O,M>$, let $X, Y \in S$.

Then X *C-commands* Y iff

i. X is the input of some $o_i \in O$. (*we consider inputs*)

and ii. X is not in a relation with Y in any

proper subderivation D' of D (*the relation is "new"*)

and iii. Y is member of some $o_j \in O$ such that $< o_i, o_j > \in M$ (*the terms are in a relation*

only if the operations are)

However, EGKK note that, in actual fact, (4bii) is redundant (p.168). This being so, we should rewrite it without the redundancy as in (6).

(6) Definition of Dominance

Given a derivation $D = <O,M>$, let $X, Y \in S$.

Then X *dominates* Y iff

i. X is the output of some $o_i \in O$. (*we consider outputs*)

and ii. Y is member of some $o_j \in O$ such that $< o_i, o_j > \in M$ (*the terms are in a relation*

only if the operations are)

Now two points. First, of course, Dominance and C-command are no longer exactly parallel in their definitions. Second

> the notion of a ‘new relation’ is needed to explain the naturalness of the *derivational* definition of C-command, but to the best of our knowledge for nothing else. Though a ‘new relation’ is easily defined, its definition serves only to facilitate a particular outlook on C-command; no notion of ‘new relation’ is required independently.

Now where have we heard something like *this* before?

Soooo, once you untrick their definitions, they are guilty of *exactly* the worst sin they locate in the representational account of C-command in TPM. This is what the philosopher G.A.Cohen has called a “look who's talking argument”. It isn't that the point being made is, as such, a bad one; it’s that, for various reasons, the *person/persons making it are especially badly situated to be bringing it forward*. [24]

It seems, then, that, at best, there’s a stand-off here. But maybe not. First, a smallish, nearly empirical point. EGKK notice that their C-command is reflexive (p. 179, fn.7). They point out that “with respect to semantic interpretation, no category is ever ‘dependent on itself’ for interpretation . . . .” They say this is no problem. But isn’t it? After all, if, e.g., binding domains and relations are licensed by C-command, why shouldn’t, say, an anaphor be its own binder given this reflexive C-command? Moreover, it was argued in R&C, that C-command is antireflexive and nonsymmetric, and this led them to derive (7) (their (38)),

(7) All predicates which contain C-command as a necessary condition for their satisfaction will be antireflexive and nonsymmetric.

Moving along, EGKK say that all one can do, after looking at outputs of operations, as in their Dominance, is look at inputs, as in their C-command “if we are to conceive of intercategorial relations as properties of operations (rule-applications) in a derivation. . . .” (p.169) But why should we do *that*? If there *were* the strict parallelism between Dominance and C-command that they try to palm off, that *would* be a reason, no doubt. *But there isn’t*. So, what’s left—other than an a priori commitment to Derivationalism?

Well, how about the conceptual underpinning for their Dominance definition, viz., “The First Law of Syntax”, that everything has to get together as one syntactic object. But this does not require that “inputs” be looked at; once you’ve got outputs/Dominance, you do not need C-command for the First Law to be in effect. And this just leads back to the conclusion that really Dominance and C-command are not on a par, unlike what EGKK want us to believe.

A bit of stock taking: if the representation is being built anyway, to not *allow* it some role is for that reason to *make* the theory conceptually worse than it ought to be, with, as we have now seen, no compensating theoretical advantage with respect to new, special purpose notions. A further point in the R&C approach's favor, not mentioned elsewhere[25] is that by taking the viewpoint of the C-commandee, you align the relation with some (most? all?) of its significant applications, e.g., it is anaphors or pronominals or traces or predicates that have a “be C-commanded (or not)” requirement on them—it’s not that there are some inherent binders that have a “C-commanding” requirement on them.

But all of this is, surely, beside the point. What EGKK have done is *nonsense*. They have taken a *name* “minimal factorization” and mistaken it for some kind of *essence*, or at least a (significant) *concept*. The important *idea*, the point, is that C-command is a generalization of the sister relation. The set in question has been given a couple of different names, in order to facilitate discussion, and, so it was naively hoped, understanding. But the *name* is really quite irrelevant. Maybe this isn't as clear as it should be in R&C, or in Chametzky (1996 or 2000). Maybe; but I think it is there.

Now, the deep puzzle about (almost) all derivational approaches to syntax is this. Why is there what EGKK call "cyclic structure-building" if the resultant built structure is going to be syntactically impotent? As I've stressed, isn't it *odd* to build this object and yet not allow it any positive role? Shouldn't we expect the structure so built to do *something*? And the basic problem for (almost) all derivational approaches to C-command is this. *It is a representational relation*. EGKK concede as much when they say they "are looking for relations between *terms*, such as C-command…." (p.165). Their attempt to squeeze C-command out of their derivational approach is valiant, but it leaks. It illustrates the principle I like to call "if all you have is a hammer, everything looks like a thumb."[26]

These problems are related, of course. Once you accept that there is the Big Object, then C-command ceases to be a mystery. Not a *necessity*, surely, but, as I've harped on, if there are to be other than dominance mediated substantive linguistic relations, then *lack* of C-command would be more in need of explanation. Indeed, if there's no peeking at the whole Big Object, then there's just no reason for C-command, EGKK to the contrary notwithstanding. And the idea of *peeking* is a suggestive one. If where the peeking is done is "from the interface(s)", where presumably the whole Big Object could be available, and what gets peeked at are individual constituents that may or may not have needs to be met (i.e., are dependents, require some kind of licensing, whatever), then it does seem that this particular key-hole would naturally reveal the "minimal factorization" from "the point of view of the C-commandee". We are ready now for our leave-taking.

4.0 *But what would Zeno say?*

As noted above,[27] there's a very widespread idea that grammars are essentially "local", in the sense that what's really involved are basically just mother-daughter/sisterhood relations. Proponents of such views (and they come in various guises) then find themselves suggesting various ways to "string together" their favored form of baby step to make a long march. But why? Why shouldn't grammar be satisfied with just what, on these views, are the basics/essentials? That is, are there *any* grammars of *any* languages that do <u>not</u> seem to manifest *any* nonlocal dependencies? *If locality is truly the be-all, why isn't it (ever) also an end-all?* Minimalists especially ought to wonder.

I am not aware that anyone has come up with a good answer to this question—but then I am not sure anyone has bothered to ask it, either—and I'm not betting that there will (can?) be one. What we see instead are various after-the-fact rationalizations for stringing together the strictly local bits in order to graft onto these treelet collections a result from a differently premised approach. If grammar isn't 100% local, a (the?) "first step beyond" is the generalization of sisterhood advocated in R&C. It is at least arguable that just about everything interesting in grammar is tidied up fine with this first step beyond. Why fight it? What's the point? But, if you accept that C-command (= the generalization of sisterhood) is a real part of grammar, then you're likely stuck with the

Big Object. They go together: if there is a Big Object, you'd expect C-command; if there is C-command, you need a Big Object.

There only other viable option, as far as I can see, is to deny that C-command is in fact relevant to grammar. There are two ways to do this. One is to deny that this is the right kind of grammar and to build a different kind. The other is to keep the kind of grammar but to deny C-command. Within broadly Minimalist approaches, Brody (2002, 2003), Hornstein (2009) in different ways take the latter course, while Collins & Ura (ms.) take the former.[28] Either of these is actually ok by me. Evaluating these positions is more a matter of the best analyses, I think, than one of theory, per se, so I have no ideas or recriminations to contribute.[29]

The Big Picture, then, is just this: it's kind of impossible to make much sense of a "purely derivational" approach to syntax. And insofar as one tries to, as it were, asymptotically approach that as an ideal, one finds that progress slows, conceptual puzzles arise, and confusions mount, which aren't usually considered hallmarks of a promising set of initial assumptions. And yet, despite Brody's (2002) scolding of those who advocate "mixed theories" that are both representational and derivational,[30] I really have no objection to Hornstein & Uriagereka's (2002: 106) suggestion/conjecture "that grammars are (at least in part) derivational systems."[31] And just imagine how painful it must be to publicly end on so *conciliatory* a note.

Notes

[1] Boeckx (2008) is a prominent exception, purveying a mixed, derivational-cum-representational Minimalism.

[2] For my views on how much pretending would be necessary, see (Chametzky 2000).

[3] Here and throughout, I ignore functional heads and categories; this is both convenient and, quite possibly, correct (Chametzky 2003).

[4] There is a point not being made here, viz., that subjects can be arbitrarily complex syntactically—and, indeed, the fact that a subject might contain, e.g., a *tough*-movement construction was one of the central empirical facts motivating First Wave Minimalism and the Over Throw of DS—that is nonetheless worth noting in passing (or passing in a note).

[5] de Vries (2009: 346) refers to an "auxiliary derivation" when noting this fact.

[6] In a more current idiom: "all rule application is purposeful" (Epstein & Seely 2006: 5).

[7] John Collins (p.c.) has addressed some of this: "This is difficult, but at a first stab, I'd say that this [nonbinary branching/Merge] would make merge non-uniform, since we know that binary branching is fine for say verbs and their objects, etc. Non-uniformity or a lack of symmetry is kind of an imperfection. Also, given that branching is required, binary is the most economical, as only two things at most need to be in the workspace, as it were. So, perfection is the most economical and uniform meeting of interface conditions by the most general operations we find throughout nature. The interfaces rule out singletons and the empty set, and general uniformity/economy considerations go for binary, given that there must be branching for composition (lack of composition would be

an imperfection as set formation would need to be restricted). Thus, something other than binary would thus be an imperfection, as far as we can tell."

[8] We could perhaps call it the Wobbly or IWW principle: "One big union".

[9] So, de Vries (2009: 357) notes in passing that Merge applies "until a final single-rooted structure is created." On the other hand, linguistics seems to require that every possible position be occupied (as well as every impossible one).

[10] Good luck.

[11] I wonder about this some. I don't see that there are the sorts of conceptual arguments contra an LFish thing that there were vis a vis DS and SS. So, the argument will have to be pretty much that one *can* do without such a thing, and with some advantage empirically. I simply do not know about this, though I am curious with respect to, e.g., the treatment of inversely linked quantificational sentences.

[12] The idea, perhaps, is a kind of uniformity: if there are syntactic "steps" in the "step-by-step" derivation, then each step should define its own complete little universe, so to speak, not just a bit of Merging. See Boeckx (2008) for some discussion.

[13] Well, they do say "for us"; maybe Iowa isn't heaven because Michigan (apparently) is.

[14] They invoke (2006:7) Joshua Epstein's slogan for "generative social science": "if you haven't grown it, you haven't explained it." This is actually a misquotation of J.Epstein (1999: 43), who writes "if you didn't grow it, you didn't explain its emergence." They do this also in their (2002: 5). This, I suppose, is a liberté licensed by fraternité, so we could let it pass. On the other hand, I have a brother who is an art historian, and so I could probably find something positive about "representations" in his work, if I looked hard enough. But so what? And further, J.Epstein (1999: 46) also characterizes this work as

"connectionist social science": "distributed, asynchronous, and decentralized and hav[ing] endogenous dynamic connection topologies" a "social neural net". And the actual phenomena studied, and the models used, have both a spatial and a temporal aspect that are each crucial. Perhaps the analogy still holds, or perhaps not. Additional irrelevant biographical detail: the Chametzky and Epstein brothers have known one another for over forty years.

[15] He also (2002: 27-33) dismantles some of the explanatory pretensions of the derivational approach to C-command, about which more directly.

[16] This maybe isn't *quite* right. We would have been happy enough if everyone had simply acknowledged we had explained C-command. And Kayne (1981) did ask the right question; see Chametzky (1996) for why his answer is wrong.

[17] This maybe isn't *quite* right, either. John does honorable work in animal rescue.

[18] Though I understand why this has happened (I think), and have even made some of these sorts of arguments myself, I'm not totally convinced that precedence is not syntactic. But we need not worry that here and now. My own arguments, for those who care, are in (Chametzky 1995 and 1996). I do not actually argue that precedence is not syntactic.

[19] de Vries (2009: 367) appears to have discovered this: "essentially, it [C-command] identifies possible dependencies."

[20] One can, however, try to make it true, and a sizable amount of syntax is devoted to trying to, the idea being that all nonlocal relations are actually composed of linked local ones. See Section 4.0 below for some comment.

[21] What we've got is not, afterall, nomic necessity or whatever, and, if, as per current phylotaxistic longings, someone can come up with a way to explain C-command using the Fibonacci series (Medeiros 2008, Soschen 2008), I'll bow out gracefully. I'm proud, but not stubborn. Also not losing lots of sleep.

[22] I guess this is what lies under Chomsky's (2000) attempt to derive or explain C-command by means of "the elementary operation of composition of relations" operating on *sisterhood* and *immediately contain* (actually, on *contain*, the transitive closure of immediately contain). I've denigrated this attempt elsewhere (2003: 200-201), and while I certainly don't mind repeating myself, I'll forbear this once. It's enough to remark that from our perspective this looks like an unwitting attempt to arrive at the fundamental insight of R&C.

[23] There's also something on pp.106-7, but it's not important.

[24] Famously: Dear Pot, You're black. Signed, Kettle

[25] It almost made it into R&C, but that was way overlong already.

[26] The parenthetical "almost"s are due to Collins & Ura (2001). See footnote 28.

[27] See footnote 20.

[28] Collins & Ura give up structure building and the phrase structure representation, and offer a "search algorithm" analogue of C-command. The problem here is that it's not clear that anyone would ever come up with such a thing expect as a reconstruction of already existing, essentially classical C-command.

[29] This zero is just with reference to Minimalist approaches; for other approaches, I would have to begin with negative contributions.

[30] Brody seems to play the severely responsible Confucian to the standard Minimalist's blithely wandering Taoist.

[31] As pointed out in footnote 1, Boeckx (2008) tries to synthesize the derivational/representational thesis/antithesis.

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