

Part Two

In these constructions another possible derivation has so far been ignored. Consider the passive counterpart (21) of (16), and suppose that Agree holds of (T, EXPL) while Move applies to (T, DO), giving (iii):

- (21) (i) C [T be expected [EXPL to-arrive [_{DO} a man]]]
(ii) there is expected to arrive a man
(iii) *a man is expected there to arrive

We have just seen that deleted features remain visible until the strong phase level; hence the [person] feature of EXPL is visible throughout the computation of (21).

So these features remain active, regardless of Case considerations.

The agreement facts show that EXPL does not induce an intervention effect, blocking Match of (T, man); but EXPL does intervene to bar raising of DO. The apparent contradiction is resolved by the maximization principle (14). The first matching pair detected in the computation is (T, EXPL). The pairing can delete the [person] feature of EXPL and the EPP-feature of T; and under (14) it therefore must do both, inducing raising of EXPL and hence allowing (T, DO) Match/Agree under (17), as discussed.

This is the beginning of a very interesting discussion, which has been postponed for a long time. Chomsky finally gives us arguments for his view that the normal order of participials and expletive associates is with the associate last, despite appearances.

For English, the unwanted raising of DO in (21) is barred for idiosyncratic reasons that we have so far put aside; also in MI (note 40). As is well-known, unaccusative constructions such as (15)-(16), (18)-(21) are awkward in English. More accurately, they are barred, as we see from such constructions as (22)¹:

It's not clear to me that these have the same status. My judgements, for what it's worth; (i)(?); (ii) ?; (iii) ?*; (iv) *, (v) *. In other words, I do find the questions pretty bad, but the affirmative ones seem fine, especially if you set them in a narrative/rhetorical context, as in telling a story to your child.

- (22)(i) *there came several angry men into the room
(ii) *there arrived a strange package in the mail
(iii) *there was placed a large book on the table
(iv) *how many packages did there arrive in the mail
(v) *how many packages were there placed on the table

Comparable constructions are grammatical in similar languages, for example, Italian (where the in-situ position of the DO is also revealed by ne-cliticization) or Dutch, which has an overt EXPL and offers word-by-word translations:

- (23) hoeveel mensen zijn er aangekomen
(how many men did there arrive)

¹ On the English-Italian contrast with regard to (iii), see Lasnik (1999), who develops a different approach.

This is certainly true, well-taken, and definitely significant.

The gap in the English paradigm is filled by idiosyncratic constructions such as (24) that are either fully or partially acceptable ((i) and (ii), respectively):

- (24) (i) there were several packages placed on the table
(ii) there were placed on the table several (large) packages

I'm not sure what this notion of 'filling a gap in the English paradigm' means.

Examples (15)-(16) and (18)-(21) are presumably of the same type as (ii), with invisible extraposition.

That's going to be the crux of the argument.

Constructions of type (i) run counter to a general tendency among the Germanic languages: at one extreme, English permits few word order options, while at the other, Icelandic permits many; but English permits (i) while Icelandic does not.

This is too oversimplified, to the point where it might mislead. What I'm about to say is irrespective of whether an extraposition analysis for English examples is correct. I think the relevant generalizations within Germanic are nicely given by Holmberg: if the language has participial agreement, the associate comes before the participle; not otherwise. However, things get more complicated the moment you introduce Romance into the picture. Even when a language has participial agreement, you may get the associate post-verbally *if the verb also exhibits agreement*. In contrast, if the verb doesn't, you get the opposite order. For instance, the Spanish contrast:

- (i) a. Esta arrestada mucha gente.
IS-AGR arrested-AGR many people

b. Hay mucha gente arrestada.
Have many people arrested

You could argue that when the verb has no agreement (as in French) and the participial does not exhibit systematic agreement either (recall the comparison with Catalan), you get the order participial, associate. The rule of thumb: Agreement in BOTH verb and participial or in NEITHER results in the <PART, ASSOCIATE> order. Agreement in EITHER one of those, verb or participial, results in the <ASSOCIATE, PART> order. It's no simpler than that. Icelandic, incidentally, should give you the order <PART, ASSOCIATE>, as it has agreement in both verb and participial. But as Chomsky said, in Icelandic you get many orders, possibly as a consequence of further movements (unique even within Scandinavian languages). Most Scandinavian languages are of the <PART, ASSOCIATE> type, since they have agreement in neither verb nor participial; in Scandinavian languages with participial agreement (often internal dialects) and still no agreement in the verb you get the reverse order, as expected from the description. Obviously, that's not an explanation, just a statement of facts.

It seems, then, that English bars surface structures of the form [V-DO], where the construction is unaccusative/passive. In such cases, DO is extracted to the edge of the construction by an obligatory "Thematization/Extraction" rule TH/EX. The operation is reminiscent of normal displacement of subject

and of object, both to edge positions, but it differs in not yielding the usual surface-semantic effects (specificity, etc.).

The phenomenon may be more general. As has occasionally been discussed, there seems to be some condition that has the consequences (25):

(25) In transitive constructions, something most escape the $\bar{v}P$

Furthermore, as Richard Kayne has observed, English marginally allows a kind of transitive expletive construction (TEC) if the subject is displaced to the right, as in (26):

(26) (i) there entered the room a strange man
(ii) there hit the stands a new journal

The idiosyncratic properties of TH/EX might lead us to suspect that (27):

(27) TH/EX is an operation of the phonological component

That's the key proposal that Chomsky wants to explore here, and regardless of whether the expletive-associate relations are good instances of this, the idea of (27) is interesting.

At the relevant stage of the cycle, the syntactic object " so far constructed is transferred to the phonological component for application of TH/EX.

Why? Because for some reason 'English bars surface structures of the form [V-DO], where the construction is unaccusative/passive', perhaps something related to (25).

The narrow-syntactic computation then proceeds on course with " unchanged except that the trace of TH/EX is phonologically empty even prior to the strong phase level, at which point the position would have become phonologically empty even if not subject to TH/EX.²

'Phonologically empty' trace is a trace in the old sense, or no trace at all -at any rate something that precludes reconstruction or any form of semantic interpretation. This is a stylistic process, and see fn. 2.

The English constructions reach LF in the same form as in similar languages, as we would expect if LF-external systems of interpretation are essentially language-independent and prefer the LF interface to be as uniform as possible across languages -- a particularly natural case of the general uniformity principle (1), given the virtual absence of evidence about these systems for language acquisition.

A closer look tends to support (27). One relevant consideration is that TH/EX, leftward or rightward,

Keep that in mind, as it is confusing at first: TH/EX can go in either direction, although I don't know under what circumstances.

is incompatible with independent movement of the extracted nominal EN, as we see in (28) ((i),(ii) = (22iv,v)):

² Note that this amounts to highly limited access of narrow syntax to effects of the phonological component.

- (28) (i) *how many packages did there arrive in the mail
(ii) *how many packages were there placed on the table
(iii) *how many men did there enter the room
(iv) *how many journals did there hit the stands

(24) and (26) must be incompatible with wh-movement, or (28) would be permitted.³ **[important footnote]** Similarly, (29i) is much worse than (ii), indicating that (i) has undergone (invisible) rightward extraposition, again barring wh-movement:

- (29) (i) *how many men did there arrive
(ii) ?there arrived three men

I'm slightly troubled with the question mark awarded to (29ii), as compared to the start given to (22i) or (22ii), all repeated:

- (i) a. ?There arrived three men.
b. *There came several angry men into the room.
c. *There arrived a strange package in the mail.

Is the implicit suggestion that (ia-29ii) is much better than (ib-22i) or (ic-22ii)? In particular, the comparison between (ia) and (ic) confuses me. What could possibly be the difference? Animacy? The length of the postverbal material? I'd say that all of these have roughly the same status, marginal in normal discourse and fine in narratives. But if so one has to qualify the comment that 'more accurately they are barred'. More accurately, they seem less bad, anyway, than corresponding instances with extraction, as (29i) above.

The incompatibility is not between TH/EX and wh-movement. Thus in (30) both operations apply⁴:

- (30) (i) to whom was there a present given
(ii) ?at which airport did there arrive three strange men

Rather, the two operations cannot apply to the same phrase.

Why does the English-specific rule TH/EX, which extracts DO either to left or right, bar wh-movement? A preliminary question is whether the position of EN (= DO) is immune to all syntactic operations, for example, wh-movement from within EN. Compare the examples (31):

- (31)(i) what are they selling books about t (in Boston these days)
(ii) *what are there books about t being sold (in Boston these days)

³ Compare (ii) with "how many packages were there lying on the table," much more acceptable (as is (ii), if interpreted as involving stative/adjectival passive), indicating that the status of (ii) is not simply the result of incomplete internal constituent constraints on movement (Kuno 1973).

⁴ Extraction from within PP is of course barred here (*"which airport did there arrive at t three strange men" vs. "which airport did three strange men arrive at"), presumably an illustration of constraints of the preceding note. If TH/EX belongs to the phonological component, then these effects hold within that component as well, a conclusion about "surface effects" that is consistent with (13).

The contrast rules out one pattern of derivation for (31ii): given the base form (32), apply wh-movement to what within EN (as in (31i)) and then apply TH/EX to EN including the trace of what, yielding (31ii):

(32) there are being sold [_{EN} books about what] (in Boston these days)

If that were possible, the two cases of (31) should be on a par. We conclude that the base position of EN is completely inaccessible to wh-movement, either as a whole or in part.

It remains to determine whether the EN output of TH/EX is accessible to wh-movement. We know that the whole EN is not. The natural expectation is that no part of EN is either, in which case, the contrast in (31) cannot be attributed simply to the degradation of movement from an internal phrase (see note 40 [here, 3]). The examples (33) provide some evidence that the expected conclusion is correct:

- (33)
- (i) ?who did they deliver to your office a picture of t
 - (ii) *who was there delivered to your office a picture of t
 - (iii) ?there arrived in the mail some books about global warming
 - (iv) *what did there arrive in the mail some books about t
 - (v) ?what topics were there some books about t selling in Boston stores
 - (vi) *what topics were there some books about t (being) sold in Boston stores
 - (vii) ?he's the guy there were lots of songs about t playing on the radio
 - (viii) *he's the guy there were lots of songs about t (being) played on the radio

Note that (33v) vs. (33vi), and (33vii) vs. (33viii) all involve expletive-associate constructions, but presumably only in (33vi) and (33viii) would TH/EX be involved. I think this is where ‘the data are less sharp than one would like’; in particular, it would seem as if both sets of examples are pretty degraded. I’m not sure I can tell which is more degraded. Remember, all these constructions are marginal to start with (more accurately, ‘barred...’) and for some reason extraction when involving a real verb (as opposed to copulative constructions, see below) are even worse; to decide whether there are to different degrees of badness -‘mostly dead’ for instances with simple extraction, and ‘completely dead’ for instances with simple extraction when there has been TH/EX as well- is too subtle even for me.

The data are less sharp than one would like, but the differences between ENs and their non-EN counterparts seem fairly clear. A reasonable conclusion, then, is that the output of the TH/EX operation is inaccessible to syntactic rules entirely, just as the input is -- either prior to TH/EX or by application to the trace of this operation.

Before we go any further, I must point out also how things work in other languages. Holmberg notes that in Scandinavian languages Wh-movement across the expletive is possible if the participle does not agree with the argument (in other words, if the associate is not in preverbal position). That is not inconsistent with Chomsky’s position, since after all he claims that (33v) and (33vii) are good even in English; what makes (33vi) and (33viii) bad for him is the fact that they involve TH/EX, or in Holmberg’s terms, they are ‘postverbal’ (it’s slightly more complex than that, since for Chomsky TH/EX need not be postverbal, but let’s set that aside now). Consider also Spanish data.

(i) SWEDISH

- a. Hur manga bocker blev det skrivet det aret?
how many books was EX written that year
- b. *Hur manga bocker blev det skrivna det aret?
how many books was EX written.AGR that year

(ii) SPANISH

- a. Cuantos libros estaban ya escritos en esa epoca?
how-many books were.AGR already written.AGR in that epoch
- b. ??? Cuantos libros habia ya escritos en esa epoca?
how-many books have already written.AGR in that epoch
- c. De que libro estaban ya escritas [varias criticas t]
of what book were.AGR already written.AGR several critiques
- d. ??? De que libro habia ya [varias criticas t] escritas
of what book were already several critiques written.AGR

Recall first that in Spanish you always have agreement in the participial, but you may or may not have agreement in a presentational verb; when you do not, the normal order of the associate is not final, whereas when there is agreement across the board, the participial is final. Now, extraction across the ‘inconsistent agreement’ instance is degraded, whether it is the entire associate that extracts (iib) or a part of it (iid). In this instance, it is not clear we can blame this fact on a mere TH/EX process, unless we are willing to say that in Spanish TH/EX displaces the associate to the left, whereas normally it would be to the right. It is curious to note that Spanish behaves like Swedish with regards to where it allows extraction: in the post-verbal instance. This is irrespective of whether that instance agrees with something to the left; it does not at all in Swedish, but it does across the board in Spanish (with both participial and main verb).

Other operations are not barred for EN. Thus EN still has to satisfy the Case Filter, which in our terms means that it is accessible to Agree, either in its base or extracted position. And it is accessible to operations that are plausibly regarded as interpretation at the interface (binding theory, absorption):

- (34)(i) *he thought there were songs about John being played on the radio (he = John)
(ii) they thought there were songs about each other being played on the radio
(iii) who thought there were songs about what being played on the radio

This might mean that the EN output of TH/EX, while immune to Move (either of the whole EN, or of part of it), is accessible to other operations. A simpler alternative is that the EN output of TH/EX is immune to all narrow-syntactic or LF-interface operations, and that the operations that apply (Agree, or those at the LF-interface) are accessing the trace left by TH/EX. It is not easy to distinguish these alternatives on

empirical grounds. I will assume the simpler alternative to be correct. We are then led to the conclusions (35) ((i) = (27)):

- (35) (i) TH/EX is an operation of the phonological component
(ii) Traces are inaccessible to Move, but accessible to some other operations

It is actually worth asking what it means to leave a trace in the phonological component, exactly when categorial information ceases to be available, and whether a trace can be definable other than through categorial information. More on this below.

Still adopting the simplest principle of a single cycle, with phonology and narrow syntax proceeding in parallel, we conclude that TH/EX applies at the level of the verbal phrase, which we may presume to be vP (v a light verb marking unaccusative/passive), a weak phase only; the smallest relevant strong phase is the next higher CP or v*P. TH/EX moves EN rightward or leftward leaving a copy without phonological features, presumably adjunction to vP and substitution in SPEC-v, respectively, if a weak phase has a phonological counterpart to EPP.

As I understand this, then, in one of the two instantiations of TH/EX (the leftward one) you need an EPP analogue, which means this particular process is almost impossible to distinguish from the suggestion I made in Part One about movement to induce concord in related domains. Then there is the rightward displacement, which seems akin to whatever goes on in extraposition and heavy NP-shift. In either instance, you go into TH/EX whenever you have a weak vP phase, so presumably you ‘have time’ until you get to the strong v*P or CP phases to ‘do things’ phonologically.

The next task is to sharpen (35ii): Why is trace inaccessible to Move, and what other operations can access trace? Trace is an empty category EC, lacking phonological features, these having been stripped away at Spell-Out, either in the normal course of derivation, or by the English-specific rule TH/EX prior to the strong phase level.

This is the intuition. TH/EX extracts the phonological material from XP and displaces it around. In that sense TH/EX would seem to relate to ‘deaccentuation’, which also ‘extracts’ the phonological material from an XP which would otherwise not be pronounced in ellipsis contexts, except deaccentuation doesn’t move things around, so far as I know.

Then there is a sentence added in the M-version that confuses me: ‘Trace is thus an empty category EC.’ What does this mean? In a sense, it is trivial; but I presume that the ‘thus’ is meant to mean something. Does this generalization from trace to empty category pertain to the assumption below?

The compound operation Move consists of Agree, Pied-Piping, and Merge. Hence traces must be inaccessible to one of its three components. Other ECs are accessible to Merge, e.g., first Merge of PRO or pro. Therefore trace must be inaccessible to Agree or Pied-Piping.

I’m afraid I don’t see how that follows, unless this is what the comment about EC’s was supposed to introduce. A trace is arguably different from PRO or pro: the latter are configurational formatives, whereas a trace is part of a larger object, an occurrence, however we want to put it. It could be that Merge is incapable to work with occurrences; of course, that’s not true for heads of chains, which might be the reason why we don’t want to say that the problem is with Merge, but my point is simpler: mere phonological similitude between trace and PRO/pro does not logically entail identical behavior.

We have just seen that active trace is accessible to Agree (since EN has to satisfy the Case Filter). Therefore it must be immune to Pied-Piping, preventing Move.

There's a footnote added in the M-version [46]: 'A consequence, Luigi Rizzi points out, is that null Op cannot pied-pipe ("the man [OP I spoke to]' vs. *'the man [[to OP] I spoke]')'. Again, this is only a consequence that can be reached, so far as I can see, if ALL null categories are treated alike, regardless of whether they are of one sort or another. That could be right, but it is an assumption.

The same is true, vacuously, of inactive trace, but this element seems to satisfy an even stronger condition:

In what follows there is an important change in the M-version, which I signal in brackets below:

[if] inactive trace is invisible not only to Agree (like other inactive elements) but even to Match (unlike others): [then] the principle (17), which restricts intervention to the head of a chain [would follow].

I understood things better in the older version. That said: a regular (inactive) trace is like the special traces we're considering in that it also (vacuously) disallows Pied-piping, however, regular traces obey an even stronger condition, namely principle (17) -concerning blocking by the entire chain and not its links. Fine. But now we're adding 'if' and 'then'. What would principle (17) follow from? The unstated 'stronger condition'? I suppose that's what is meant, and that the condition in question is (35ii) appropriately extended.

Principle (35ii), then, seems to have the following cases:

- (36) (a) EC disallows Pied-piping
 (b) inactive trace disallows Match

How is (35i) (which was explicitly about Move) now about EC's? Just because we added 'trace is thus an empty category'? Say I claim that 'Americans are rich' (equivalent to 'trace disallows Pied-piping') and furthermore that 'Americans are humans' (equivalent to 'trace is an empty category'); can I then conclude: 'humans are rich' (equivalent to 'EC disallows Pied-piping')? What am I missing?

Apart from these restrictions, traces should be subject to operations freely; and (36) ought to hold of ECs generally, not just trace, unless trace has to be distinguished from other ECs by virtue of its relational properties, an unwanted complexity.

Alright, so this is now admitted. Whether (36) ought to hold for ECs generally is not something I have a view about; my point is about presentation. It would have been helpful if we started with this last paragraph we've just read, as an assumption. As to whether the relational properties of traces are unwanted complexities, I'm afraid that wanted or not, they're there, the minute we accept chains. But I won't press this point, since I really don't mind the conclusion that EC doesn't involve pied-piping, especially given Rizzi's observation, which I think is very interesting.

Such extension of (36) raises various questions. Lexical ECs undergo Move as well as Merge, unlike trace; but they are XEs, so Pied-Piping does not apply.

Except Rizzi's instance is not trivially dismissable in these terms, as it is not immediately obvious that empty operators should have XE status. If they do, then Rizzi's point is irrelevant.

And (36) holds only if no XP consists solely of several merged lexical ECs.⁵

In what circumstance would that even arise? I thought lexical ECs are ECs *because* they are licensed as pronominal elements. The only other instance of a lexical EC I can think of, which is not a pronominal, is copular *be* in Semitic languages, Russian, Ebonics, etc. That this element should have phonetic content in other languages is what is rare, as it probably is just a Tense holder with no semantics, and as such equivalent to a null pleonastic. What one doesn't really find is ECs which are the equivalent of argument-taking elements, verbs or prepositions. I suppose one could make a case for saying that empty prepositions do exist, as in bare NP adverbs (*he arrived Monday* meaning *on Monday*); and I suppose it's true that even in languages where you have rampant pro-drop, you don't say *Speaking of Monday, he arrived 0 0* meaning 'he arrived on Monday'. But it's also true that even in relevant languages you don't say *SILENCE* meaning 'pro (he) 0 (is) pro (the one)'. Because for some reason -granted, not a trivial one perhaps- sentences must have *some* PF realization, or listeners don't even know we're talking. That might not be what's going on in *P-pro* meaning 'on Monday', but a version of it might. I'm not being facetious in any of this: the conditions under which you're allowed not to lexicalize something are extremely interesting, as ellipsis attests.

Inactive argument EC (PRO, pro) raises problems for (36b) with regard to intervention effects.

I suppose what this means is that inactive PRO, pro do not behave like inactive trace. That is, compare:

- (i) a. **There is likely t to arrive a man**
- b. ***There is likely that it/pro was told a man that...**

We don't want the trace of *there* in (ia) to block the relation between the upper T and *a man*; but we do want *it/pro* (in relevant languages) to block the relation between the upper T and *a man* in (ib), even if *it/pro* is inactive after it checks Case in the intermediate T. So the bottom line: we cannot entirely assimilate all ECs, at least in terms of intervention. This is expected, if pro is a regular, configurational formative, whereas the trace of *there* isn't. But if that's the case, then it is less obvious that (36) ought to hold for ECs generally.

We would also expect that there is a more unified version of (36).

Putting these concerns aside, the properties bearing specifically on TH/EX are (37):

- (37) (i) TH/EX is an operation of the phonological component
- (ii) Pied-Piping requires phonological content

⁵ There are other assumptions, among them, that in structures relevant for pied-piping, roots will have at least some phonological specification in the optimal encoding of LEX; very radical allomorphic variation is unexpected for open classes, as noted earlier.

(37i) is an idiosyncratic rule of English,

That might be, but we've seen awfully similar conditions on extractions from associates in Swedish and Spanish; granted, the generalizations there are not like the one being considered now, but this point holds: something prevents some extractions from associates, not just in English.

(ii) (= (36a)) a principle of UG, if valid. From (i) it follows that the output of TH/EX is immune to all but phonological operations. From (ii) it follows that the trace left by TH/EX is immune to Move but allows Agree and LF-interpretive operations, as required.

I should clarify that I don't have a problem with these conclusions, just have difficulties seeing the way we've reached them. Whether (i) above or something else, I'm prepared to accept narrow stylistic rules that take place in the phonological component, and are immune to all but phonological operations. In turn, it seems to me entirely plausible that, whatever pied-piping turns out to be, it should require phonological content. Pied-piping is a very bizarre process, to begin with. It shouldn't exist, in languages with 'perfect' Agree or any such variant. Pied-piping comes along and basically says: 'although you should lexicalize down here, for some reason you'll magically lexicalize up, where 'you' is a very large chunk of structure'. Doesn't make any sense, especially because languages differ on how large the chunk of structure is that gets to lexicalize up (e.g. nothing in Japanese Wh-movement, words with a Wh-feature in Slavic, phrases with a Wh-feature in Romance or Germanic, entire clauses with a Wh-feature in Basque or Quechua). But in all these instances, it seems as if pied-piping affects phonological structure; you don't need to say any such thing for LF. At LF you can do scope without pied-piping, for instance (if you code quantificational force in the element that enters Agree relations).

The conclusion supports the picture of LEX outlined earlier, and modifies slightly the standard assumption that phonological features do not enter into narrow syntactic derivations. They do not do so individually, but their absence or presence makes a difference if (37ii) is correct.

The rightward variant of TH/EX is like extraposition in that it does not iterate, perhaps a more general property of operations not driven by uninterpretable features, and/or phonological operations.

Why that should be their property (the Right Roof Constraint of Ross) is interesting. Hoffmann 1996 had this idea that stylistic rules involve absence of merge between relevant constituents; if that's the case, iteration would not make sense, since it would be impossible to iterate something that hasn't happened.

We would expect the same to be true of the leftward counterpart. But alongside of (20) and (38i), the latter surfacing as (38ii) after application of TH/EX, we also find (iii), (iv):

- (38)
- (i) there are expected to be caught many fish
 - (ii) there are expected to be many fish caught
 - (iii) there are many fish expected to be caught
 - (iv) many fish are expected to be caught

(38iv) is the unproblematic result of successive-cyclic A-movement with the intermediate stage (39):

(39) are expected [many fish to be caught t]

Actually, whether or not (39) is problematic depends on whether the alternative of inserting *there* was a (more optimal) option:

But (38iii) is unexplained. It does not result from (39) by leftward TH/EX, if iterated TH/EX is barred, as expected for such rules. And both (39) and (38iii) should be barred if Merge preempts Move.

We therefore ask whether there might be a different source for (38iii). The obvious suggestion is a true existential construction "there be NP," where NP includes a reduced relative.

That's certainly a suggestion -I don't know if it's obvious. Another suggestion would be that, although 'rightward' TH/EX behaves as studied thus far, leftward TH/EX is more standard.

The conclusion is supported by the semantics of (38). Examples (i-ii) and (iv) have no existential import, but (iii) does: it states that there are many fish such that they are expected to be caught.⁶ [interesting footnote]

The idea is that whenever you don't have a 'leftward' source, you're dealing with a reduced relative which forces true existential import.

The same conclusions are illustrated in (40):

- (40)(i) there are expected to be found many flaws (--> many flaws found) in the proof
(ii) #there are many flaws expected to be found in the proof
(iii) many flaws are expected to be found in the proof
(iv) there are likely to be baked many cakes (--> many cakes baked) in that oven
(v) #there are many cakes likely to be baked in that oven
(vi) many cakes are likely to be baked in that oven
(vii) there is likely to be taken umbrage (--> umbrage taken) at his remarks
(viii) #there is umbrage likely to be taken at his remarks
(ix) umbrage is likely to be taken at his remarks

In these instances the arrows indicate normal spell-out to the right.

Examples (ii), (v), (viii) have existential import, but not the others; hence the oddity of (ii) and (v), presupposing that the flaws and the cakes independently exist (the cakes have already been baked), and of the idiom chunk of (viii). Syntactic tests yield the same conclusion. Thus constructions of the form (38iii) are islands for extraction, but not the others; (41iii) can be derived from (i), but not (iv) from (ii)⁷:

The idea being that only constructions of the first form involve TH/EX, and thus no possible pied-piping.

⁶ Existential import is not a simple matter. E.g., "there are steps missing in that proof" does not presuppose that the steps exist and can be filled in to complete the proof; and "there is something lacking in his novel" (a spark, vitality, humor, the characters are wooden,...) presupposes nothing about the possibility of overcoming the defect. But in "there are steps expected to be missing in that proof" and "there is something expected to be lacking in his novel," the existential import returns.

⁷ Examples from Lasnik (1999), within a somewhat different framework of assumptions.

- (41)(i)there is likely to be demolished a building (--> a building demolished)
(ii)there is a building likely to be demolished
(iii) how is there likely to be a demolished a building (--> a
building demolished)
(iv)*how is there a building likely to be demolished

Here starts the second big empirical topic of the paper:

Leftward TH/EX is distinct from Object Shift (OS), a phenomenon that is open to many questions with regard to its status and scope. One distinction, already noted, has to do with semantic consequences: the semantic neutrality of TH/EX is one of the reasons to believe that it falls within the phonological component; but as is well known, that is not true of OS, which we therefore expect to fall (at least in part) within narrow syntax, given the simplifications that led to (13). Continuing to assume cyclicity, PIC, and preference of Merge over Move, OS must involve raising to the outer edge of the phase v*P, outside the merged subject in SPEC-v*. Let us examine a few possibilities.

Languages differ with regard to the OS option: e.g., Icelandic allows it freely and MSc partially, while standard English/Romance do not. But if PIC holds, the picture is a little different: every language allows OS, but in languages of the English/Romance types the object must move on beyond the position of OS.

That is, to allow OS. The reason was mentioned in the glosses:

There is no other way to derive such expressions as (42i):

- (42) (i) (guess) what_{OB} [John_{SUBJ} T [_{vP} t_{OB} [_{t_{SUBJ}} read t_{OB}]]]
(ii) John_{SUBJ} T [_{vP} that_{OB} [_{t_{SUBJ}} read t_{ob}]]

Here the direct object DO (what, that) raises by OS, satisfying the Case Filter and erasing the uninterpretable features of v. In (i) the shifted DO moves on to SPEC-CP. In (ii) DO remains below SPEC-TP -- allowed in an OS language, * in English/Romance.

I suppose this means Chomsky will analyze ‘absence of object shift’ as a further step of movement for 0...

The same holds for topicalization and other forms of A'-movement.⁸

Let us put aside the language variation for a moment, and ask how the non-OS languages work, allowing (i) but not (ii) of (42).

The first thought that comes to mind is that the equidistance principle (43) (= (41) of MI) should be reconsidered:

- (43) Terms of the minimal domain of H are equidistant from probe P

The relevant part of (43) has to do with the edge of H.

⁸ Nothing here turns on the question whether OS is substitution in outer SPEC or adjunction; in either case it is to the edge of the construction, invoking PIC.

I don't understand the syntax here. (43) is a proposition; the relevant part of proposition (43) *has to do with the edge of H*? What does that mean? 'Relevant' for what, in what sense? judging from the restatement below, I suppose Chomsky meant that 'the relevant part of the minimal domain of H is really only the edge of H'.

Insofar as this is true, we can restate (43) as (44):

(44) Terms of the edge of HP are equidistant from probe P

In the $\bar{v}P$ of (42), the shifted object (what in (i), that in (ii)) and the in-situ subject John (at t_{SUBJ}) are equidistant from the probe T, which selects the subject as its goal, inducing Agree. But only in (i), with OS moving on to SPEC-C, can Move apply, raising the subject to SPEC-T.

Right, but I don't see how the new definition of equidistance has anything to say about what can move, as under the conditions described so far as I can see both O and S are equidistant from any target.

In both (i) and (ii) of (42), the shifted object in OS position is inactive,⁹ hence not able to agree with T or to move to SPEC-T. But that is unlikely to be the reason for allowing the probe to pass over it to find in-situ John: inactive nominals induce intervention effects.

We saw that already with pleonastics. So again, I'm not sure how the modification of 'equidistance' helps. And the plot thickens:

Another consideration is that shifted object may agree with T and nevertheless allow subject extraction, as in the Icelandic counterpart to (45), with shifted DO in a nontransitive strong verbal phase, DO with quirky Case and V raised to T¹⁰:

The phase is nontransitive in that it doesn't have the usual subject and object (this is an instance of quirky Case). Nonetheless Chomsky takes it to be strong, assuming a v^*P expansion. In these circumstances the object is what agrees with T, thus getting nominative, and yet the indirect object is what moves to subject position:

(45) [many students.DAT]_{SU} [find.PL]_{VB} [the computers.pl.NOM]DO
 [t_{SU} t_{VB} t_{DO} not ugly.PL]

It seems, then, that the distinction between (i) and (ii) of (42) lies in the fact that the wh-phrase moves on, leaving the position "empty" (i.e., void of phonological features).

Okay, so does this mean then that the 'first thought that comes to mind' about equidistance is after all irrelevant? Or is it relevant and we haven't seen yet how? Here, I take it that the idea is this: if the Wh-phrase moves on, leaving the shifted object position without phonological content, then whatever goes on in actual shifting (a single displacement of the object? that with no further

⁹ Irrelevantly, in (i) the wh-phrase is active, having a unvalued uninterpretable feature wh-; but the probe T does not seek that feature.

¹⁰ Examples provided by Anders Holmberg and Thorbjorg Hroarsdottir, who also point out surprising and unexplained complexities about intervention effects in such cases, apparently turning on matching of quirky Case number and raising of subject to TEC position. See also note 54, below.

movement of the verb? something else entirely?) is for some reason not relevant. If so, I still don't see why equidistance plays any role in all of this.

The problem we now face is that the argument seems to require countercyclic operations: Assuming strict cyclicity, the subject John raises to SPEC-T before the operation is authorized by vacating the outer edge of yP, at the subsequent CP phase.

Okay, so are we going to say that movement of the subject 'across the object' is possible only if the object is phonologically 'out of the way'? Say that's true. (I still don't see how equidistance helps here, since I'd say if it holds, then all of this should be irrelevant -but set this aside now). Then is the problem that the subject moving to T has to take place before the object moving to C? I'll assume so.

The problem we face is to show that the countercyclicity is only apparent, by recasting it in a framework that keeps to the strict cycle and allows no backtracking to recover crashed derivations along a different path: specifically, cancelling OS if the position is not later evacuated.

So I think I got the description right. What we'll try next is a definition of cycle that allows a minimal 'look ahead', if you wish, just enough to know that the subject can move in these instances because, at the strong phase, the object will too.

Optimally, the operations Agree/Move, like others, should apply freely.

Actually, I think that's true, but it seems as if in different moments we call optimal this business of applying freely, whereas in other moments we want, say, strictly cyclic application (e.g. of interpretive procedures), also on optimality grounds.

Let us assume that they do, raising the in-situ active subject freely to SPEC-T in the course of the derivation. The probe-goal relation must be evaluated for the Minimal Link Condition MLC at the strong phase level after it is known whether the outer edge of yP has become a trace, losing its phonological features.

This is the minimal 'look ahead' I was talking about. You probe freely, and whether or not that probing violates MLC has to consider what happens at the strong phase, whether there is a full category or a trace 'in between'. Of course, that presupposes two things: (a) that there is an 'in between' to start with, not obvious given the definition of equidistance; and (b) that somehow having a trace eliminates the 'in between-ness' quality; we've seen instances of that already in other contexts, the content of (17) (which restricts intervention to the head of a chain, or the whole chain). Why doesn't Chomsky use simply (17)? I suspect because he takes (17) to follow from (35ii), with its sub-cases in (36) (recall also the extensions discussed above from traces -subject to (17)- to ECs), although I can't say I understand the guiding intuition for that.

Assume so, setting aside for the moment the apparent countercyclicity. That appears to leave all other consequences unchanged, with (44) restricted to the phonological edge of a category -- that is, an edge element with no phonological material c-commanding it within the category¹¹: **[Footnote shows the extent of the technical complication]**

¹¹ Perhaps "c-commanding from the left," depending on the proper treatment of such matters as rightward adjunction and questions raised by Kayne (1994) about linear ordering.

(46) The phonological edge of HP is accessible to probe P

Is this supposed to be a *substitute* for (44)? Also, is (46) a fancy way of saying that only the highest spec (adjunct?) in a given category is accessible to outside probing? I'll keep assuming positive answers...

In the structure (47), XP prevents Match of probe P and SPEC, under MLC, only if XP has phonological content:

(47) [ZP ...P...[HP XP [SPEC [H YP]]]

Okay, so it does seem I read this correctly. So (46) is the new MLC, and the concept of equidistance, so far as I can tell, is gone, perhaps restricted to elements without phonological content.

The background assumption is that the lexicon LEX is only partially distributed, namely, when optimal encoding of information justifies that complication of LEX; see text at note 19. Also presupposed is that Spell-Out is at the next higher strong phase ZP, and that MLC too is evaluated at this stage of the derivation under the general principle (10); a reasonable conclusion, as discussed earlier.¹² [This note, coupled with the conclusions we are now reaching, shows that MLC's role is now drastically reduced] It is only at ZP that the phonological content of XP (hence the phonological-edge status of SPEC) is determined.

This is all fine, the small 'look ahead' I mentioned (principle (10) is the one that guarantees interpretation at the next strong phase). What I have a harder time understanding is the relevance of LEX being partially distributed. I suppose this has to do with why *phonological* structure is relevant to all of this, but I don't really see how.

In these terms we dispense with the notion of equidistance and the principle (44) in which it enters. Operations are strictly cyclic, but the problem of backtracking to recover a failed derivation along a different path remains.

These are the conclusions I reached, but I still don't understand that 'problem of backtracking'; isn't this a very local domain? Is backtracking a problem in such a narrow derivational window?

We can improve the picture further by relating these conclusions to earlier ones about traces, namely (36), repeated as (48):

- (48) (a) EC disallows Pied-piping
(b) inactive trace disallows Match

Right. Recall that (36)/(48) were taken to be sub-cases of (35ii):

(35ii) Traces are inaccessible to Move, but accessible to some other operations.

¹² The effect on MLC is limited under PIC, which bars "deep search" by the probe.

The expressions ‘active’ and ‘inactive trace’ are introduced, entirely obliquely, in the course of the discussion after (35). I think they are used in the general sense that ‘active’ and ‘inactive’ have in the paper, though see below. Is a trace ‘active’ when an unmatched feature of its head is still alive? Heuristically, an ‘active’ trace is left by TH/EX, which the system can access for the purposes of Agree, though not Pied-piping. And traces become ‘inactive’ by the strong phase. What follows is a summary that clarifies the picture slightly:

If XP with phonological content remains at the edge of (47), then it intervenes to prevent Match(P, SPEC), hence Agree or Move of SPEC. If XP moves on to SPEC-Z, its trace is inactive

The M-version corrects: ‘its trace, being inactive with respect to T (see note 51 [here 9])

and is invisible to Match by (48b), so SPEC, now at the phonological edge, is accessible to the probe P.

So if I’m understanding this, first of all traces are not absolutely inactive or active, but rather *relative* to probes; for instance, when XP moves on to SPEC-Z in (47), although (irrelevantly) active with respect to, say, C, the trace left is relevantly inactive with respect to T. And it is when a given trace is inactive with respect to a given probe that it doesn’t enter into a matching (48b), thereby letting other elements proceed with their operations. The point:

That suffices to establish (46), which is therefore not needed as an independent principle.

So the fact that only the phonological edge of HP (in (47)) is accessible to probe P (i.e. (46)) is a consequence of the fact that inactive phrases (with respect to P) disallow match (48b). But I don’t see how this follows without a further assumption; that the ‘outermost spec’ is what matches a higher probe. Suppose you did not assume this. I could still say: ‘alright, fine: an active phrase does not disallow match (the converse of (48b)); but so what? I have two (or more) categories which are capable of undergoing the match, so I choose. We have to prevent that, either because matching is somehow unique (so something disallowing multiple possible matching) or else because only the outer spec (perhaps the closest goal to the probe) counts as matching. Here’s yet another stab at deducing (46):

(48a) suggests another way of thinking about (46): lacking phonological content, the trace of XP is not even a candidate for Move (under (a)), so that SPEC is the closest “competitor” for the operation, under a natural version of MLC.

That seems relevantly similar to the point I raised about ‘closest’ goal, which can be seen as an inadequacy: (46) could be made to follow from either (48a) or (48b), with overlapping assumptions. So something is being missed.

We might raise a more subtle question, which might have some bearing on these issues: is there any difference between Move and Agree with regard to the intervention effect of a trace in the outer edge?¹³

I take it from the rhetoric here that it is somewhat unclear to Chomsky what (46) follows from.

Suppose there is a difference: Move is permitted and Agree alone is barred; the phonological-edge condition of (46) frees Move but not Agree alone. That would raise interesting questions (for example,

¹³ The question is suggested by problems raised by Susi Wurmbrand.

about the locality of the post-raising (SPEC, T) relation as a factor), but it is not easy to find relevant facts. To test this possibility we would want to investigate a language with subject-in-situ constructions (SSC) but no Object Shift of type (42ii), so as to determine whether the pure-Agree analogue of (42i) is permitted with SSC, as in (49):

(49) (guess) what_{ob} [there T [_{vP} t_{ob} [a man read t_{ob}]]]

In Icelandic, with full OS and SSC,¹⁴ the constructions are permitted, as expected. Thus we have the counterpart of (50):

(50) (i) there painted probably the house some students red
(ii) which house painted probably some students red

In English, (49) and (50) are barred, but for independent reasons. We would have to investigate the marginal [**more accurately, barred**] constructions in which English has something like abstract SSC, such as (26):

(51) (i) there entered the room a strange man
(ii) which room did there enter t_{wh} a man

Case (ii) seems considerably worse, which might suggest that only Move is freed from an intervention effect when at the phonological edge. But the data are too marginal for any confidence, and the degradation of (ii), if real, might be attributed to internal-extraction constraints (see note 40 [**here, 3**]). MSc should be a better case, because it lacks the obligatory TH/EX rule of English. Here we find the configuration (52):

(52) (i) *?there painted some students the house red
(ii) *?there painted the house some students red
(iii) *?which house painted there some students red
(iv) which house painted probably some students red

Case (i) illustrates the marginality of the construction, carried over in (iii). Case (ii), with OS as well, is much worse. Case (iv) is fine, which would be unsurprising and irrelevant if "some students" is in SPEC-T and "probably" is in a TP-external position.

The claim that (iv) is fine means that (52) are abstract Mainland Scandinavian instances. Which is consistent with the following:

But Holmberg points out that a definite pronoun cannot be substituted for "some students," suggesting that it may be in its in-situ position in a kind of SSC construction, in which case the (iii)-(iv) contrast seems problematic.

Pending better evidence and understanding, I will put aside the question of a possible Move-Agree distinction under (46).

On the assumption that OS is a free option, what distinguishes [\pm OS] languages, say Icelandic vs. English/Romance? The distinction might lie in intervention effects applying to the output of OS, or in the

¹⁴ See Jonas (1996). Many questions about the data remain obscure, but not in ways relevant here, it appears. Icelandic and MSc data here provided by Anders Holmberg, pc.

option of applying OS in the first place. Let's begin by exploring the first alternative, then turning to the second (concluding finally that they can be unified).

Good, so we have a road map here. I'll label the sections 'Intervention Effects', 'OS application' and 'Unification'. Actually, it turns out that 'Intervention Effects' has two possibilities, also labeled below: 'Deeper Search' and 'DISL'.

DEEPER SEARCH (INTERVENTION EFFECTS, POSSIBILITY ONE)

One possibility is that the shifted object remains in the outer SPEC of VP, and that the [\pm OS] distinction has to do with evaluation of the probe-goal relation: the relation of T and in-situ subject. [+OS] languages allow association of T and in-situ subject (effectively, (43) [**the old equidistance idea**]); [-OS] languages allow such association only under (46), at the phonological edge. The parameter might be related to "richness of T," a richer T allowing a deeper search of the category including the goal.

So I suppose Romance would have a poorer T, unlike say, Latin (assuming Latin had OS). A rich T allows search in terms of the old 'equidistance' (searching in the whole edge plus domain of a category) whereas a poor T is restricted to the edge, and with the phonological transparency that we discussed. On the other hand...

DISL (INTERVENTION EFFECTS, POSSIBILITY TWO)

What seems a more plausible possibility is that there are no differences with regard to intervention effects, and that [+OS] languages have a dislocation rule DISL that raises OS to a higher position, possibly a phonological rule similar to English TH/EX.

So then the last step of OS, the visible one, is of a totally different nature.

Then Icelandic, for example, also excludes OS without further raising of the object, either A'-movement or DISL. Perhaps supporting that conclusion are some properties of MSc, which permits object-fronting for pronouns but only partially for full noun phrases.¹⁵ Holmberg points out that the fronted pronoun is not at the edge of v*P but in a position higher than the auxiliary, indicating that it raised there from the v*P-edge position, perhaps by a phonological operation, or perhaps by a rule of the kind that has been proposed for clitic-movement.

This is an interesting suggestion. It could be that clitic movement, also, is one of those phonological rules, as has been proposed (Anderson, Marantz, etc. and many of their students).

If the former, we have an explanation for the fact that the pronoun does not bind anaphors, as Holmberg observes, citing Holmberg and Platzack (1995).

The idea of course would be that these phonological processes have no LF reflex.

One might then ask whether a similar rule DISL raises the full Icelandic shifted object (though to a different higher position). Constructions of the kind illustrated by (45), repeated as (53), provide additional support for this conclusion:

¹⁵ See Holmberg (1999), on which I am relying for data here and below, along with personal communication.

(53) [many students.DAT]_{SU} find.PL_{VB} [the computers.pl.NOM]_{DO} [_{t_{SU} t_{VB} t_{DO}} not ugly.PL]

For some reason, in the M-version the traces in the lower constituents have disappeared; the same is true of (45) above. I don't know whether this is significant.

Here we have Agree(T, DO) but it cannot induce Move, raising DO rather than SU to SPEC-T. If DISL applies (as a phonological rule), then DO cannot raise to SPEC-T for the reasons discussed in connection with TH/EX, even when it agrees with matrix T.¹⁶ One can think of possible answers related to the default vs. T_{COMP} options of quirky Case in SPEC-T and other options available to quirky Case; or it might be necessary to weaken (3i), which requires that goal as well as probe be active for Agree to hold. The cases reviewed so far (see notes 30, 40) are consistent with the assumption that (3i) holds only for (P, G) pairs in different phases, with G in the domain (not the edge) of the lower phase. But unresolved factual questions make suggestions premature (see note 52 [here 10]).

In other words, because of the Pied-piping limitation (37ii).

Consider now the backtracking problem. Under the "richness of T/deeper search" alternative, the problem remains: if OS has produced $\bar{v}P$ with multiple-SPEC, as in (42), then search will fail in a non-OS language if the raised object has not moved on, and the derivation must return to the pre-OS stage and proceed without application of OS.

Right, but so what? This is so local that it seems hardly a problem.

Under the DISL alternative, that may not be necessary. If DISL applies, it can be followed unproblematically by raising of subject to SPEC-T, at the phonological edge. If DISL does not apply, subject can still raise to SPEC-T, but the derivation will crash by evaluation of MLC at the CP phase unless the outer SPEC has been evacuated by raising to SPEC-C (see note 6 [9 in M-version]). That raises no new problems if the object is a wh-phrase, as in (42i); then it can move to SPEC-C. If the object is not a wh-phrase, as in (42ii), it can be raised to SPEC-C by Topicalization. If that is a free option, then backtracking will never be necessary in this category of constructions.¹⁷

We return directly to the second alternative: that the [\pm OS] distinction lies in the option of applying OS in the first place.

Still another question has to do with Holmberg's generalization HG: to first approximation, OS is permitted only if V has raised to T. The principle is countercyclic, hence problematic.

I suppose that 'countercyclic' is meant seriously here. That is, V to T has to proceed across v^*P (in relevant instances) whereas OS is, pretheoretically at least, something happening within v^*P . In

¹⁶ This note is changed in the M-version, where it starts: 'Unexplained is why shifted quirky DO can remain active, unlike shifted structural Accusative DO. Compare:

OS for quirky Case NP falls into place if the phenomenon is understood as in note 15 [I suppose this would have been note 9], though problems remain. One has to do with optionality of DISL. Another is why shifted quirky DO can remain active, unlike shifted structural Accusative DO.

¹⁷ Free Topicalization may produce expressions with deviant interpretations, but that is not a problem. The wh-phrase case depends on how the operation is to be understood: Is there an uninterpretable wh-feature? Is there matching with an appropriate C = Q? What is the status of wh-in situ in typologically different languages? And other questions, including some mentioned earlier.

that sense we'd have something countercyclic. There is a more trivial sense of countercyclic here: you only know that you could move the object after you moved the verb to a position that is higher than that of the object. But whether or not that's going to be problematic, per se, depends on what other assumptions one makes about phases.

One might seek to reformulate it in terms of phase-level evaluation of optional OS, along lines suggested above for trace at outer edge. A number of problems then arise, among them, the fact that A'-movement to the outer edge of v*P does not observe HG: wh-movement of (or from within) the direct object, for example, does not require V-raising in OS or non-OS languages.

Of course, factually (and assuming that those instances of Wh-movement involves a prior step of OS).

Also pertinent is the fact that some form of phonological adjacency seems to be involved in OS¹⁸: "verb-topicalization" (raising of V to SPEC-C, Holmberg argues) frees OS even when an auxiliary verb blocks V-raising to T, making the countercyclic property even worse. And furthermore not just in-situ verb, but any element (say, a preposition) bars MSc shift of object pronoun, which, Holmberg argues, should be assimilated to Icelandic OS, despite some differences, primarily because both fall under HG. Particularly striking is the comparative Scandinavian evidence that Holmberg reviews concerning extraction from verb-particle constructions: just in the cases where the pronominal object precedes the verb particle (perhaps by raising) can OS apply.

All of that is hard to follow without concrete examples, although we could perhaps reconstruct on the basis of what follows.

Holmberg suggests that HG be reformulated along the following lines:

- (54)(i) OS is a phonological operation that satisfies condition (ii) and is driven by the semantic interpretation of the shifted object (new information, specificity/definiteness, focus, etc.; call the interpretive complex INT)¹⁹
- (ii) OS cannot apply across a phonologically visible category asymmetrically c-commanding the object position except adjuncts

The basic idea is persuasive, but the implementation raises a number of questions. It requires countercyclic operations, and unlike TH/EX, it violates the semantic expectations for phonological rules (see (13)). It is also necessary to find some different way to handle "invisible" OS in A'-movement in [\pm OS] languages, and visible OS in Icelandic-type languages (both violating (54ii)). Also problematic is the way the operation is driven by informational properties of the XP that is raised, interweaving with phonological properties of the construction. Note that the proposed rule does not fall together with other cases plausibly accounted for in terms of phonological adjacency, e.g., T-V assimilation.²⁰

The countercyclicality of the proposed operation, and the introduction in (54ii) of a property similar to "phonological edge," suggest that the phenomena may fall under principles of phase-based derivation.

¹⁸ See Holmberg (1986), Bobaljik (1995), observations extended in Holmberg (1999).

¹⁹ See Diesing (1992) and other work. Condition (ii) is (38) of Holmberg (1999).

²⁰ Raising or lowering. See Lasnik (1981, 1999), Bobaljik (1994).

This is reasonable, and also gives us a couple of intuitions concerning what Chomsky is after: phonological edges (whatever that is, but keep in mind that the notion exists in morphology, e.g. in Anderson's approach to V2) and apparent counter-cyclicity (possible only within the narrow confines of the active part of a strong phase). Underlying all this is also the idea that 'information driven' transformations should not be motivated by such notions as specificity features and the like; rather, the operations should arise for some purely mechanical reason (e.g. getting rid of intervening material), and have as a consequence the semantic effect.

Let us ask, then, whether the basic content of Holmberg's recent revision of HG can be derived in these terms, appealing as much as possible only to plausible general principles, along with a simple parameter that keeps intact the leading intuition: that both phonological and semantic (informational) properties are involved in HG.

This seems to me very reasonable:

Consider first the semantic properties INT of the object OB that undergoes OS (see (54i)). Sometimes the operation is described as driven by these properties of OB, perhaps by features of OB that bear the interpretation INT. That is a questionable formulation, however. A "dumb" computational system shouldn't have access to considerations of that kind, typically involving discourse situations and the like. These are best understood as properties of the resulting configuration, as in the case of semantic properties associated with raising of subject to SPEC-T, which may well be related to those of OS constructions. One might also say informally that in (55) the phrase "the men" is raised in order to bind the anaphor:

(55) the men seem to each other to be intelligent

But the mechanisms are blind to those consequences, and it would make no sense to assign the feature "binder" to "the men" with principles requiring that it raise to be able to accommodate this feature. We may also say informally that he's running to the left to catch the ball, but such functional/teleological accounts, while perhaps useful for motivation and formulation of problems, are not to be confused with accounts of the mechanisms of guiding and organizing motion. The same approach seems sensible in the case of OS. The computational system presumably treats it as an option -- if the MI approach is on the right track, feature-driven by properties of v^* , with the option expressed as optional choice of an EPP-feature.

Then again, that might not be the right formulation; it doesn't matter for the present reasoning. Whatever it is, it has to be 'dumb' (in the technical sense of 'dumb computation')

The resulting configuration has particular properties, as in the case of raising to subject (or catching a ball). These properties may have internal inconsistencies, posing interpretive problems -- e.g., a definite pronoun left in a position that assigns nonspecific interpretation. If so, the expression is deviant.

The general configuration we are considering is (56):

(56) [$\$$ C [SPEC T ... [\cdot XP [SUBJ v^* [$_{VP}$ V...OB]]]]]

The position SPEC-T is created by merging the surface subject (by pure Merge or by Move); the XP position is created by OS (perhaps later vacated by A'-movement or DISL). We are concerned with two

properties of (56): the interpretations assigned to the configuration, and the phonological adjacency properties that enter into the formation of the (XP, OB) chain.

Let us adopt the general background assumption that at the interface, arguments are A-chains (with one or more members).²¹ The theta role of the argument is determined by the position of first Merge -- the configuration in which it takes place, within the Hale-Keyser (1993) framework for theta theory. "Surface interpretation" is determined by the position of the head of a (multi-membered) chain; see (13).
[that just says that surface semantic effects are restricted to narrow syntax]

I may be wrong, but I see a D-structure component here and an S-structure component. Mind you: I have no problems whatsoever with either, in fact I think you need them. They are not levels of representation (we have serious arguments against those, discussed elsewhere) but they are nonetheless components of the system. After all, Hale and Keyser's system is about something. And these 'surface interpretation' that we're now discussing (specificity and the like) is presumably also about something. And that 'something' is not obviously whatever LF is about, which is just scope. One could try to reduce thematic and surface structure to scope, but short of that (a program, and an unfulfilled one so far as I know) you need those components in the system, obeying whatever conditions they obey. For example, here's something substantive: first merge is what gives you theta-theoretical properties (presumably for configurational reasons) while the head of the chain obeys surface conditions. That doesn't follow from necessity, so far as I can tell, and inasmuch as it is a part of the system, it is information that you want some relevant component to register.

In (56), the theta role is determined by the configuration of OB. If OS does not apply and OB remains in situ in a trivial A-chain, then the same configuration is freely interpreted at LF, taking account of inherent properties of the lexical items and the theta role: in particular, it may have the "surface" interpretation INT or its complement INT'.

I'm confused here. Do we mean 'complement' in a set-theoretic sense? That is, INT is a set of interpreting characteristics and INT' is the complement of that set? Say INT refers to 'specificity' and thus INT' to 'non-specificity'. Does this mean that when you interpret the chain in situ (as there hasn't been any movement) then you can go with a typical 'surface' interpretation (that is INT, here specificity) or with a typical 'deep' interpretation (that is INT', here non-specificity)? If that's the idea, it is interesting, and I think according to fact.

If OS does apply, forming the two-membered chain <XP, OB>, the surface semantic role of the chain is determined by the peripheral EPP position occupied by XP, the raised object. We assume that INT is assigned to the peripheral configuration universally, adopting (57), probably a subcase of a more general principle governing peripheral non-theta (EPP) positions including SPEC-T -- a traditional idea, still somewhat obscure:

(57) The EPP position of v*P is assigned INT

I can only think of one complication. I thought agree produced chains, as much as move does. If so, there is a chain here regardless of whether OS has taken place. But we don't want to say that INT happens unless that chain actually lexicalizes up. So it can't be that INT goes with the head of the chain (as suggested in the text) whereas INT' goes with the foot. It has to be something more precise, making reference to EPP (or Pied-piping or at any rate overt stuff). That's the essence of

²¹ We take a chain to be a set of occurrences of the first-merged XP, as in MI and Chomsky (1995), returning to the matter below. The discussion can easily be revised for other formulations of LF-interpretive operations.

'surface' effects, that they are overt. No matter how we end up solving all of this, that's probably an intuition that we should not give up.

OS APPLICATION

Let us now return to the parameter that distinguishes [\pm OS] languages, adopting the second of the two perspectives mentioned earlier: that the distinction lies in the option of applying OS. Two factors enter into OS: assignment of INT-INT', and phonological adjacency. Suppose the parameter is (58):

(58) At the phonological border of v*P, XP is assigned INT'

By the "phonological border" of HP, we mean a position not c-commanded by phonological material within HP (see note 48 [here, 11]). The concept is broader than "phonological edge," which holds only of edge elements (and is now only an expository device, with the reduction to (48)). For example, in (59), if H and the edge (SPEC) of HP are vacated by raising, then COMP is at the border but not the edge:

(59) HP = [SPEC [H COMP]]

New mismatch between constituent structure and phonological structure. It's as if, in relevant languages ((58) is going to be a parameter), 'surface syntax' cannot help but see what's the phonological border of v*P and assign it 'non-surface' interpretation. That is a Diesing-style approach, but in more mechanical terms. Parametrically, stuff within v*P is seen as 'non-surface' (INT'); whereas, universally, stuff at the edge of v*P is seen as 'surface' (INT). The procedure is dull, mechanical, and appropriately surfacy in that the notion 'border' is a mixture of syntactic and phonological information, unlike the notion edge, which is syntactic. A more 'elevated' semantics would look at edges, not at borders.

As throughout, we assume that (58) is evaluated at the next higher strong phase, in accord with (10): at the stage \$ in (56).

The basic idea:

OS languages observe (58), non-OS languages do not. In the construction (56) in an OS language, if OB is at the phonological border of v*P and resists interpretation INT' (say, a definite pronoun), it must undergo OS to avoid a deviant outcome, raising to the EPP position so that the chain will be assigned INT. It need not undergo OS in a non-OS language, or in an OS language if it is v*P-internal -- by which we mean not at the phonological border.

In other words, even in non-OS languages relevant objects are allowed to OS, and even in OS languages relevant objects are allowed not to OS, given the appropriate conditions. In the latter instance, for example, when there are a bunch of elements in the way, so that the object that would be inappropriate as INT' (a definite) is nonetheless fine, since the procedure in (58) doesn't kick in, because the object in question is not at the phonological border.

UNIFICATION

I'm not entirely sure, but I suspect that the attempt to unify the OS Application and the Intervention Effects approaches starts here.

That is a start, but more is needed. We have to guarantee that OS applies just in the right places. In the worst case, these effects are specified by particular conditions; a much better result would be that they follow from a general principle applying to optional rules, which captures the functional/teleological intuition about rule application. The natural suggestion, based on ideas of Reinhart (1993, 1997) and Fox (1995, 1999), is a general economy principle: an optional rule can apply only when necessary to yield a new outcome.

The tricky issue is what one means by ‘new outcome’. The formulations I’m familiar with, from Fox, rely on truth-theoretic notions that are not without their complications, and that, in any instance, are not obviously relevant for INT. But let’s set this aside now.

Within the current framework, the optional rule in these constructions assigns an EPP-feature to v^* , thus allowing (and requiring) OS. The guiding intuition is formulated in MI as (60)²²:

(60) Optional operations can apply only if they have an effect on outcome: in the present case, v^* may be assigned an EPP-feature to permit successive-cyclic A'-movement or INT (under OS)

And what follows summarizes:

The proposal we are considering, then, is that OS reduces to (61), where (A) and (B) are invariant principles -- special cases of more general principles governing optionality and interpretation of peripheral positions, respectively -- and (C) is the parameter that distinguishes [\pm OS]-languages:

- (61)(A) v^* is assigned an EPP-feature only if that has an effect on outcome
- (B) The EPP position of v^* is assigned INT
- (C) At the phonological border of v^*P , XP is assigned INT'

Under (10), principle (A) is evaluated at the next strong phase, where the interpretive operation (C) also applies: CP containing v^*P , in the cases under consideration. At that point it is known whether v^*-V has raised (to T or to C/SPEC-C).

Suppose L is a non-OS language, not observing (C) (e.g., Romance). Then interpretations can be assigned freely in the position of first Merge, including INT or INT'. Under the economy principle (A), v^* may have an EPP-feature, inducing OS, only if that is required in order to yield some outcome other than INT-assignment (which is already free without the EPP-feature and resulting OS): for example successive-cyclic A'-movement. Otherwise OB remains in situ, whether v^*P -internally or at the phonological border.

The richer instance is this:

Suppose that L is an OS language observing (C) (Icelandic, and following Holmberg, MSc as well, with somewhat different conditions). Suppose OB is v^*P -internal, c-commanded by unraised v^*-V , preposition, particle, etc. Then the situation is exactly as in non-OS languages: interpretation can be assigned freely, and v^* may have an EPP-feature, forcing OS, only to yield some new outcome other than INT-assignment (already available in situ); A'-movement for example. Suppose OB is at the phonological border of v^*P . If it remains in that position, the (one-membered) chain will be assigned

²² Discussion of (24) and notes 51, 53 of MI, paraphrased here.

INT' under (C); if it raises by OS, the (two-membered) chain will be assigned INT. Under principle (A), v* may have an EPP-feature, forcing OS and the new interpretation INT. The choice is optional. If OB resists INT', say a definite pronoun, failure to exercise the option yields extreme deviance; if OB resists INT, exercising the option has the same effect.

This is the guiding idea:

But the internal semantic properties of OB are not part of the mechanism of the rule, just as the intention of binding an anaphor is not part of the mechanism of raising.

Strictly, this doesn't violate last resort -it's more complex than that. True, the reasons for the movements (when they take place) have nothing to do with semantic interpretation, but that doesn't mean that last resort is violated: the EPP feature (admittedly, a trick to guarantee either successive cyclic movement or INT) is responsible for triggering the transformation. It is not clean: EPP has a disjunctive effect. Furthermore, INT does not arise as a result of a general rule; rather, whatever carries given elements to phonological edges will do. But in any case, this is all rather intentional, both to accommodate the complex phenomenology of European languages and to avoid 'smart' derivations that move in order to obtain a given pragmatic array.

To illustrate, in Icelandic or MSc, the counterpart of (62i) is not well-formed but (ii) is:

- (62) (i) *I have her_{OB} not [seen t_{OB}]
(ii) I saw_v her_{OB} not [t_v t_{OB}]

In (i) the trace t_{OB} of her is in a v*P-internal position: the v*-V complex has not raised. The parameter (61C) is inapplicable, and therefore v* cannot have an EPP-feature (as in English/Romance). Accordingly, (i) cannot be generated: the pronoun remains in situ, free to receive INT (or, deviantly, INT'). In (ii), the trace t_{OB} is at the phonological border of v*P after verb raising. Accordingly parameter (C) permits the option of an EPP-feature for v* under principle (A), allowing assignment of INT to the pronoun-chain.²³

The presentation here is very confusing. Consider (62) prior to movement of the pronouns:

- (62') (i) *I have not [seen her_{OB}]
(ii) I saw_v not [t_v her_{OB}]

In (62'i) *seen* is at the phonological border of v*P, whereas in (62'ii) it is *her* that is. Then parameter (C) is relevant only for (62'ii), not for (62'i); in other words, in (62'i) *her* is not assigned INT' by rule. But then we couldn't add the EPP feature to v* (to move *her*) because we can only do that, given economy principle (A), if this will result in an effect on outcome. By assumption this is not the case. In all fairness, I must say I have a slight difficulty with this: obviously, assigning the EPP feature would have an effect *on PF*, though not on anything else if INT' is not assigned to something which is not at the border. Why isn't PF effect enough to license the unwanted EPP feature? Perhaps 'effect on output' should be more constrained, to effect on interpretation (meaning the QR instances a la Reinhart/Fox and these INT/INT' instances); of course, that would eliminate 'mere' EPP for successive cyclicity from the picture. But I'll put all of that aside.

²³ The tacit assumption is that the object pronoun first moves to the outer edge of v*P (as required by (13)), then on to its surface position, whatever the latter operation may be.

For an English-type language L lacking V-raising, the parameter is inapplicable: the condition in (C) never holds, so L is necessarily non-OS. Suppose L is an OV language, the order base-generated or derived by object raising. If L is [+OS] then shift is always possible, whether or not V raises. Parameter (C) distinguishes Romance from Germanic (standard English aside), while the OV/VO parameter distinguishes Dutch-German from Scandinavian.

Keep in mind that OS is taken to be different from the OV order.

Ignored so far is the case of Icelandic OS crossing the subject, e.g., constructions of the form (50i) and (63), where σ is the original v^*P , t_{VB} the trace of the raised v^*V complex, and t_{OB} the trace of the shifted pronominal object²⁴:

(63) there read it (never) [σ any students t_{VB} t_{OB}]

Though the verb has raised, its object remains in v^*P -internal position because the subject has not raised, so that OS should not be permitted under (61). The problem could be overcome by restricting the notion v^*P -internal to the domain of v^* , but that might not be necessary. Consider again the principle (25): something must be raised from transitive v^*P . Hence if the subject remains in situ, the object must escape v^*P . Therefore v^* is permitted to have an optional EPP-feature under the economy principle (A), allowing OS in this case.

Two things. First, up to now (25) was a speculative condition; now it would be necessary (and needless to say we want to understand what its role is or what it follows from if it's not a principle). Second, if I understand the reasoning, it is (25) that makes the EPP feature legitimate. So we're now putting a lot of weight on this, and the question is when are optional rules possible. Up to now we had Reinhart/Fox style QR and possibly EPP. Now we've extended our notions to INT in relevant languages, but we cannot just say that EPP is possible if it will result in a different PF, period. Furthermore there is the tricky issue of successive cyclicity, and now also (25), whatever that is. We clearly need a theory of all of this, if it is unified under the rubric 'when optional rules are possible'.

Returning to the basic configuration (56), repeated as (64), we have the two strong phases σ and $\$$, v^*P and CP respectively:

(64) [$\$$ C [SPEC T ... [σ XP [SUBJ v^* [v_p V...OB]]]]]

OS takes place within the v^*P phase σ , but the conditions (61) that apply to it are evaluated only at the next higher CP phase $\$$, when V-raising may have taken place: to T, or to C/SPEC-C under V-topicalization. This is in accord with the principle (10) that all evaluation/interpretation is at the next higher strong phase, including Spell-Out.

For some reason the following comment didn't make it to the M-version:

Furthermore, Spell-Out must take place after interpretation/evaluation by (61) at $\$$, so that trace is distinguished from non-trace in terms of intrinsic (non-relational) properties.

Does this mean that Spell-out takes place *before* interpretation at $\$$?

²⁴ See note 51. [9 here]

If tenable, these proposals yield the essential conclusions of HG on the basis of general principles and a simple parameter, capturing Holmberg's intuition that the rule of OS involves a kind of phonological adjacency but is motivated by informational [**in the M-version, 'semantic'**] requirements.²⁵ There is no recourse to countercyclic operations violating the extension condition.

So the Extension Condition lives... This is somewhat surprising, given the 'Tucking in's' in MI. In effect, we have several things ensuring the cycle. The Extension Condition, in a radical way for the upward boundary of the phrase-marker. The PI condition for a kind of downward boundary, beyond which the system doesn't see any further operations. The idea of interpretation/evaluation at the strong phase in addition to both of these, as the derivation unfolds. And finally the phase-like access to the numeration. Much room for improvement and unification...

Nothing need be said about violations of phonological adjacency: specifically, Icelandic OS and the first stage of A'-movement.²⁶ The function of the rule is expressed without teleological devices or special features on OB to drive the derivation, questionable in any event and also invoking Greed with its attendant computational complexity. We can keep to the interpretive principle (13) [**surface meaning restricted to narrow syntax**] for "surface interpretation," and the simplifications of general architecture that underlie it. The only features involved are the familiar ones: EPP, and Case-agreement features, functioning in accord with independently-motivated principles. Icelandic and MSc OS are partially unified, with differences remaining to be explained; for both, OS satisfies the revised version of HG.

The account so far leaves open the possibility that V-raising is comparable to TH/EX and DISL: not part of the narrow-syntactic computation but rather an operation of the phonological component. Either way, the phonological content of base V is determined prior to Spell-Out at the phase level, so that the effects of the revised HG are determined as well.

I think this cryptic remark makes reference to the idea that a verb that hasn't moved (base V) has its phonological contents in place prior to the time when these phonological contents may affect the computation of the procedures that enter into the revised Holmberg Generalization. It would be interesting to see what happens to the generalization in instances of verbal ellipsis, whether the ellided verb acts or does not act as a phonological boundary, if this can be determined at all.

There are some reasons to suspect that a substantial core of head-raising processes, excluding incorporation in the sense of Baker (1988), may fall within the phonological component. One reason is the expectation of (near-) uniformity of LF-interface representations, a particularly compelling instance of the methodological principle (1), as in the case of TH/EX (see (27) and comment). The interpretive burden is reduced if, say, verbs are interpreted the same way whether they remain in situ or raise to T or C, the distinctions that have received much attention since Pollock (1989). As expected under (1), verbs are not interpreted differently in English vs. Romance, or MSc vs. Icelandic, or embedded vs. root structures.

²⁵ Consider the issue of intervention effects discussed earlier, with the "richness of T" and DISL alternatives. Neither alternative introduces further parametric variation. For non-OS languages, an extra EPP-feature is permitted for v* under (61(A)) only with wh-movement and Topicalization. The "richness of T" alternative (invoking equidistance, otherwise perhaps unnecessary) applies only in OS-languages. Under the more natural DISL alternative (without equidistance), the phonological rule DISL is a universal option, vacuous for non-OS languages.

²⁶ In the latter case, interpretation depends on how wh-constructions and the like are construed. Holmberg observes that it-type expletives may undergo OS; whether this obviates INT depends on how we understand the expletive-associate relation in such cases.

I made this point explicitly in my LI article on clitics. However, since then I have discovered the following contrasts:

- (i) a. **La cria esta cansada / esta la cria cansada**
the girl is-SL tired is the girl tired
- b. **La cria es preciosa / *es la cria preciosa**
the girl is beautiful is the girl beautiful
- c. **El general mataba esclavos cuando lo nombraron dictador**
the general killed slaves when him declared.they dictator
- d. **Mataba el general esclavos cuando llego el enemigo**
killed the general slaves when arrived the enemy

In Romance, verb movement determines stage-level vs. individual-level interpretation. Thus observe that the SL verb *esta* in (ia) can be in situ or pre-subject; however, the IL verb *es* in (ib) cannot be pre-subject. Similarly, in (ic) and (id) we obtain a categorial intepretation vs. a thetic interpretation apparently just in terms of the position of the verb. Significantly, the thetic/categoric distinction is information-theoretic, and not theta-theoretic. So it is entirely possible that this is a “surface semantic” effect. It must be kept in mind, however, that this verbal displacement may be an instance of ‘verbal topicalization’, an idea explicitly defended in Raposo and Uriagereka (1995).

More generally, semantic effects of head-raising in the core inflectional system are slight or nonexistent, as contrasted with XP-movement, with effects that are substantial and systematic. That would follow insofar as head-raising is not part of narrow syntax.

A second reason [for assuming head movement is at PF] has to do with what raises. Using the term “strength” for expository purposes, suppose that T has a strong V-feature and a strong NOMINAL-feature ([person], we have assumed; D or N in categorial systems). It has always been taken for granted that the strong V-feature is satisfied by V-raising to T (French vs. English), not VP-raising to SPEC-T; and the strong NOMINAL-feature by raising of the nominal to SPEC-T (EPP), not raising of its head to T. But the theoretical apparatus provides no obvious basis for this choice. The same is true of raising to C and D. In standard cases,²⁷ T adjoins to C, and an XP (say, a wh-phrase) raises to SPEC-C, instead of the wh-head adjoining to C while TP raises to SPEC-C. And N raises to D, not NP to SPEC-D.

Well, I wonder how we’re going to get the fact that, say, Basque is D last, or C last. Granted, these are across-the-board properties of the language, nothing related to the Wh-system, for instance. But the bottom line is that in a language like this you have NP in a position that looks like the spec D (if you want to keep Kayne’s LCA) and a TP that appears to have moved to the spec C.

²⁷ The qualification is intended to leave open other possibilities: e.g., TP raising to SPEC-C in accord with Kayne's theory of linearity (Kayne 1994), head-raising vs. XP-raising as a possible distinction between wh-in situ and overt-raising (see MI and sources cited, particularly Watanabe 1992 and Hagstrom 1998). The N --> D rule developed in Longobardi (1994) and subsequent work has crucial semantic consequences, but it seems that these might be reformulated in terms of the properties of D that do or do not induce overt raising.

These conclusions too follow naturally if overt V-to-T raising, T-to-C raising, and N-to-D raising are phonological properties, conditioned by the phonetically affixal character of the inflectional categories (see note 58 [now 16]).

That would be compatible with TP to C or NP to D movement in Basque.

Considerations of LF uniformity might lead us to suspect that an LF-interpretive process brings together D-N and C-T-V (see note 6) to form word-like LF "supercategories" in all languages, not only those where such processes are visible.

If I understand this correctly, two things are being said. First, 'affixal properties' work by processes that are not part of the standard syntactic computation. That has been said independently, for instance by Lasnik, but also in the morphology project of Marantz (e.g. in Bobaljik's thesis). But Chomsky wants to extend it also to what one might think of as 'LF morphology' (to use Lasnik's phrase). That 'bringing together' D and N or C, T and V to form 'supercategories' is nothing short of LF morphology, so far as I can see.

The following paragraph is out of the main text in the M-version:

Such phenomena as V-second might also be more natural within the phonological component: there was a good syntactic argument for them in X-bar theories with stipulation of a single SPEC,²⁸ but in a bare phrase structure theory without that condition the argument disappears, and there is no natural syntactic notion "second."

Another consideration has to do with the nature of the head-raising rule, which differs from core rules of the narrow syntax in several respects. It is an adjunction rule; it is countercyclic in ways that are not overcome along the lines discussed earlier; the raised head does not c-command its trace; it observes somewhat different locality conditions.²⁹ The head-movement constraint HMC has been assimilated to general locality conditions in various ways, but special features remain, it seems.

The M-version rearranges this a bit, adding: 'identification of head-trace chains raises difficulties similar to those of feature movement, since there is no reasonable notion of occurrence.' I'll clarify this bit about occurrences immediately below, when we talk about chains.

All of this is unproblematic if overt adjunction is a phonological process reflecting affixal properties.

The M-version adds a paragraph: 'Note further that if excorporation is excluded, head movement is not successive-cyclic, like rules TH/EX and DISL discussed earlier, which are plausibly assigned to the phonological component. It could be, in fact, that iterability is a general property of operations of narrow syntax, but these alone. I'm not familiar with the use of the term 'iterability' in this respect, but I think it is meant to distinguish rules that, basically, obey the Extension conditions from rules that enter broader cyclic considerations.

²⁸ It is sometimes supposed that multiple-SPEC is a stipulation, but that is to mistake history for logic.

²⁹ Commonly head-movement is held to observe c-command, but with a stipulated disjunctive definition for this case, which does not fall under the "free" relation of c-command derived from Merge (see MI).

In other words, to get command to work for head movement, you need to complicate the definition in unnatural ways.

The same conclusion is suggested in some recent work on aphasia by Grodzinsky and Finkel (1998), extending earlier work of Grodzinsky's. They argue that a range of symptoms can be explained in terms of inability to identify XP-chains, but note that the results do not carry over to XE chains; the result is expected if head-raising is a phonological process, creating no chains.

The following paragraph is before the previous in the M-version.

Boeckx and Stjepanovic (1999), extending and modifying work of Howard Lasnik, argue that some problems of pseudogapping can be accounted for by the same assumption [**that head movement is a phonological process**], also citing other recent work that finds differences between head- and XP-movement.

A few final comments of a more general nature.

In considering these issues we want to be careful to distinguish terminological artifacts from what may be substantive matters. The question commonly arises with regard to the status of entities introduced in linguistic description and theoretical exposition, raising issues often regarded as controversial. Let us turn to a few of these.

Consider the notion of chain. In MI and earlier work, a chain is understood to be a sequence of identical elements -- more precisely, a set of occurrences, where we may take an occurrence of K to be its sister (not an entirely innocuous proposal, as noted in MI). Thus in (65i) we have the chain (ii) consisting of two occurrences of K = John, K₁ being the syntactic object corresponding to "T-be killed John" and K₂ corresponding to "kill":

- (65) (i) John was killed
(ii) {K₁, K₂}

Several aspects of this description have been regarded as problematic under minimalist assumptions, among them the fact that an occurrence of K is an X' category, neither XE nor X^{max}, hence arguably invisible; and the very notion of a chain as a set of occurrences formed by multiple merger, not a "syntactic object" susceptible to computational operations as the concept is recursively defined.³⁰

It is not clear, however, that the issues are real. Take the notion of occurrence as X' sister. The conceptual and empirical arguments for X'-invisibility are slight. The conceptual argument relies on the assumption that X' is not interpreted at LF, which is questionable and in fact rejected in standard approaches. The empirical argument is that it allows incorporation of (much of) Kayne's Linear Correspondence Axiom (Kayne 1994) within an impoverished (bare) phrase structure system.³¹ But that result, if desired, could just as well be achieved by defining "asymmetric c-command" to exclude (X', YP) (a stipulation, but not more so than X'-invisibility).

True, although the MSO system deduces the invisibility of X' in less stipulative ways, I believe.

³⁰ See Epstein and Seely (1999).

³¹ Other apparent distinctions between X' and XP dissolve -- largely or completely -- with more systematic reinterpretation of relations and operations in terms of labels rather than phrasal categories, as in MI and here.

Furthermore, even if X' were to be shown to be an inappropriate choice for "occurrence," nothing much seems to follow. Recall that Merge yields two "free" relations: Sister and Immediately Contain. Instead of defining "occurrence" in terms of the first, we can define it in terms of the second, taking an occurrence of K to be not its sister but its "mother," the category containing it, a terminological device that also has the advantage of rendering the notion occurrence-of asymmetric. If we adopt it, we replace the chain (ii) in (65) by CH = {K₃, K₄}, where K₃ is the syntactic object corresponding to the TP (65i) and K₄ to the VP "kill John," CH a proper chain because there is a single XP (here John) of which both K₃ and K₄ are occurrences.³² chains are properly distinguished on independent grounds, and the arguments for invisibility are by now so weak that there is no compelling reason to pursue the matter. [interesting footnote]

As it turns out, K₃, corresponding to TP, is quite literally the probe that starts the relation to start with. If you had defined things in terms of 'sisterhood', it is not clear what the sister of *John* is in the probe position for an Agreement chain (supposing these exist), since *John* hasn't moved at all (its sister is V, period). Of course, it is not clear what the *mother* of the element is, either, in the unmoved position. But if we define the upper context as the probe, then it turns out that it makes no difference whether it is a sister or a mother: it is a T projection. This in turn suggests that we should define the 'lower' occurrence not in terms of the mother or the sister, but in terms of the goal (so the chain is the probe/goal relation). If this is the case, the relevant chain above would be {TP, John}. Note that this would have consequences for successive cyclic movement:

[John ... [(John) ... [... (John)]]]
 K L

Relevant chains would be {L, John} and {K, John}. The second is indistinguishable from a chain that starts in the 'lowest' *John* and reaches all the way up to K. That's good, for that's actually the chain that we want to send to interpretation. The chain {L, John} is relevant only for the purposes of reconstruction. And of course there is no chain between K and L -a fact that you want your notation to reflect.

Consider the status of chains and the operation Multiple Merge, constructing chains. Both are terminological conveniences. No operations of L apply to chains. Principles holding of chains can be expressed directly in terms of occurrences (e.g., the uniformity condition on bar level), as can interpretive operations referring to chains: for example, principles of theta-role assignment and surface interpretation discussed earlier, or conditions on reconstruction.

In what follows, Chomsky is taking 'chain' to be just the object that you obtain when movement takes place. That has as a consequence that none of the interpretive properties that researchers have explored of chains (most obviously the Kitahara/Hornstein facts about scope) can be extended to putative chains formed by agree.

³² An occurrence defined in terms the "free" relation Immediately Contain is an X^{max} except in the case of multiple-SPEC. If necessary for some reason, the notion could be modified to require it to be an X^{max} here as well (perhaps taking the occurrence to be a pair (K, XP), XP the smallest X^{max} containing K). But sharpening may be superfluous. Occurrences are significant only for moved elements,

I'm not sure I agree with that. Suppose Kitahara/Hornstein are right in that scope is (partially) determined by A-chains. Suppose further that many A-chains are not movement chains, but agreement ones. Then you'll need occurrences to give you the different scopal possibilities, without having movement to start with.

Some heads H have a property P that determines that H heads an occurrence; P is the EPP property of H, commonly taken to be a selectional feature satisfied by merging K. K may be an expletive or a category determined by probe-goal agreement and pied-piping; the latter is the case of multiple Merge. With no apparent substantive change, we may dispense with multiple Merge and reconstrue Move as the operation Agree/Pied-Pipe/Mark, where Agree holds of (probe H, goal G) as before, and Mark identifies H as the head of an occurrence H^p of the Pied-Piped category K determined by G.

In other words, Mark is responsible for pronouncing the category determined by G at the H site. From this perspective, there should not be any chains when mere Agree (and no Mark) obtains.

How Mark identifies H is a terminological matter: it could be, for example, by constructing an "occurrence list" to which we add the pair $\langle K, H^p \rangle$.

Aside from adding Mark to the system, we would have to also add objects like $\langle K, H^p \rangle$.

K now appears only once in the syntactic object formed, in the position of initial Merge, a theta-configuration if K is an argument.³³ Under the simplest assumptions, a principle of phonology spells K out at its highest (i.e., maximal) occurrence in the course of the cyclic derivation, as already discussed; and surface interpretation attends to the highest occurrence as well.³⁴

From this perspective, Mark is a totally different operation from Move, meaning by that something plus Merge. The main empirical concern is whether only these objects are ‘chains’.

Under the conventional interpretation, satisfaction of EPP (by Merge or Move) forms the new category $H^{\max} = \{K, H^p\}$ with label H and an H-projection H^p as occurrence of K. Under the interpretation just outlined, satisfaction of EPP adds the new item $\{K, H^p\}$ to the occurrence list. The two approaches are essentially the same, virtually down to notation.

To be fair, in the conventional interpretation, it could be said that EPP was always satisfied by Merge, either simple Merge or the Merge embedded under Move.

There are differences in the computational operations, but they seem insignificant. And sharpening of the concepts does not seem to raise very serious difficulties.

As for EPP, under the former interpretation, but not the latter, it is a selectional feature. In either case, it is an uninterpreted feature ("feature," as usual, meaning just linguistic property), an apparent imperfection, which we hope to show is not real by appeal to design specifications (perhaps along lines sketched in MI and elsewhere). Resort to an uninterpreted feature is inescapable. Something distinguishes among heads H that require, allow, or disallow that H head an occurrence of K. And while there may be semantic consequences to displacement, these are surely not properties of an interpretable feature of the head. Expletive constructions do not have the semantic properties of overt-subject constructions and similar considerations hold for OS, as discussed; the semantic properties of in-situ vs. displaced wh- are not plausibly expressed as properties of the interrogative head; and so on.

³³ This is the theta-theoretic principle (6) of MI, a direct consequence of the Hale-Keyser conception of theta theory adopted there. The added assumption is that violation of the principle, which is detectable at once, causes crash.

³⁴ Consider successive-cyclic movement that begins by forming an A-chain and then proceeds to form an A'-chain; standard wh-movement of HP, for example. It is not necessary to think of the single chain as broken up into two for the purposes of interpretation. Rather, there are two sets of occurrences for HP, which is merged just once: the set of occurrences of H (the A-chain) and of wh (the A'-chain).

Again, the ‘so-on’ depends in part on what one wants to do with the Kitahara/Hornstein treatment of QR as A-movement. In part, of course, that also depends on how the system treats different kinds of object shift, a matter that is still open. Similar considerations apply to Nunes’s treatment of parasitic gaps, or virtually all approaches to control in the minimalist literature.

If this analysis is correct, then we can proceed freely to use the conventional chain notation with all of its conveniences, understanding that it is reducible to the more stern "official" theory with no chains or multiple Merge.

A broader category of questions has to do with the "internalist" conception of language adopted in this discussion, and in the line of inquiry from which it derives for the past 40 years, a branch of what has been called "biolinguistics."³⁵ FL is considered to be a subcomponent of Jones's mind/brain; Jones's (I-)language L is the state of his FL, which he puts to use in various ways. We study these objects more or less as we study the system of motor organization or visual perception, or the immune or digestive systems. It is hard to imagine an approach to language that does not adopt such conceptions, at least tacitly. So we discover, I think, even when it is strenuously denied, but I will not pursue the matter here. Internalist biolinguistic inquiry does not, of course, question the legitimacy of other approaches to language, any more than internalist inquiry into bee communication invalidates the study of how the relevant internal organization of bees enters into their social structure [**but see fn.36!**]. The investigations do not conflict; they are mutually supportive. In the case of humans, though not other organisms, the issues are subject to controversy, often impassioned, and needless.

There’s much to say here about that and about what follows, but I’ve spoken about it elsewhere.

We also speak freely of derivations, of expressions EXP generated by L, and of the set of such EXPs -- the set that is called "the structure of L" in Chomsky (1986), where the I/E- terminology is introduced.³⁶ Evidently, these entities are not "internal." That has led to the belief that some externalist concepts of "E-linguistics" are being introduced. But that is a misconception. These are not entities with some ontological status; they are introduced to simplify talk about properties of FL and L, and can be eliminated in favor of internalist notions. One of the properties of Peano's axioms PA is that PA generates the proof P of " $2+2 = 4$ " but not the proof P' of " $2+2 = 7$ " (in suitable notation). We can speak freely of the property "generable by PA," holding of P but not P', and derivatively of lines of generable proofs (theorems) and the set of theorems, without postulating any entities beyond PA and its properties. Similarly, we may speak of the property "generable by L," which holds of certain derivations D and not others, and holding derivatively of an expression EXP formed by D and of the set {EXP} of those expressions. No new entities are postulated in these usages beyond FL, its states L, and their properties. Similarly, a study of the solar system could introduce the notion HT = {possible trajectories of Halley's comet within the solar system}, and studies of motor organization or visual perception could introduce the notions {plans for moving the arm} or {visual images for cats (vs. bees)}. But these studies do not postulate weird entities apart from planets, comets, neurons, cats,... There is no "Platonism" introduced, and no "E-linguistic" notions: only biological entities and their properties.

³⁵ See Jenkins (forthcoming).

³⁶ The purpose was to avoid consistent misunderstanding of the technical usage of the notion "grammar" and others, but the proposed terminology was further misunderstood as suggesting that there are two topics, I- and E-linguistics (I- and E-languages, etc.), the latter concerned with utterances, corpora, behavior, Platonic objects, etc. The E-concepts, however, were defined as "anything else," intended to identify no coherent object or study. Furthermore, utterances and behavior are no more or less a concern of I-linguistics than of studies of language that might be called (misleadingly I think) varieties of E-linguistics.