

# Off the Chain<sup>\*</sup>

Juan URIAGEREKA (UMD) & Ángel J. GALLEGO (UAB)

## 1. Proposals for chain dependencies

Minimalism has no agreed-upon approach to chains. The key problem, as noted by Collins & Groat (2018), is that an all-you-need-is-MERGE logic (Chomsky 2008, Chomsky et al. 2019) is not enough to distinguish repetitions (selected from the Lexicon) from copies (taken from the derivational workspace that carries the computation). The matter can be illustrated with (1):

$$(1) \quad \{\mathbf{X}, \{Y, \{Z, \mathbf{X}\}\}\}$$

In this set-theoretic object we have two instances of  $X$  (in bold); the question is if they are independent or, in some sense, a “discontinuous” object. With elaborate representational objects like traces or indices placed in quarantine, minimalism has explored different formulations of corresponding dependencies. Chomsky (1995, 2000, 2001), for instance, resorts to Numerations (NUM) to track the distinction. Given a NUM like  $\{X_2, Y_1, Z_1\}$ , in (1) we have two tokens (repetitions) if  $X$  is reduced to zero ( $X_0$ ), but two copies (occurrences) if  $X$  is reduced to one ( $X_1$ ).

Consider that as in (2) and (3), where the key step is (c) in each instance.

$$\begin{aligned} (2) \quad & \text{NUM} = \{X_2, Y_1, Z_1\} \\ & \text{a. MERGE } (Z, X) = \{Z, X\} \quad \rightarrow \text{NUM} = \{X_1, Y_1, Z_0\} \\ & \text{b. MERGE } (Y, \{Z, X\}) = \{Y, \{Z, X\}\} \quad \rightarrow \text{NUM} = \{X_1, Y_0, Z_0\} \\ & \text{c. MERGE } (X, \{Y, \{Z, X\}\}) = \{X, \{Y, \{Z, X\}\}\} \quad \rightarrow \text{NUM} = \{X_0, Y_0, Z_0\} \\ (3) \quad & \text{NUM} = \{X_2, Y_1, Z_1\} \\ & \text{a. MERGE } (Z, X) = \{Z, X\} \quad \rightarrow \text{NUM} = \{X_1, Y_1, Z_0\} \\ & \text{b. MERGE } (Y, \{Z, X\}) = \{Y, \{Z, X\}\} \quad \rightarrow \text{NUM} = \{X_1, Y_0, Z_0\} \\ & \text{c. MERGE } (X, \{Y, \{Z, X\}\}) = \{X, \{Y, \{Z, X\}\}\} \quad \rightarrow \text{NUM} = \{X_1, Y_0, Z_0\} \end{aligned}$$

---

<sup>\*</sup> We dedicate this piece (part of Uriagereka & Gallego in progress) to the memory of Sam Epstein, Roger Martin, and Carme Picallo, with whom we discussed many of these matters. We appreciate comments from Omer Preminger and Juan Romero to an earlier draft.

Subscript reduction in (2) indicates that the two tokens in the numeration have been accessed, unlike in (3)—as the attentive reader will notice. But, of course, in this fashion, we again have to assume the paraphernalia of indices. Alternatives presuppose, e.g., multi-dominance or an independent operation (like COPY) to implement the distinction (cf. Chomsky 2005, Epstein et al. 1998, Gärtner 2002, etc).<sup>1</sup> Chomsky (2008) pursues yet another approach where MERGE, plus phase-level memory, is supposed to capture the distinction. This tactic has known two implementations. In the first, whether we have repetitions or copies follows from the computation having accessed the lexicon once or more times. The second implementation focuses on distinguishing merge instantiations: If the higher X in (1) was created by internal MERGE (IM), we have a copy; if by External MERGE (EM), a repetition.

Conceptually intriguing as they may be, all such options pose difficulties related to those arising for indices or an additional operation like COPY. As Collins & Groat (2018) point out, the MERGE-alone approach should be able to clarify how lexical selections (or the application of EM vs. IM) is available *at the phase level*:

In either case—be it the number of lexical selections or the distinction between IM and EM—memory of preceding workspaces in addition to the current workspace is not just required, but needs to be encoded. This can be done in various ways, by using chains or diacritics for example. But then we are back to square one, since we have already established that using chains and diacritics is not consistent with [the Inclusiveness Condition].

[from Collins & Groat 2018: 5]

So the difficulty is not so much in clarifying chain properties as have accumulated for years; it is, rather, in finding a sound hypothesis about how these objects are formed, in a streamlined framework that dispenses with indices, numerations, traces, etc.

In the next section, we argue that chains could be understood as making use of a process of Case *tokenization*, which we take relates to the operation AGREE.<sup>2</sup> Our approach has an obvious connection with locality, since it invokes phases by directly involving C and v\* (assuming these are the loci for  $\phi$ -features). From this, we will argue that this very phase involvement correlates with the well-known distinction between A and A' chains, which we try to refine as well.

---

<sup>1</sup> The operation COPY is available if MOVE is a composite operation, more complex than MERGE. Chomsky (2004) simplifies that approach by analyzing MOVE as (internal) MERGE.

<sup>2</sup> We do not assume that Case *depends* on agreement, “is checked” on an agreement site, or any related proposals. We make our own assumptions about Case explicit in sections 8, 9, and 10.

## 2. A Case-based approach

Martin & Uriagereka (2014) suggest an approach to chains with ingredients of what we are about to say. Their intuition stems from Uriagereka's (1997) attempt to reinterpret sameness / distinctness in terms related to Case. The gist of the proposal is that we find repetitions (and distinctness) of X and Y ( $X=Y$ ) if we have different Cases on each, as in (4); and copies (and sameness) if the same (single) Case obtains instead, as in (5).<sup>3</sup>

(4) *John* accused *John*.

(5) *John* was accused <*John*>.

In Martin & Uriagereka's (2014) proposal, two instances of the same element are regarded as copies if they are "too close", within a relevant domain. The tricky matter is how to define that closeness.<sup>4</sup> Heuristically, the metric is straightforward for (6), but less so for the well-known situations in (7), all related to (5) in some form—and all presenting *John* instances "far away" from one another:

(6) *John* will <*John*> criticize himself/*Mary*.

- (7) a. *John* seems to have been criticized <*John*>.  
b. *John* seems to have been referred <*John*> to the principal.  
c. *John* seems to have had <*John*> acquaintances report on him.

The phenomenology in (7), involving (clear or arguable) A-movement, suggests that something "Case sensitive" is going on, just as in (4)-(5) above.

Martin & Uriagereka (2014) analyze closeness in terms of phases, interpreting elements in the complement domain of a phase as copies. This poses two interesting problems. One is that it has nothing obvious to say about A' chains, where relevant dependencies are *across* domains. Second, an issue also arises for cases like (8) or (9), where the different *John* instances seem reasonably close, *a priori*:

---

<sup>3</sup> The brackets denote a copy, although it is meaningless to signal one particular occurrence as *the* copy, in detriment of the other (see Orús et al. 2018 for an alternative formulation).

<sup>4</sup> The clearest case is Self-MERGE (Chomsky 1995, Guimarães 2000, Kayne 2010), which we put aside for now, although we are happy with the conclusion that iterations of the *John-John* type, as discussed in Uriagereka (2008:chapter 4), are also of this sort.

(8) *I saw John's (picture of) John.*

(9) *I sent John to John.*

For either (8) or (9), it is simply not possible to say that one *John* is inside the v\*P phase and the other outside—for they both are inside the same (and no other) v\*P.<sup>5</sup> So why are we then able to treat these elements as distinct repetitions?

One could attempt an approach based, instead, on mere Case considerations (of the sort Uriagereka 1997 had): two elements with different Case (within some Case domain, see Uriagereka 2008 chapter 5) are distinct, not otherwise. The issue of course is what to do with an example like (10), then, where *John* exhibits a regular accusative in the lower instance and an exceptional accusative in the higher repetition.<sup>6</sup>

(10) *I believe John to like John.*

To recap, we seem to need a combination of a Case-based analysis (to account for (8) and (9)), and a domain-based analysis (to accommodate (10)). So, we have two ways to proceed: call them A and B, for elements  $X = Y$  (which we denote as  $X, X$ ):

For  $\{X, \{X, \{ \dots \} \} \}$  a syntactic object in domain/phase D at workspace W:

(11) *PLAN A*  
X and X are *repetitions* if they have a different Case values within D,  
X and X are *copies* (occurrences) if they have the same Case value within D.

(12) *PLAN B*  
X and X are *repetitions* if they have Case within D,  
X and X are *copies* (occurrences) if one has no Case within D.

---

<sup>5</sup> Also inside DP in (8). Note that, although it may be debatable whether DP is a phase, both elements are inside (the same) DP—so one would have to argue that NP also counts as a phase.

<sup>6</sup> More dramatically, the same can be said about (i), if the same Case obtains for both NPs:

(i) *Mary is (and will always be) Mary.*

Each *Mary* appears with the same Case (nominative in Spanish). In that scenario, one could try to correlate the identity reading to there being copies, while the predicative reading could follow from having repetitions instead. Interestingly, the readings have a different pronominalization pattern in Spanish, as shown in (ii)—a matter we will leave open.

(ii) *María {lo / la} es.* (Spanish)  
María CL<sub>ATR</sub> CL<sub>ACC</sub> is  
'María is that / her'

To reiterate the obvious: the alternatives in (11) and (12) are not Case *theories*, since they are not even theories, as such. They are attempts to rationalize a distinction, in ways formulated in sections 8, 9, and 10. Plan A is obviously more complex, and it predicts that situations with more than one dependent with the same Case value should be ruled out. Consider that prediction first (we return to Plan B momentarily).

While Obligatory Contour Principle effects restrict combinations of like-elements,<sup>7</sup> situations with multiple accusative, dative, nominative, or genitive, have simply been shown to exist. For example:

- (13) a. *I sent him (John) him (Peter).*  
 b. *Mary (Smith)'s Mary (Jones)'s child loves his grandmother.*

It may be telling that double objects as in (13a), multiple genitives as in (13b), and multiple nominatives or datives, are all marked.<sup>8</sup> Perhaps UG does not set this type of situation as a default for a learner, and whenever it arises, other factors (word order, cliticization, person differentiation...) need to be invoked so as to break the relevant Case identity. So one could pursue plan A, but then it is necessary to start revising what categories count as phases, what the complement and edge domains are, whether the relevant “repulsion” effect concerns labels or features, and so on.

Plan B is simpler, if only because it dispenses with Case sameness considerations. This option amounts to a view where (at least) one copy lacks a Case value (possibly even when endowed with a Case attribute). One version of this problem is addressed by Nunes (2004: p.70 and ff.). He proposes that Case is checked in the highest copy only, an additional mechanism (*Case Uniformization*) deleting Case features of lower copies. One could also ignore the need for some copies to have a Case value, possibly for the same reason said copies are “invisible” for the purposes of IM, AGREE or labeling (Chomsky 2001, 2008, 2013). We want to flag the matter, however, because the issue seems to us challenging on all variants in the literature. The problem is why some copies should be “invisible”,<sup>9</sup> or why a “uniformization” mechanism should need to be invoked, treating copies differently from repetitions,

---

<sup>7</sup> For various reasons, ranging from symmetric c-command, to anti-locality, touching on minimality, distinctness, etc.; cf. Colomina (in progress) for a summary.

<sup>8</sup> We are thinking of multiple subjects in East Asian languages, Romance causatives, or ethical/illocutive datives added onto structures with regular datives, etc.

<sup>9</sup> It is often assumed that lower copies are “invisible”, for unclear reasons. There is a formal asymmetry between the highest chain copy and the others, in terms of c-command, but that need not have a bearing on how (or where) the chain should be pronounced or interpreted.

which do not get “uniformized”. This is of course at the crux of the Collins & Groat (2018) critique.

We agree with Collins & Groat (2018) that related proposals face identical challenges; this, thus, reduces to the famous “syllogism” that, since this is everybody’s problem, it is not *our* problem. Tongue-in-cheek as that remark is made, we do not know how to go into resolving this particular issue without fundamentally altering the system (see Orús et al. 2018 for an alternative). What matters for our purposes here, however, is that the repetition/copy distinction must be anchored on Case considerations, for A-chains at least. Next, we explore where that idea leads.

### 3. Chain types: A and A'

Implementation aside, we have assumed that one X and a second X count as repetitions if they both have Case—be it the same or not. This is enough for A chains, but is at right angles with A' chains. The GB literature provided us with a rich phenomenology separating these chain types,<sup>10</sup> in terms of theta-theory, Case, binding, locality, “reconstruction” effects, and so on (cf. Safir 1982, Barss 1986, Lasnik & Uriagereka 1988, Sauerland 1998, Abels 2003, Epstein 2006, among countless others). The first question, therefore, is whether there is a principled way to predict such distinct phenomenology. The very idea that Case plays a role in A, but not A', chains raises the issue of distinguishing copies from repetitions among the latter too.

Consider the fact that an interrogative sentence can be analyzed in two ways:

- (14) *Which man left?*

The classic GB approach assumed a “composite chain” involving two A and one A’ position, raising obvious uniformity problems (Lasnik & Uriagereka 2005: ch. 4):

- (15) [CP [DP *which man*] [TP [DP *which man*] [VP *left* [DP *which man*] ] ] ] ?  
 (A') (A) (A)

<sup>10</sup> In that theoretical approach, the Visibility Condition (Aoun 1979, Vergnaud 1985, Chomsky 1986b) takes elements bearing a  $\theta$ -role to be associated to a case, for some (non-obvious) reason. We are not making any commitments in that regard, especially with respect to a further assumption in the classical approach: that Case itself associates to the first chain occurrence and the  $\theta$ -role to the last chain occurrence. In section 6, we show Case assignment without movement, and in section 7, A-movement unrelated to case purposes.

An alternative approach is suggested in Uriagereka (1998:5.7): two independent chains connect through the foot of each, the first-Merge position of *which man*. Discussing independent matters, Chomsky (2008) also alludes to that very sort of analysis, taking A and A' chains to be created independently ("in parallel"):

It has been conventionally assumed that in such constructions . . . there is an A-chain formed by A-movement of the wh-phrase to Spec-T, and an A'-A chain formed by A'-movement of the subject to Spec-C. There was never any real justification for assuming that there are two chains, a uniform A-chain and a non-uniform A'-A chain, rather than just one A'-A-A chain formed by successive-cyclic raising of the wh-phrase to Spec-T and then on to Spec-C . . . *There is no direct relation between the wh-phrase in Spec-C and in Spec-T, and no reason to suppose that there is a nonuniform chain at all: just the argument A-chains and an operator-argument/variable construction.*

[from Chomsky 2008:149-150, our emphasis, U&G]

In that way to proceed, we need to ask if it makes sense to assume that *which man* (in CP) and *which man* (in v\*P) are actual copies—and what would go wrong with their being repetitions instead. For A' chains, the issue is not so much whether repetitions involve different arguments (as expected among different A chains). In an A' chain, we normally need an operator at a scopal position and a corresponding variable at the initial merge configurations, with the associated restriction in intermediate sites. Of course, it is unclear whether the way to achieve *that* is through copies or repetitions...

Chomsky & Lasnik (1991) address the asymmetric nature of A' (operator-variable) chains and discuss how "reconstruction" is supposed to let the semantics use the relevant part of the wh-phrase in each position. Now, one aspect of the move towards a minimalist program was to question the existence of a computational process of that particular kind; in fact, copies (first introduced in Chomsky 1955/1975) were recovered to address the matter instead, thus raising the kinds of questions we are now facing. Semantically, we want the variable in a thematic position and the corresponding operator with its scope fixed. It may then seem as if interpreting the operator "down" (not in the scope position) is simply illogical. But that, as such, is just *vacuous quantification*, which is perfectly logical as it would unnecessarily complicate any logical formalism to rule it out (see Potts 2002 for perspective). In other words, "the semantics" will simply not get the quantifier into its proper scope position in those terms.

A related possibility pertains to so-called type-clashes, as discussed in Heim & Kratzer (1998). Possibly, an operator may not converge at the semantic interface if it involves the wrong type (in particular, if the context demands an “entity” type, and what we have is, for some reason, more complex, as assumed for quantifiers). As such, however, all that possibly does is force the element from staying at the relevant  $\theta$ -position—it does not make it take the appropriate scope (see Lasnik & Uriagereka 2005: chapter 6). While the semantics is equipped to interpret what the syntax hands it, it is harder to see how it would guarantee that we get the requisite (operator-variable) relations. We should not expect of the semantics more than we would of the phonetics; if we give “the wrong type” to PF (e.g., a stop consonant in a stress position), it may complain about that, but it is less clear how it would dictate the shape, for example, of phrasal stress. When it comes to determining which part of a chain ends up an operator and which a variable (and whether that has a bearing on the matter of copies vs. repetitions) that too seems like a syntactic question. We cannot say we know of a worked out semantic theory to tell us how we ought to settle it as the derivation goes to LF.

Consider next the derivation of *Who did John like?*

(16)  $[_{CP} [_{DP} \textit{Who}] C [_{TP} [_{DP} \textit{Who}] [_{v^*P} \textit{John v^*} [_{VP} \textit{like} [_{DP} \textit{Who}]]]] ] ]?$

Let us start by focusing on the  $v^*P$  phase associated to  $v^*$ . Suppose the derivation reaches a point in which *Who* has to raise to an outer Spec- $v^*P$ :

(17)  $[_{v^*P} \textit{John v^*} [_{VP} \textit{like Who}]]$

At this point, we should have two options: to apply EM (base generation, lexically selecting some *Who*) or IM (displacement) to the *Who* already in the derivation. In the first case, we have a repetition; in the second, a copy. Given the logic of our approach to A chains, one may ask whether Case has a bearing on the decision. But *Who* is not involved in the  $v^*P$  edge for Case reasons, so the rationale to invoke Case is not there, to start with. In consequence, if Case somehow matters, we ought to have a repetition.

While arguably problematic, that situation is not new. Suppose the derivation above reaches the stage in (18) (putting aside whether the top *Who* is copied or repeated):

(18)  $[_{\alpha} \textit{Who} [_{John v^*} [_{\beta} \textit{like Who}]]]$



At this point, TRANSFER applies, which expunges  $\beta$  (the complement domain) or makes it inaccessible (Chomsky et al. 2019),<sup>11</sup> so the derivation only sees articulated materials within  $\alpha$ , not *into*  $\beta$ . Right there, the system must decide whether *Who* is copied or repeated. But it cannot, unless it has access to the cashed out material—which may contain arbitrarily many clauses. The situation we are considering is entirely the same.

We are arguing that, under such circumstances, the system goes for the repetition option, as it is the less complex. Why? Simply because it does not need to compare *Who* with any other element; so a repetition is the default option for the derivation. Of course, the possibility that A' chains involve repetitions (not copies, as standardly assumed, setting aside resumption analyses)<sup>12</sup> should have a direct impact for interpretation, ideally fitting the A / A' asymmetry we have been discussing. In what follows, we explore the effect this claim may have on “reconstruction” effects.

#### 4. “Reconstruction” effects

Chomsky (1995) rules out “reconstruction” for A chains, but this is problematic, both conceptually and empirically. If A chains involve copies, why should it not be possible to interpret those copies? Chomsky alludes to there not being operator-variable dependencies in A-chains, but it is not obvious why *that* should be what licenses the “reconstruction”, particularly when it is unclear what is behind the dependency itself.

Empirically, well-known data directly argue for A-chain “reconstruction”:

- (19) a. [<sub>TP</sub> [*Rumors about each other*] *seem to them* [<sub>TP</sub> *t to be unfounded*] ]  
       b. [<sub>TP</sub> [*No doctor*] *is believed* [<sub>TP</sub> [*t to be at the scene*] ]  
       c. [<sub>TP</sub> [*No criticisms of each other's theory*] *seem* [*to any two linguists*<sub>1</sub>]  
           *to appear* [*to their advisors*<sub>2</sub>] [*t to be without merit*] ]

Examples like (19a) were used by Belletti & Rizzi (1988) to argue for the licensing of anaphors “anywhere” in the derivation; that could be analyzed in terms of interpretation

<sup>11</sup> While syntactic effects can be shown to obtain after TRANSFER, this does not rule out the process: post-TRANSFER effects may obey accessibility conditions weaker than those expected of fully phrase-markers (see Lasnik & Uriagereka forthcoming on this).

<sup>12</sup> Resumptive pronouns as in (i) have always suggested that the Wh-phrase associated to them (via an operator-variable pairing) is moved or “base-generated”:

(i) ***Which candidate*** *are donors wondering whether* ***he*** *can win?*

at a lower copy.<sup>13</sup> Examples as in (19b) have a long history, reviewed in Lasnik (2012) and dating back to Partee (1971)—who showed scope rigidity for predicates like *be certain* under those circumstances. Iatridou & Sichel (2011) demonstrate, however, how Partee’s (1971) restriction does not affect predicates like *seem*, *appear*, or passive forms as in (19c); again, it is natural to account for these “lowering” effects (in the sense of May 1977) in terms of lower copy interpretation. Finally, (19c) is built based on similar examples from Lasnik & Funakoshi (2011). The example obviously challenges the idea of there being no “reconstruction” for A-chains.

The previous section suggested that A chains may be different from A’ chains, the latter (somehow) being composed of repetitions, not copies. This should have drastic consequences, if correct. To begin with, in the narrow syntactic component, some mechanism must clarify why the repetitions are unified phonetically, so that normally each separate copy is not pronounced (see Nunes 2004 for the situation with copies). But there should be differences also in the interpretive components, e.g. vis-à-vis “reconstruction”. This brings us to an interesting set of questions arising in terms of what Lebeaux (1991, 2009) called the “single tree” condition, which we prefer to call, essentially following ideas in Hornstein (1995), *Chain Uniqueness*. At issue is whether a given chain, understood as a set of copies (occurrences), has a unique point of interpretation. In a sense, this cannot be the case for A’ chains, given our discussion in the previous section. And *prima facie*, an example like (19c) immediately challenges Lebeaux’s (1991, 2009) view, even for A chains.<sup>14</sup> But the matter can be pushed.

First, this is our approach: whether there is uniqueness of interpretation should correlate with how many cases a given dependency involves. Starting with the A chain, we expect just one. Then again, in the Lasnik & Funakoshi (2011) examples, there are *three* Case associations for a phrase like *no criticisms of each other’s theory*. While the entire phrase receives whatever Case it does (nominative in (19c)), parts thereof

---

<sup>13</sup> While it is not obvious how *them* c-commands the anaphor in the “reconstructed” site, (i) shows a corresponding obviation effect arising from that very position:

(i) [TP *It seems to him* [CP *that John is smart* ] ]

The obviation in (ii) also (arguably) shows that “reconstruction” cannot bypass a binding filter:

(i) [TP *He seems to him* [TP *t to be smart* ] ]

<sup>14</sup> A piece of data we will not dwell on involves the inability of *multiple* A-reconstructions:

(i) a. \***Each other** seem to **themselves** to be friendly.  
b. **Each other** seem to **themselves** [**each other** to be friendly]

(ia) should be possible if *each other* is interpreted in two positions at once (UP, so as to license the anaphoric *themselves*, and DOWN, so as to be licensed by *themselves*). There could be other semantic problems with the example, though—like “referential circularity”.

also receive genitive associated to *of* and *'s*. It is not straightforward how putative chains related to *each other's* or *of each other's theory* work, but that is squarely the rationale behind the challenge the example poses: it is “reconstructions” *at those chains* that break the overall uniqueness of the containing chain. So this matter ought to be resolved.<sup>15</sup>

If we set aside challenges of the Lasnik & Funakoshi (2011) type, not only do we know of no argument against Lebeaux's (1991, 2009) uniqueness, but we furthermore can think of at least one argument *for* it (and see also fn. 14). Any such argument cannot decide what ultimately boils down to whether it would make sense to interpret (at least) two different copies *independently*, which in most relevant instances would violate just about any version of the  $\theta$ -criterion (if trying to interpret more than the lexical semantics licenses). But now consider these contrasts:<sup>16</sup>

- (20) a. ***Nobody*** is home (for ***any*** of them). [idiomatically: “They are insane.”]  
 b. (To me) ***nobody*** seems [ to be home (for ***any*** of them)].  
 c. ***Nobody*** seems to me [ to be home (for ***any*** of them)].  
 d. ***Nobody*** seems to ***anyone*** [ to be home (for ***any*** of them)]. [Idiom lost.]

The reading of interest for (20a) is idiomatic. With Chomsky (1995), we may assume that, in raising conditions, strong idioms (i.e. not just light verbal periphrases or merely poetic/colorful compositions) require “reconstruction”, as in (20b) or (20c). This is so as to keep *nobody home* unitary for the idiomatic interpretation, as opposed to in standard compositional fashion (in other words, one has to interpret the lower copy of *nobody* together with *be home*). Note that the idiomatic reading, as such, does not prevent *nobody* from licensing c-commanded polarity *any* in (20a-c).<sup>17</sup> Then the key is (20d), where we have “pinned” *nobody* to the upper clause, so as to license

<sup>15</sup> We will not address the matter here, since it presupposes, at the very least, developing a serious theory of what we will be calling, shortly below, “super-agreement”.

<sup>16</sup> While relevant, a related paradigm on the basis of data as in (i) could be challenged:

(i) ***Nobody*** seemed to ***anyone*** (??to appear) to care about a (damn) thing.

Licensing of two polarity items by a single *nobody* in subject position is a possibility (although, to us, the example significantly degrades when they are separated by an intermediate TP, if the second polarity item is weak, and see (21)). But it may be that what is involved here is *multiple agree* from a single probe associated to *nobody*, which is consistent with the example degrading across another TP. (20) is designed so as to avoid that loophole.

<sup>17</sup> We suspect this means the idiom *nobody is home* preserves its downward-entailing nature even in the idiomatic interpretation, or it would not be able to license the polarity item within its adverbial *for any of them*. The example does get worse, however, if the polarity item is weaker; thus observe how only the literal reading is left in (i):

(i) ***Nobody*** is home (for a damn soul).

polarity *anyone* there under c-command. Crucially, then, the idiomatic interpretation appears to be lost. But if each of the copies could be interpreted separately—contrary to what *Chain Uniqueness* presupposes—that reading should be possible.

We can next run a similar test for A' conditions. Consider an example involving Negative Inversion, with a polarity interpretation for *anyone*, to which we have added another polarity item, *a freaking soul*, which seems to require a clause-mate licenser.<sup>18</sup>

(21) *Nobody does anyone believe a freaking soul can arrest!*

If (21) is grammatical, the displaced *nobody* must sanction both *anyone* and *a bloody soul* in the two subject sites that the sentence involves. As far as we can tell, this requires *nobody* to license *a bloody soul* from the intermediate CP position, and finally *anyone* from the peripheral landing site.<sup>19</sup> In our terms, this correlates with there being (at least) two repetitions of *nobody* in (21), as opposed to just two copies, as in (20).

Having considered what our analysis of A and A' chains has to say about some “reconstruction” nuances, we would like to discuss, next, how a series of repetitions can somehow give rise to a unitary—if discontinuous—object.

## 5. How to “chain” repetitions

The proposal above has consequences for construal or long-distance dependencies—not only wh-movement, but also control, parasitic gaps, *tough*-movement, and so on. What we have said for A chains is rather standard. In a situation like (5), repeated here as (22), the two instances of *John* are copies, as only one has Case:

(22) *John was accused <John>.*

<sup>18</sup> Ross (1967) discussed the long-distance licensing of polarity items, even across his “islands”. Several since Linebarger (1980) have studied a variety of such effects, of the sort in (ia) (built from a comparable example in Chierchia 2013:392). What matters now is (ib), though:

(i) a. *I doubt Bird drunk, did any drugs, and played well at once.*

b. ??? *I doubt Bird drunk, did a gooddamn drug, and played well all at once.*

The reason we invoke one of these stricter polarity items in the text (in (21), *a freaking soul*) is so as to ensure a local licensing—not from the left-periphery (and involving the multiple licensing of both polarity items in the sentence), irrelevantly for our purposes.

<sup>19</sup> The loophole in fn. 16 may not arise here if the displaced *nobody* is too distant in the left periphery from directly licensing, via agree, both the closest and the remote polarity item.

Thus the system treats both NPs as the same, to form a chain. Which compares to:

(23) *Who did John accuse <Who>?*

For (23), we have argued that *Who* in CP and *Who* in v\*P are separate repetitions.

To be explicit, consider the first-merge position for the repetition of *Who*:

(24) [<sub>v\*P</sub> Who [<sub>v\*P</sub> John v\* [<sub>VP</sub> accuse Who ] ] ]

It is only at this point that the system can tell whether the identical elements are copies or repetitions—at the next phase, it should not, if the system loses access to the phrasal nuances of what was transferred. So how are the repetitions, then, regarded differently at the interpretive components, so as to allow the interpretation of multiple repetitions (multiple “reconstruction”), while pronouncing just one at the phonetic interface? Of course, this is also related to the, perhaps more obvious, question of how something like (23), with intermediate structure (24), never gets to be interpreted as “which are the individuals such that John accused *other* individuals?”<sup>20</sup>

The situation is not qualitatively different from the well-known ones below:

(25) *What do you think what Cookie Monster eats?*

(26) a. *Mary tried [ PRO to leave town alone/\*together]*  
b. *Mary asked (Bill) [PRO to leave town alone/together]*

(27) *He is the man (I know) who Mary likes.*

(28) *Which book did you file t [ Op without reading e ]?*

(29) *A man easy to please is easy [ Op (for anyone) to convince t ]*

---

<sup>20</sup> The problem may be even clearer with multiple points of view, as in (i):

(i) *Which idiot did people say John accused?*

Can (i) mean “which individual, who I think is smart, did people, who think s/he is an idiot, say John accused that individual? The complexity of these dependencies can grow as follows:

(ii) *Which candidate are donors wondering about the alleged idiot whether the ultimately decent bastard can win?*

These phenomena illustrate circumstances where two arguably independent chains, related to the boldfaced elements, compose into one (Chomsky 1982, 1986), forming a “super-chain” (Uriagereka 1998: ch. 5). It is easy to show that none of these examples are chains in any traditional sense, even if attempts exist to argue otherwise, at least for the cases with the strongest identity (like Obligatory Control as in (26a), e.g. as in Hornstein 1999). In our view, it is best to keep all of these as separate A or A’ chains, seeking a mechanism to unify the repetitions implicit in that approach.

To us, it is telling that identity conditions in each of those are actually very different. Control illustrates this best, inasmuch as it comes in several varieties (even more than those in (26)). So, we may suppose that the identification mechanism for the super-chain—call it “non-distinctness”—may hold at different comparing sizes: a phrase, a head, even features within a head. The intuition is akin to specifying the merger of two companies, like Pixar and Disney, which can be partial or total in ways that could affect their stock, legal status, nationality, and so on. Intuitively, we would want to say that this super-merge of super-chains is at the level of XP for obligatory control, *tough*-movement, or possibly parasitic gaps; but only at the level of heads for partial control and, perhaps, some relative clauses and resumption.

In a sketch like the present one, it is impossible to fully specify the possibilities that emerge (e.g. when super-MERGE of different sizes yield what, and with what scope), but the way to proceed seems relatively straightforward. To be sure, this “super-MERGE”, whether of XP or X, would have to be specified the way we have for regular MERGE, understanding what is presupposed and entailed, how the mechanisms differ, etc.<sup>21</sup> In any case, dependencies as in (25) / (29) plainly exist and are, in fact, as well known as complementations or predications.<sup>22</sup>

While saying that does not resolve our approach to super-chains, it seems to us less problematic than to pretend that all one needs is, well, pure and simple MERGE.

---

<sup>21</sup> Or for that matter, compounding of various sorts, at an even more basic level.

<sup>22</sup> Although we will not go into this here, donkey-anaphora in Geach’s (1962) terms seems to be of the same sort too, even though real chain conditions have not been proposed for it:

- (i) a. *Everyone who owned a donkey beat it.*  
b. *Everyone who owned a donkey beat the poor beast.*  
c. *Everyone who owned a donkey beat the relevant beast (that he or she owned).*

We do not want to treat (ia) as describing a situation where whoever owned a donkey beat, say, their neighbor’s—even though there is no chain formation between *a donkey* and *it* in (ia). So the interpretation in (ia) ought to be related to some version of the paraphrase in (ic), the epithet in (ib) being an incomplete (definite description) version (see Uriagereka 1993). If such an analysis is possible in (i), it may extend to cases as in (25)/(29) or fn. 20.

To be clear: names aside, our “super-MERGE” is *not* MERGE, in any way we understand it (external, internal, first, late, elsewhere, parallel, sideways, submerged, etc.). In that sense, we are not particularly worried about why, together with (25)—which is common in child language, even for English speakers (Crain & Thornton 1998) and well-known across languages (see den Dikken 2017)—we do not find (30):

(30) \* *What type of food do you think what type of food Cookie Monster eats?*

To us, the fact that this is clearly disallowed, while a corresponding super-MERGE as in (25) is fine, is in the league of why control or parasitic gaps typically do not involve the pronunciation of multiple repetitions, as in (31) or (32):

(31) *He expected/wanted [ (\*him)/(Mary) to be smart]*

(32) *Which book did you file t [ (\*which book) without reading e ]?*

In (31) there is no obvious reason why *him* is ungrammatical when a gap is possible (what used to be called the “Avoid PRO” strategy, see Chomsky 1981), particularly when a noun non-co-referent with the controller is fine. The situation is equally clear for (32), where Case is not even at issue: why can the second *which book* (a repetition in standard accounts) not be pronounced, where we can easily argue it is situated?<sup>23</sup>

For what this is worth, some realizations of full-repetitions (as opposed to heads), in conditions of non-distinctness, are more felicitous when the repetition contexts are separate, as in appositives (33) or across islands (34) (see fns. 20, 22):

(33) “‘*Vir qui fecit*’ is equivalent to ‘*Vir, qui vir fecit*’, ‘*the man, which man did it.*’”  
[from Anthon 1854: 28]

(34) *He is the man (I know) who John wonders why I like him/the poor devil.*

This suggests to us that whatever conditions hold for the formation of super-chains are sensitive to matters of Multiple Spell-Out (Uriagereka 2012). In other words, just as Nunes (2004) worried about how to linearize regular chain occurrences, and had special provisos for the putative realization of repeated copies under morphologically separate heads (e.g. in clitic reduplication), we will need to be sensitive to how clean the space is that connects non-distinct chains, in terms, at least, of linearization.

---

<sup>23</sup> For instance, via Subjacency tests for the parasitic gap—see Chomsky (1986).

## 6. Inherent case and trivial chains

We have shown full-fledged A-chains, with putative interpretation at the copies, as in (19) above. A related question is whether A chains are ever trivial (without various steps). It boils down to whether there is short-movement to Spec-T or Spec-V (à la Koizumi 1995, Lasnik & Saito 1991, Lasnik 2002). For subject raising, the matter concerns the EPP. The object raising situation has to do with ECM constructions as in (35a), although Chomsky (2007, 2008, 2015) extends it to all object dependencies.<sup>24</sup>

- (35) a. *Nobody expected Arthur* [ <Arthur> to remove the sword].  
b. *Nobody expected* [ *Arthur* V <Arthur>].

In Chomsky's (2015) analysis, subjects and objects raise because T and  $\sqrt{R}$  are too weak to label their projections,<sup>25</sup> so the situation in (35b) is uniformized to (35a).

The theoretically strongest position to take is that standard A chains are always non-trivial, although this may not be true in situations of inherent case.<sup>26</sup> We started our discussion with passives, which generally involve two copies,<sup>27</sup> but inherent case chains would not seem to pose this particular question to begin with. There are no obvious movements, for example, in the following instances involving partitive case:

- (36) *Ogirik nahi duzuia?* (Basque)  
bread-part want it.aux.you  
“Do you want (of some) bread?” [from De Rijk 1972]
- (37) *Hän pani kirioja poöydälle.* (Finnish)  
He put the books.part on the table [from Renault 1984]

While in the Basque instance there is overt agreement between the auxiliary and the theme, in minimalist terms that does not entail movement (see Preminger 2014, Polinsky 2018). Proposals about how partitive is “assigned” have varied, especially when considering its putative, apparently associated, semantic effects—related to indefiniteness (Belletti 1988), specificity (Enç 1991), atelicity (Kiparsky 2005), or

---

<sup>24</sup> This “object shift” may be even more general than implied in Chomsky's works, and unrelated to Case consideration (as motivating the process). We return to relevant examples.

<sup>25</sup> That is, the relevant NPs label corresponding syntactic objects, a C-I demand.

<sup>26</sup> We are using the term Case, with capitals, to refer to the structural phenomenon, and the lower-case case to refer to the inherent phenomenon.

<sup>27</sup> This, of course, is not obvious in “pro-drop” languages, as in Spanish versions of any passive, which allow the agreeing subject in EPP position or post-verbally.



whatever else may matter.<sup>28</sup> Our point here is more modest: simply that, syntactically, it is quite unclear whether any movement is involved in these particular chains of theme arguments, which would thus seem to be trivial, as chains in themselves. That is consistent, also, with any semantics expecting such elements to be scopally “low”, for nothing can “be high” that is neither base-generated, nor moved, high...

Implicit in that discussion is the notion “inherent case”, introduced in Chomsky (1986) to separate it from “structural Case”, in the classical sense. The idea is that case can be directly associated to  $\theta$ -role assignment, in much the way that Fillmore 1968 first made his case for recovering the notion of case from a tradition dating back to antiquity. Chomsky wanted a set of (typically oblique, often overt) cases to occur “inherently”, without the need of chain formation, then reserved for the “structural” instances. Woolford (2006) carefully shows how there may be more distinctions needed, as one may require also to separate idiosyncratic “lexical” case from genuinely systematic inherent case. The matter does not get any simpler with studies on applicatives,<sup>29</sup> and whether designated, non-obvious, structural positions within the clausal skeleton turn out to be responsible for that particular class of dependents—in terms of  $\theta$ -roles, case conditions, or both.

Complexities of the sort just flagged are too intricate for us to go into here, although we can provide a flavor (of the sort discussed in Dvořák 2001 for Czech, for instance) with recalcitrant dative dependencies of the following sort:

- (38) a. *They sent Ulysses to Achilles.*  
       b. *Ulysses was sent to Achilles.*  
       c. *Achilles was sent Ulysses.*
- (39) a. *They married Cary Grant to Betsy Drake.*  
       b. *Cary Grant was married to Betsy Drake.*  
       c. *\*Betsy Drake was married Cary Grant.*

In both (38) and (39) we have ditransitive structures, but whereas both internal arguments may undergo A movement to Spec-T in (38), this is not possible in (39).<sup>30</sup>

---

<sup>28</sup> For instance, an implicit polarity item, if Russian genitives are included (Pesetsky 1982).

<sup>29</sup> The growing literature on this topic includes Marantz (1993), Pesetsky (1995), Pytkäinen (2002), Harley (2002), McGinnis (2002), Anagnostopoulou (2003), Cuervo (2003), McFadden (2004), McIntyre (2006), Pineda (2014), etc.

<sup>30</sup> One may relate the asymmetry to the following contrasts, a matter we will not pursue now:

- (i) a. ?? *They sent Ulysses with Achilles (in Troy).*  
       b. \* *Ulysses and Achilles sent (in Troy).*

To complicate things further, bear in mind that some dative-marked NPs are known to preserve their case even under passivization, as shown in (40) for German or (41) for Icelandic so-called quirky NPs, in that instance in ECM contexts:

- These examples are closer, though still not identical, to (35): Here dative morphology remains, which may indicate that the case is “inherent” (or “lexical” in Woolford’s 2006 sense). Of course, in (35) the Case showing up in the upper position is nominative.

It is interesting to ponder whether we ever have situations in which either (i) an element displaces via A-movement to an inherent case-marking site, or (ii) an inherently case-marked element displaces via A-movement to a structural Case-marking site.

(ii) a. *They married Cary Grant with Betsy Drake (in Vegas).*  
b. *Cary Grant and Betsy Drake married (in Vegas).*

relations.<sup>31</sup> Consider, in this regard, a standard causative in the Romance languages, as in the following Italian example:

- (42) *Ho fatto lavare la macchina a mio figlio.* (Italian)  
 Have.I made wash the car to my son  
 ‘I made my son wash the car’

While *a mio figlio* (Eng. ‘to my son’), is dative in (42), the phrase in point is in no obvious way receiving a  $\theta$ -relation from a plausible source for inherent dative case, for example as common for experiencers. This must mean that this particular use of the dative is actually structural, and if so (42) is entirely analogous to (35b)—thus involving genuine copies of *a mio figlio*, in our terms.

That conclusion is different from the one Řezáč (2008) reaches when discussing Basque data from Artiagoitia (2001), who argues for the structural nature of ergative and absolutive Cases. The facts are straightforward, so we will not review them: they show how there exist ergative expletives and ergative/absolutive can be associated to non-thematic positions. Řezáč (2008:97) concludes that, in contrast, “there is no raising to dative and no dative-type agreement with non-thematic” nominals and that, therefore, dative (unlike absolutive or ergative) is theta-related. But examples have been shown in which the argument marked dative—the causee of the action that the (ergative) subject propitiates—bears an entirely different  $\theta$ -role in various circumstances: agent in (43a), patient in (43b), and theme in (43c), for instance:

- (43) a. *Pellok Maddiri ogia janarazi dio.* (Basque)  
 Peter.ERG Mary.DAT bread.ABS eat.CAU AUX:3SG.3SG.3SG  
 ‘Peter made Mary eat the bread’  
 b. *Pellok Maddiri eztul eginarazi dio.*  
 Peter.ERG Mary.DAT cough make.CAU AUX:3SG.3SG.3SG  
 ‘Peter made Mary cough’  
 c. *Erregeak gerlari iraunarazi zion.*  
 king.ERG war.DAT last.CAU AUX.PST:3SG.-.3SG  
 ‘The king made the war last (a long time)’  
 [from Oyharçabal 2003:230-231]

It is unlikely for these causative-induced datives to be theta-related in any obvious way.

<sup>31</sup> The possibility does exist, however, in theories within which  $\theta$ -roles are assigned as features, as in Bošković (1994), Lasnik (1995), Bošković & Takahashi (1998), Takahashi (1998). This is crucially so for Hornstein (1999), or Manzini & Roussou (2000), as this is precisely the mechanism they use to implement obligatory control dependencies.

Examples as in (43) parallel (42), in unrelated languages. The situation may also be behind the English contrasts in (44), which hark back to that between (38) and (39):

- (44) a. *John was told that linguistics is in crisis.*  
 b. *John was made to show his support for the field.*  
 c. *\*John (was) appeared that linguistics is in crisis.*

In a language with dative alternations, an indirect object may displace to an open subject position, as in the passives (44a) or (44b), the latter of the causative shape in (42) or (43). But the process is unsystematic, as the well-known ungrammaticality of (44c) shows. We thus suggest to assume that the experiencer of *appear* receives inherent dative, unlike the goal of *tell* or, even more plausibly, the causee (embedded) underlying subject in (44b).

Consider, next, situation (ii), where we seek an element in an inherently case-marked position that A-moves, albeit *not carrying its case*. While (44a) could have been seen as such an example, a fact questions that conclusion: that we have shown how the case in (44a), when it surfaces as dative, is plausibly structural (as is obvious for the corresponding (44b)). Perhaps pseudo-passives provide a better example, then:

- (45) a. *We can rely on Mary/her.*  
 b. *Mary/she can be relied on.*

Here, it seems clear, (a) that *Mary* as in (45a) is receiving some (overt) oblique case, and (b) that when *Mary* displaces as in (44b), it shows up with a nominative structural Case.

Perhaps (44b) involves an intermediate transformational step akin to a dative shift. Many such alternations, of the sort below from Catalan (which allows for the oblique clitic *en* “one”) and Spanish (which does not have oblique clitics), suggest a connection between oblique and corresponding structures in dative or accusative guise. In other words, perhaps a pseudo-passive like (45) first involves a transformational step, essentially akin to the Spanish (47b), which is closely related to (46b).

- (46) a. *La Maria es va riure de la Núria.* (Catalan)  
           the María CL laughed.3.sg of the Nuria  
           ‘María laughed at Nuria’  
 b. *La Maria se’n va riure, de la Núria.*  
           the María CL laughed.3.sg of the Nuria  
           ‘María laughed at Nuria (on Nuria)’

- (47) a. *María se rió de Nuria.* (Spanish)  
 María CL laughed.3.sg of Nuria  
 ‘María laughed at Nuria’  
 b. *María se le/la rió.*  
 María CL CL.DAT/.ACC laughed.3.sg  
 ‘María laughed (to) him’

[adapted from Roca 1992]

In effect, the Spanish example may be the structural variant of the inherent Catalan (46b), with either dative or accusative (depending on dialect) showing up in association to a clitic corresponding to the basic phrasal structures in (46a) or (47a).

Still reflecting on scenario (ii) above, consider Basque examples as in (48):

- (48) *Mirenek ematen du Aitorrek liburua eman diola* (Basque)  
 Miren.ERG seem Aitor.ERG book give AUX:3SG.3SG.3SG Comp  
 ‘Miren seems that Aitor gave her the book’

[adapted from Salaburu 1987]

There are two issues here: first, the possibility that the subject of a psych verb may show up as ergative; second, that it may have moved from a lower dative site. In that regard, Ricardo Etxepare (p.c.) finds (49) comparable to Salaburu’s example:

- (49) *Mirenek ematen du Aitorrek Xabierri liburua eman diola* (Basque)  
 Miren.ERG seem Aitor.ERG give AUX:3SG.3SG.3SG Comp  
 ‘Miren seems that Aitor gave Xabier the book’

Etxepare also remarks how, to him, the examples recall the English ones in (50):

- (50) a. *Mary seems as if John gave her the book.*  
 b. *Trump seemed yesterday as if the impeachment gave Melania the chills.*

What sort of relation exists between the matrix subject in these psych constructions and some linked phrase in the lower clause, to justify the relevance of the subject’s state to those particular terms? Judging by the vague “aboutness” in (50) or (51b), it is doubtful that the relation in point is movement at all (and see Landau 2009).

In that sense, the colloquial Galician (52) may be more relevant to our question in scenario (ii), as it involves an experiencer from the matrix clause,

somehow promoted to nominative position, contrary to what we saw for (42c). To be clear, observe how the subject overtly exhibits nominative case:

- (51) *Eu paréceme que el vai chover.*  
 I.NOM seem-CL.DAT that it goes rain  
 ‘I seems to me [it seems to me] that it is going to rain’

Note, however, that (51) is not quite fully productive, first. Thus compare:<sup>32</sup>

- (52a) a. ? *Ti parecíache que el ía chover deste xeito?*  
 you.NOM seem-CL.DAT that it went rain of-this guise  
 ‘Did you seem [it seem to you] that it would be going to rain like this?’  
 b. ?? *E eles pareceralles que el vaia chover tamén alá?*  
 And they.NOM seem.may-CL.DAT that it go.subj. rain also there  
 ‘And may they seem [it seem to them] that it may rain there too?’

And observe how the examples manifest *two* cases: nominative in the strong subject pronoun and dative in the associated clitic. Eliminating the clitic is ungrammatical:

- (53) a. \* *Eu parece que el vai chover.*  
 I.NOM seems that it goes rain  
 ‘I seems that it is going to rain’  
 b. \* *Ti parecía que el ía chover deste xeito?*  
 you.NOM seem that it went rain of-this guise  
 ‘Did you seem that it would be going to rain like this?’  
 c. \* *Eles parecerá que el vaia chover tamén alá?*  
 they.NOM seem.may that it go.subj. rain also there  
 ‘May they seem that it may rain there too?’

So, in rigor, these Galician examples are also not exactly what we are seeking; which boils down to us *not* having, in fact, found an illustration of subcase (ii), presenting an inherently case-marked element A-moving to a structural Case-marking site. Although we are entirely open to discovering such cases in other languages, for the remainder of the article we will proceed under the assumption that they do not exist.

---

<sup>32</sup> The example recalls cases as in (i), which are archaic or somewhat humoristic in English:

- (i) a. *Methinks that would be quite remarkable.*  
 b. ?? *Youthinks that would be quite remarkable?*  
 c. ?\* *Themthink(s) that would be quite remarkable!*

These, of course, relate also to the productive Germanic instances in (40) and (41) above.

## 8. Towards a Theoretical Approach

To summarize, the possibilities discussed so far are as in the following table:

CHAIN REACH	NOMINATIVE SITE	DATIVE SITE
[1] PASSIVE INDIRECT OBJECT	NOMINATIVE HEAD	STANDARD TRACE [RC]
[2] QUIRKY SUBJECT	DATIVE HEAD	STANDARD TRACE
[3] “AS IF” SUBJECT	NOMINATIVE/ERGATIVE	STANDARD TRACE? [RC?]
[4] PSYCH SUBJECT	NOMINATIVE	CLITIC [RC?]

Subcase [1] is straightforward: if a language allows dative alternations, the element that would otherwise receive (shifted) accusative Case can be promoted to subject position, thereby possibly receiving nominative; this is not really a situation *starting* in an inherently case-marked site, as its emergence relates to whatever underlies the phenomenon of “reduced Case” (RC), associated to passives in general. Subcase [2] is more relevant to our concerns, although only partially: inasmuch as the case showing up at the chain head is the one associated to its foot; this does not say anything about the “A-status” of the chain, only that it is at right angles to the issue of non-trivial chains associated to inherent case. It is likely that the movement associated to the quirky subject—A-movement as it appears to be—involves other requirements having little to do with case, of the sort possible in scrambling (for clearly case-marked phrases). Subcase [3], as such, could be the most relevant, if the subject (showing up in ergative in Basque and nominative in English) does in fact come from a lower goal position; not likely in the embedded clause, but possibly in the matrix—if the structure is parallel to [4]. The latter would be perfect illustrators of the situation we are after, except they involve a clitic in the trace position...

We are exploring these possibilities in the event that inherently case-marked phrases may, in fact, displace to structural-Case sites; which may be behind a step in creating A chains for elements that otherwise would seem to stay *in situ*. The very fact that an example like (52a) is grammatical in Galician, but a corresponding one is ungrammatical in English, suggests one of two things. Either the clitic “doubling” is essential, for some reason, to the Galician structures (and English lacks that phenomenon), or what is plausibly an inherent case experiencer in English is a structural one in Galician. This is under the assumption that what licenses these displacements from an A-site to another, where Case is determined, is what we are calling RC (for “reduced Case”), as in standard passives. In a sense, then, this is the “structuralization” of case, apparently associated to a case reduction, which ends up in the characteristic  $\theta$ /Case mismatches. It seems harder to find the inverse picture: a structural Case that becomes inherent. Our basic approach to chains expects this asymmetry: We cannot “inherenticize” a structural Case any more than we can make a

non-trivial chain trivial. In contrast, “structuralizing” an inherent Case for us just means complicating the corresponding chain (making what is done, done more elaborately). But we reach this conclusion tentatively, as it is possible to falsify our claim: finding an A-chain starting in an inherently case-marked position that could not be plausibly re-interpreted in structural terms (e.g. from oblique case to structural dative Case), then showing how that chain proceeds with its derivational life, ending up in a structural Case marking site.

All of this raises a deeper question, to which we have not found a satisfactory answer: what *is* case to start with, whether we capitalize the term or not? We can list the, admittedly puzzling, attributes that we associate to the phenomenon, as follows:

(54) *C/case puzzles*

- a. C/case associates to arguments (not genuine adjuncts).
- b. While there are some “base-generated” inherently case-marked arguments...
- c. ... Structurally Case-marked arguments may involve non-trivial chains.
- d. While inherently case-marked arguments correlate with some  $\theta$ -role...
- e. ... Structurally Case-marked arguments go with uninterpretable Case features.
- f. There is no interpretive reason for arguments to bear C/case.
- g. There is no interpretive reason for operator-variable pairs to bear C/case.

These speak for themselves. We wish we could make deeper generalizations, but we want to at least come clean in terms of what it is that we are assuming, including places where we are the first to admit we have no idea what is ultimately going on.

We want to be candid, also, with regards to what we are *not* assuming, since we do not know how to make sense of it within the Minimalist Program:

(54) *Familiar assumptions about C/case* [not assumed by us].

- a. C/case is a morpho-phonemic requirement on overt noun phrases.
- b. C/case is a requirement on elements bearing a  $\theta$ -role.
- c. C/case is a requirement on operator-argument pairs.
- d. C/case is checked (locally) within a given configuration.
- e. C/case is a reflex of A-movement.

None of these ideas is senseless, but none of them appear to really work either—for empirical or conceptual reasons that we think is useful to put on the table.



(54a) was shown to be incorrect by Freidin & Lasnik (1981), immediately after the Case Filter (amounting to (54a)) was proposed (based on the thesis that became Vergnaud 1985). Their crucial example was of the form in (55f):

- (55) a. *Whom have they regarded t as worthy?*
- b. *Who have they regarded t as worthy?*
- c. *He is who/\*whom they have regarded t as worthy.*
- d. *\*Who have they tried t to leave?*
- e. *He is the man (who/whom) they regard t as worthy?*
- f. *\*He is the man (who/whom) they have tried t to leave?*

(55a) shows case-marking on a moved wh-phrase; (55b), that this case need not be morphologically of the sort one expects from the source of the movement. (55c) is a relative clause, which actually forces the morphological case (nominative) that one expects in the operator site—not the trace. In (55d) we attempt to move the wh-phrase from a caseless position and the result is bad. (55e) and (55f) are versions of the example involving a relative clause, which adds a twist: relative operators can be null in English, as the optionality of *who/whom* demonstrates. Finally (55f) introduces the punch-line: when we attempt to form a relative clause analogous to (55e), albeit from a caseless movement site (parallel to (55d)), the result is impossible—even if the relative operator is null. Conclusion: case is not dependent on the morphological realization of the case-bearer, contra (54a).

(54b) sounds like a deeper requirement, in that it cares not about the morphology of an expression, but whether it bears a  $\theta$ -role. We have shown, however, that there is no bijective correspondence between case conditions and  $\theta$ -roles, beyond instances of inherent case. In fact, even for inherent instances the foundation of the relevant condition is tenuous. For suppose we were to assert the exact opposite of the classical Visibility Condition: “if X bears a  $\theta$ -role, then X must *not* bear case features.” There is nothing logically incoherent in this *anti*-visibility requirement, but of course it yields the wrong empirical results. This is quite different from saying that if two items are not linearized they cannot be pronounced at PF, or that if a given element has the wrong semantic type, it cannot stay in the site where it cannot be composed; asserting the opposite in either of those instances is problematic for the interpreted interfaces—but there is nothing semantically problematic in *not* marking arguments for case... So while Chomsky’s (1986) Visibility Hypothesis may give us some mileage in terms of the phenomenology, it makes no minimalist sense.

One could raise similar conceptual questions about (54c), but in that instance the issue is immediately suspect on empirical grounds. It is possible to create (pure)

adjunct long-range operator-variable dependencies—which should be impossible if C/case is strictly as in (54c). In other words, (55b) is as good as (55a);

- (55) a. Who did (you say) you knew?  
b. Why did (you say) you knew Mandela?

Obviously, just as one can ask about the individuals (you said) you know, you can also ask about the reason (you said) you know said individuals. But plainly *why* is not the sort of element we think as bearing case features. At that point, one has to hand-wave in the direction of only operator-variable pairs *of the argument sort* requiring case (not those of the adjunct kind), but that just shows how serious the problem is that we're attempting to address, particularly in light of what we said in the previous paragraph.

(54d) is a tad more modest—though perhaps too modest. After all, “local configuration” describes many things: sister to X, sister to X's projection, within X's (complement) domain (up to a phase), within X's “edge” (if X is a phase). All interesting, but really relevant to case? This used to be easier to rationalize when case was thought of as assigned “under government”, assuming that those “local domains” are all characterized under that particular umbrella. Even in the good old days this was far from obvious, though, given that “government” was thought to obtain long-distance into the complement domain of a governor, though not into its specifier domain. Which is one of the reasons the minimalist program dispensed with “government” altogether, in favor of “checking domains” (via head-specifier relations). Sure enough, one could state many case dependencies as “case checking”, but there are at least two empirical problems with that, both of them noted already.

One set of problems arises from the fact that, in many languages, one would seem to see “case checking” without overt displacement to a checking domain:

- (56) a. *There arrived several ships at the same time.*  
b. *llegaron varios barcos al mismo tiempo.* (Spanish)  
arrived.they several ships at-the same time  
'Several ships arrived at the same time'  
c. *llegaron el Bordazuri y el Artagan al mismo tiempo.* (Spanish)  
arrived.they the Bordazuri and the Artagan at-the same time.  
'The Bordazuri and the Artagan arrived at the same time'

Clearly, *several ships* in (56a) is not checking nominative case in the specifier of TP, as it is generated in its post-verbal thematic position; the structure is reproduced in pro-drop languages like Spanish as in (56b), where there is no overt pleonastic.

Belletti (1988) argued that, in situations like that, inherent partitive is assigned, of the sort we saw in (36)/(37), sec. 7—which correlates with the semantic effect associated to these existential/presentational expressions. Chomsky (1986), following Burzio (1986), had a different approach, involving an abstract chain between the pleonastic *there* and its “associate” *several ships*. In minimalist terms, the issue for (56b) is having to propose a null pleonastic—the sort of element that has no interpretive consequences *either* at LF *or* PF. Moreover, one can reproduce the same issues as in (56c), where there clearly is no “definiteness” effect. It is not plausible in that instance to argue for partitive case assignment, and yet the post-verbal names yield no ungrammaticality in Spanish (and other languages). *Ergo*: if case is indeed at issue, it must be resolved without the element needing it having to be in the “checking domain” of whatever is responsible for this case (here T associated to nominative).

A related problem was shown in regards to (36)/(37), in that the argument for partitive case has never been that it involves the “checking domain” of any specifier. To the contrary, the idea has been that pure themes, in first-MERGE position, are capable of directly receiving partitive case, so that the structure stays scopally low (e.g. as in Belletti 1988, Enç 1991, Diesing 1991, Kiparsky 2005, etc.). “Staying low” is incompatible with moving beyond the lowest verbal domain (either staying in the complement domain of the verb or raising only to the first verbal specifier, if that is even a possibility). The matter is especially problematic if partitive-case arises in the verbal complement domain, without any displacement; that would be the cleanest form of an inherent case. Clearly, though, if that is true, we plainly have two different domains in which case is resolved: the complement domain of V and various functional specifiers (TP, vP, within DP, and so on).

Bear in mind, also, that the partitive situation immediately disproves the claim in (54e) that C/case reflects A-movement. If partitive is assigned without any A-movement, there goes that generalization. Of course, (56c) also disproves that same point, under reasonable assumptions about the position of the subject there. So there too the disjunction is patent, particularly because (56c) is entirely compatible with:

- (57) *El Bordazuri y el Artagan llegaron al mismo tiempo.* (Spanish)  
 the Bordazuri and the Artagan arrived.they at-the same time  
 ‘The Bordazuri and the Artagan arrived at the same time’

In (57) the subjects are in the position they presumably occupy in English. So if movement as in (57) (or the English variant) is “for Case reasons”, then what happens in (56c)?; or conversely, if case can be licensed as in (56c) (with the subject in its  $\theta$ -position; at any rate, lower than in (57)), why is A-movement as in (57) needed?

It is for these kinds of puzzles that Chomsky (2000) went into the AGREE proposal, a long-range correlation based on the agreeing heads (a Probe and its so-called Goal(s)). But the minute one assumes AGREE, any strict motivation for Case-related “triggers” vanishes, for the formation of A-chains. Either that, or the chain has to be formed abstractly, as in Burzio (1986) and Chomsky (1986). We will not be taking a position about any of these matters here, since for the most part they do not affect our own analyses. Although we do have a final twist to add from our own theoretical angle—after we touch upon some interactions that we find rather informative.

## 9. Interactions

So far, we have mainly concentrated on core argumental instances, including oblique dependents. But, evidently, the clausal structure also presents pure adjuncts, for which there is no theta-role, Case, or even an obligatory projection, which may perhaps be related to the paucity of their long-range dependencies (no control or binding conditions, and to the extent that variable binding obtains for them, it is extremely sensitive to islands, per arguments summarized in Lasnik & Saito 1992). Consider our phenomenology, which would seem to limit us to these options:

- (58) a. X is degenerate<sup>33</sup> if no Y is in-construction-with X, other than the Y that X modifies.  
 b. X is dependent on Y if X merges with Y.  
 c. X = X are copies if they share one Case internal to domain D.  
 d. X ≠ X are dependent repetitions if one has Case internal to D while the other lacks Case at D’s edge.  
 e. X ≠ X are independent repetitions if they each have (distinct?) Cases.

Note, first, that “modifies”, as relevant to the degenerate categories in (58a), must be a weaker relation than MERGE (extending to adjectives, adverbs, and possibly even compound elements; see fn. 33). We have nothing particularly deep to say about modification here, although it is clearly pertinent to what we are about to discuss in relation to genuine adjuncts. There seem to be rather few elements that constitute

---

<sup>33</sup> We borrow the notion “degeneracy” from matrix mechanics (e.g., *singular* or *degenerate* matrices), to indicate that the status and behavior of these objects is qualitatively different from, and more restricted than, that of arguments. One could invoke other more familiar notions, like “adjoined” or “pair-merged”, but we prefer not to do that in order to avoid any particular theoretical presupposition at this point, beyond the specific limitation we see.

genuinely degenerate items, in our terms. Surely heads in dependencies as in (58b) enter into further head-head relations that we have not discussed, and core predicates undergo predicate fronting and “stylistic” adjustments. So genuinely degenerate forms that cannot be part of any long-range correlation seem really limited to compounding, incorporation perhaps, and possibly associated processes like bare noun complements to main verbs.

Per situation (58d), there is a way of bringing inherently case-marked dependents to the picture without invoking anything special. This is because they arguably deal with their case internal to (domain) D, at the point of first-MERGE where their  $\theta$ -configuration is established.<sup>34</sup> Then, if we manage to select *another* X (with relevant features) at D’s edge, we can establish a super-chain by way of the super-AGREE relation that we proposed to link the dependents of super-chains. The main difference, in this view, between structural and inherent Case is through (49c), which the structural Case requires to build its A-chain, unlike the inherent Case, resulting in a trivial chain (with just one copy). This is to say that, although an inherently-case-marked element may end up satisfying the conditions for a dependent category as in (49b), this is only if there is no other item nearby that can constitute a proper repetition in terms of the super-AGREE, leading to a super-chain.<sup>35</sup>

To be candid: if we are correct in what we have just said, the following must be true as well, as a way to address the disjunction between case and Case:

- (59) Featurally, there is only one type of case attribute [just as there is a single type of gender, number, tense, etc. attributes] which may assume different values.

It is very possible that (59) is foundational to the language faculty, for reasons related to the discussion about distinctive features in Chomsky (1974) (and before, within the tradition of morpho-phonemics). It is, of course, very interesting why there should only be one  $\pm N$  or  $\pm V$  feature per word, or for that matter  $\pm c$ ,  $\pm v$  (per phonological segment). If we take the case attribute in that light (that is, we treat it no differently from any other feature in the grammar), then it follows that, if a given domain has an inherent Case, it shouldn’t have another structural Case—just as, if we conclude a segment is consonantal, turning it into non-consonantal comes at an operational cost.

---

<sup>34</sup> Assuming case, lower case, is featurally indistinguishable from Case, upper case. See (59).

<sup>35</sup> Limitations exist. For instance, there does not seem to be either genuine A-movement or *tough*-movement from, or obligatory control into, inherently case-marked phrases, but otherwise there is a cadre of long-range possibilities, from Wh-movement to parasitic gaps.

That does not resolve the disjunction of why inherent case (say, partitive) can be dealt with in the  $\theta$ -related configuration of first-MERGE, while structural case (say, accusative) must involve the functional system instead. In that, however, the system we are studying is no different from  $\theta$ -relations themselves, which presumably can take place in terms of first-MERGE (for themes), but also in terms of other types of external-MERGE (for agents and all the rest). Not to go into the sorts of reprojections that we believe are involved in argument-taking by binary quantifiers, and similar such, possibly higher-order, operations. Even in particle physics, these multiplicities happen, and thus elementary particles can come in boson arrays (like photons) or fermion configurations (like protons), leading to huge differences (e.g. only fermions compose into atoms, etc.). It would be silly to rule either type of particle out because of the irreducible disjunction of having these two fundamental types (a particle is *either* fermion *or* a boson)—or to declare one type non-existent, thus preventing interactions among them (e.g. photons bouncing against atoms).<sup>36</sup> While we are not claiming that case should be seen as firmly established as subatomic particles, the point of logic seems to us to be the same.

For adjuncts, we need a situation similar to those already mentioned, or perhaps the one implicit for head-head dependencies (58b).<sup>37</sup> Although adjuncts fit into the degenerate category in (58a), for they only attach in terms of modification. Still, what happens if we have some degenerate category modifying X within some D, and we manage to select *another* X, with relevant features, at D's edge? This should count as a repetition, and so we may be able to establish a super-chain through super-AGREE. The phenomenology is well established: we are adding face icons to represent (un)successful, genuinely adjunctal, dependencies. The path between an operator like *How* and its associated variable as in (60a) has to be so clean that even a mere c-commanding negative as in (60d) breaks the relation ("weak island")—making the phenomenon similar to head-to-head relations (Baker 1988, Roberts 1989, Rizzi 1990, Cinque 1990, and see fn. 36). Otherwise, the dependency can be long-distance as in (60b), although not into nominal expressions, as in (60c). Whatever restrictions obtain must extend, also, to why parasitic gaps as in (60e) are impossible.

- (60) a. *How (sad) have you seen [ your friend ] ☺ ?*  
       b. *How could you say [ ☺ [you will leave ] ?*  
       c. *How have you seen [ your ☺ friend ]* (cf. "*I have seen a sad friend.*")

<sup>36</sup> Not to say one should not analyze the disjunction, since spotting problematic theoretical points has always been the force driving research—e.g., in the physics instance, into strings.

<sup>37</sup> The same sorts of questions, raised about copies vs. repetitions, surely arise for head-head dependencies too (which often actually get multiply pronounced: as in clitic and auxiliary reduplications, partial Wh-movements, multiple negatives, and much more).

- d. *How have you not said* [ ⊗ [you left] ] ?  
 e. *How have you* [ left ⊙ ] [ ⊗ [after arguing] ] ?

The logic of our approach forces us to say that all the action, for allowing a degenerate relation as in (58a) to extend into long-range correlations as in the pure adjunct paradigm in (60), stems from the super-agreement conditions. It remains to be seen what forms of agreement obtain here, however, and whether the limitation, yet again, relates to the feature system in languages—to represent person, case, and the like—is behind the boundary conditions, or instead the reason for the feebleness in these situations springs from the very nature of the initial, modificational, dependency, which is for some reason degenerate, vis-a-vis *bona fide* MERGE.

One further interesting interaction pertains to relations between a genuine A chain and a corresponding A' chain. Consider the following Catalan instance:

- (61) *Cada noia vol saber quina recomanarà ...* (Catalan)  
 every girl want.3.sg know which recommend.FUT.3.sg.  
*... el seu noi a si mateixa.*  
 the GEN boy to self same.fem.sg.  
 'Every girl wants to know which (of them) her boy will  
 recommend to herself'

Catalan marks gender in nominal expressions, including Wh-questions. Thus *quina* is not just “which”, but “which-feminine”. Taking the semantics seriously, we assume that femininity, or any other such gender, is a predicate—thus part of a quantifier’s restriction (or scope, as the case may be), but never the variable. We take the variable to be nothing but a  $\theta$ -related place-holder. The matter is relevant in (61) because the anaphor *si mateixa* (Eng. ‘herself’) also has (feminine) gender features, thus needs to be locally associated to a matching antecedent like *quina* (Eng. ‘which-fem’). In turn, obviously *seu noi* (Eng. ‘her boy’) (or, more precisely, *x*’s boy, without gender) needs to be in the scope of *quina*, as the bound variable that it is. Consider that in some detail.

The relevant structure compares to the English (62):

- (62) ?\* *Which of the girls is her boy going to recommend to herself?*

Evidently, this is a situation of Weak Cross-Over, which is not very good in English. In Catalan, however, the equivalent of (63) is perfect:

- (63) *Quina noia recomanarà el seu noi a si mateixa?* (Catalan)

which girl recommend.FUT.3.sg the GEN boy to self same.fem.sg  
 ‘Which girl will her boy recommend to herself?’

We assume the example is good because the subject in Catalan, like in many other pro-drop Romance languages, is not forced to be in the specifier of TP (Rizzi 1982). Moreover, we assume the arguments, in Ordóñez (1997) and Gallego (2007, 2013), to the effect that there are further Larson-style A-positions in the Romance v\*P periphery, from which anaphors can be licensed, especially in a ditransitive configuration like this (see Larson 1988). This is consistent, also, with the feminine licensing, which would take place from just that site, setting now aside the Probe/Goal dynamics for this. It is important to bear in mind that this part of the structure involves only an A chain related to the A-movement of *quina noia* (Eng. ‘which girl’), not the ensuing A’ chain.

The latter takes place in the next derivational step, by hypothesis at the v\*P domain, where our theory proposes the introduction of a full repetition.<sup>38</sup> One last nuance in the example involves the fact that *quina* presupposes a null (feminine) noun, the equivalent of the elided nominal associated to the bare English *which*. It can be shown that this null nominal in Catalan is licensed locally to its antecedent, which in (61) is *cada noia* (Eng. ‘each girl’) in the matrix subject position. Thus compare:

- (64) a. ?\* *Què va dir cada dona que va vender a quina?* (Catalan)  
           that tense-PAST say each girl that tense-PAST sell to which  
           ‘What did every woman say that she sold to which (of them)?’  
       b. \* *Què va dir cada dona que en Pere va vender a quina?*  
           that tense-PAST say each girl that the Pere tense-PAST sell to which  
           ‘What did every woman say that Pere sold to which (of them)?’

In (64) we have separated *quina* (Eng. ‘which-fem’) from the antecedent element licensing the null nominal (*cada dona*), and the result is ungrammatical. This must mean that *quina* in (61) is pinned to the intermediate CP, so as to be close to *cada dona* (Eng. ‘every woman’) there. Of course, that is also the domain of the selected Wh-operator, which is semantically appropriate. But it is important to bear in mind that, in addition to the operator itself, we also need the restriction—the null element associated to *quina*—in the upper site. So the situation is then one of double collapse for the Wh-phrase: the restriction must be (i) in the CP to be local to *cada dona*, and it must be also (ii) in the v\*P domain so as to super-merge to the A-chain that *quina*

<sup>38</sup> This, via lexical access to another element like *quina* (Eng. ‘which-fem’) in (51).



creates within that domain, where the variable rests satisfying its  $\theta$ -position; also the A-chain is involved in allowing the structure not to violate Weak Cross-over.<sup>39</sup>

We thus take the remarkable Catalan (61) to show our system at work in its core assumptions, with both copies for the A-chain and repetitions for the A' super-chain, which is in turn super-merged to the A-chain. Further up in the derivation there are additional operations, like the licensing of the null nominal associated to *quina*, not to speak of the other bound-variable bindings in the example. It is interesting, for what that's worth, that the example as such is not particularly hard to parse.

## 10. Case as tokenization—and beyond.

Even if we do not have a worked-out theory of C/case, how it correlates with agreement, it determines the nuances of various chain types, or for that matter what is the difference, let alone the reason, behind relevant features... we do have some parting thoughts on a rationale for the case-dependent phenomenology.

The theoretical issue boils down to teasing apart repetitions from copies. Departing from a (too optimistic) view that all-you-need-is-MERGE, we have presumed, to begin with, that most syntactically significant operations happen around nouns and their (extended) projections. It is these nominal elements that rampantly agree, move, enter into correlations with other elements, and otherwise force us to ask about copies and repetitions. If one didn't have them, what solid evidence could we show that the syntactician's bread-and-butter is really happening, especially long-distance (for preposition, adjective, or even verbal projections)? But all of that is patent in the nominal system, at that point forcing us to reflect not just on separate tokens of nominal types (repetitions), but also surprising occurrences of such tokens (copies)—over a wide range of syntactic contexts.

Having said that, it would appear as if the phenomenon of C/case features relates to the tokenization of nominal (extended) projections in a local domain, what minimalists think of as a phase (see Gallego 2010, 2012 for discussion and references). Like other AGREE-related phenomena, C/case manifests itself in terms of executing values on a type of attribute, thus yielding particularly identified features—which apparently are indelible: once attribute  $\Phi$  gets a C/case value  $\varpi$ , the relation  $\varpi\Phi$  itself does not change. While the indelibility is easy to give up (if one finds evidence for that flexibility), the idea that Case valuation determines

---

<sup>39</sup> However that happens, which is still unclear; see Lasnik & Uriagereka (1988) for the facts.

tokenizations of given nominal types seems central. Intuitively, although  $\varpi\Phi$  may not reliably tell us what particular role a given nominal element is playing, it may tell us *whether* the nominal is associated to this or that configuration within a phase—and *whether* other elements with such specifications are further tokens or, instead, constitute occurrences of the same token.<sup>40</sup> Seen that way, C/case may be seen as a device to “hold things together” in a certain sense, within a dynamical system that needs to be used, and in particular, parsed.

Could the syntax have evolved without C/case? Certainly: to begin with, as a system of thought without requiring externalization of any kind. Even in an externalized system it may have, at the cost of more parsing ambiguity perhaps—adjuncts coming close to showing what that system would look like. We are even prepared to assume the possibility that uninterpretable Case features (the purely structural ones, without any association to  $\theta$ -roles) are accidental: morphemes that lost their original function, in a socio-historical context. We have suggested even a name for this type of approach, the Virus Theory of morphology (Uriagereka 1998). It is too late in this context to argue for such a view, but it seems perfectly viable to us, even with the added twist that such “viruses” need to be eliminated by the computational system, with ensuing derivational dynamics. The point of mentioning this is a) to cover our bases in terms of why it still seems as if C/case has *no rhyme or reason*, and b) to be deliberately provocative about the possibility that a “glitch” could have momentous consequences, if happening within the elegant confines of a computational system.

In truth, it does not matter much now, although the prospect of testing, verifying, and pursuing these sorts of ideas seems well worth the trouble. This is particularly so inasmuch as, if we are correct, the ensuing dynamics have vast consequences not just for the head vs. XP distinction—and within that, the adjunct vs. argument distinction, and within *that*, the A / A' distinction—but more generally for “reconstruction” effects and other long-range correlations, some of which seem to

---

<sup>40</sup> Our proposal relates to the one in Hinzen (2014), although it is also quite different (see also Hinzen 2018). Hinzen’s is a theory of reference, as relating to case; ours is not a theory, but a theoretical *move* in the direction of nominal tokenization. We can’t see how to relate Hinzen’s theory to instances in which case manifests itself in token elements with no independent reference, like adjectives. On the other hand, if Hinzen’s theory is supposed to be about structural Case only (not other forms of where case manifests itself morphologically), the issue is relating all these instances, which we have shown seriously interact. And we have not touched upon them all: for example, it is unclear why only structural Case relates to the person agreement system. This is why we keep admitting that we ultimately do not understand the phenomenon.

require the creation of what we have called super-chains. The ensuing architecture sketches a curious scenario, composed of specific hierarchies of grammatical elements. These range from features to super-chains, going through words, phrases, and chains. In the end, if what we have said proves even remotely correct, A and A' chains are significantly different kinds of creatures: A chains are copy arrays, whereas A' chains are combinations of repetitions instead, with a notable impact on different, if related, formal objects, as well as their resulting interactions.

## References

- Abels, K. 2003. Successive cyclicity, anti-locality, and adposition stranding. Ph.D. Dissertation, University of Connecticut, Storrs.
- Anthon, Ch. 1854. *An introduction to Latin prose composition*. New York: Harper & Brothers.
- Aoun, J. 1979. "On Government, Case-marking, and Clitic Placement". Mimeographed, MIT.
- Artiagoitia, X. 2001. "Seemingly ergative and ergatively seeming". In J. Herschensohn et al. (eds.), *Features and Interfaces in Romance. Essays in Honor of Heles Contreras*, 1-21. Amsterdam: John Benjamins.
- Artiagoitia, X. 2001. "Irudiak eta Emaileak". *Anuario del Seminario de Filología Vasca Julio de Urquijo*, XXXVI. Donostia: Gipuzkoako Foru Aldundia. 29-64.
- Baker, M. C. 1988. *Incorporation*. Chicago: University of Chicago Press.
- Barss, A. 1986. Chains and Anaphoric Dependence. Ph.D. Dissertation, MIT.
- Belletti, A. 1988. "The Case of Unaccusatives". *Linguistic Inquiry* 19: 1-35.
- Belletti, A. 2004. "Aspects of the low IP area." In L. Rizzi (ed.), *The structure of CP and IP*, 16-51. Oxford: Oxford University Press.
- Belletti, A. and L. Rizzi. 1988. "Psych-verbs and  $\theta$  theory." *Natural Language and Linguistic Theory* 6: 291-352.
- Bošković, Ž. 1994. "D-Structure,  $\Theta$ -Criterion, and Movement into  $\Theta$ -positions". *Linguistic Analysis* 24: 247-286.
- Bošković, Ž. and D. Takahashi. 1998. "Scrambling and Last Resort". *Linguistic Inquiry* 29: 347-366.
- Chierchia, G. 2013. *Logic in grammar. Polarity, free choice and intervention*. Oxford studies in semantics and pragmatics 2. Oxford: Oxford University Press.
- Chomsky, N. 1955. The Logical Structure of Linguistic Theory. Mimeograph, Harvard University. [published in 1975, New York: Plenum]
- Chomsky, N. 1982. *Some concepts and consequences of the theory of government and binding*. Cambridge, MA: MIT Press.

- Chomsky, N. 1986a. *Barriers*. Cambridge, MA: MIT Press.
- Chomsky, N. 1986b. *Knowledge of Language: Its Nature, Origin, and Use*. New York: Praeger.
- Chomsky, N. 1995. *The Minimalist Program*. Cambridge, MA: MIT Press.
- Chomsky, N. 2000. "Minimalist inquiries: The framework". In R. Martin et al. (eds.), *Step by step. Essays on minimalist syntax in honor of Howard Lasnik*, 89-155. Cambridge, MA: MIT Press.
- Chomsky, N. 2001. "Derivation by phase". In M. Kenstowicz (ed.), *Ken Hale: A life in language*, 1-52. Cambridge, MA: MIT Press.
- Chomsky, N. 2004. "Beyond explanatory adequacy". In A. Belletti (ed.), *Structures and beyond. The cartography of syntactic structures (vol. 3)*, 104-131. Oxford, NY: Oxford University Press.
- Chomsky, N. 2007. "Approaching UG from below". In U. Sauerland and H-M. Gärtner (eds.), *Interfaces + recursion = language? Chomsky's minimalism and the view from syntax-semantics*, 1-30. Berlin: Mouton de Gruyter.
- Chomsky, N. 2008. "On phases". In C. Otero et al. (eds.), *Foundational issues in linguistic theory. Essays in honor of Jean-Roger Vergnaud*, 134-166. Cambridge, MA: MIT Press.
- Chomsky, N. 2013. "Problems of Projection". *Lingua* 130: 33-49.
- Chomsky, N. 2015. "Problems of Projection. Extensions". In *Structures, Strategies and Beyond*, E. di Domenico et al. (eds.), 1-16. Amsterdam: John Benjamins.
- Chomsky, N. and H. Lasnik. 1991. "Principles and Parameters Theory". In W.S.J. Jacobs, A. van Stechow and T. Vennemann (eds.), *Syntax: An International Handbook of Contemporary Research*. Walter de Gruyter, Berlin.
- Chomsky, N., Á.J. Gallego and D. Ott. 2019. "Generative Grammar and the Faculty of Language: Insights, Questions, and Challenges". In Á.J.Gallego and D.Ott (eds.), *Generative Syntax: Questions, Crossroads, and Challenges, Catalan Journal of Linguistics*: 1-33.
- Cinque, G. 1990. *Types of A' Dependencies*. Cambridge, MA: MIT Press.
- Collins, C. and E. Groat. 2018. "Distinguishing Copies and Repetitions." Ms. <https://ling.auf.net/lingbuzz/003809>
- Colomina, M.P. in progress. Repulsion effects: On local clitic and DPs combinations. PhD Dissertation, UAB.
- Crain, S., and R. Thornton. 1998. *Investigations in Universal Grammar: A Guide to Experiments on the Acquisition of Syntax and Semantics*. Cambridge, MA: MIT Press.
- De Rijk, R. 1972. Studies in Basque syntax: relative clauses. Ph.D. dissertation, MIT.
- Dikken, M. den. 2017. "Predication in the Syntax of hyperraising and copy raising". *Acta Linguistica Academica* 64.1: 3-43.
- Enç, M. 1991. "The semantics of specificity". *Linguistic Inquiry* 22: 1-25.

- Epstein, S. and D. Seely. 2006. *Derivations in Minimalism: Exploring the Elimination of A-chains and the EPP*. Cambridge: Cambridge University Press.
- Epstein, S., E. M. Groat, R. Kawashima and H. Kitahara. 1998. *A derivational approach to syntactic relations*. New York: Oxford University Press.
- Gallego, Á.J. 2007. Phase Theory and Parametric Variation. Ph.D. Dissertation, UAB.
- Gallego, Á.J. 2010. *Phase Theory*. Amsterdam: John Benjamins.
- Gallego, Á.J. 2012. *Phases. Developing the Framework*. Berlin: De Gruyter.
- Gallego, Á.J. 2013. "Object Shift in Romance". *Natural Language and Linguistic Theory* 31: 409-451.
- Gärtner, H.-M. 2002. *Generalized transformations and beyond: Reflections on minimalist syntax*. Berlin: Akademie Verlag.
- Geach, P. 1962. *Reference and Generality: An Examination of Some Medieval and Modern Theories*. Ithaca (New York): Cornell University Press.
- Guimarães, M. 2000. "In defense of vacuous projections in bare phrase structure." *University of Maryland Working Papers in Linguistics* 9: 90-115.
- Hale, K. and S.J. Keyser. 2002. *Prolegomenon to a Theory of Argument Structure*. Cambridge, MA: MIT Press.
- Heim, I. and A. Kratzer. 1998. *Semantics in Generative Grammar*. Malden (MA): Blackwell.
- Hinzen, W. 2014. "On the rationality of Case". *Language Sciences* 46: 133-151.
- Hinzen, W. 2018. "On the rationality of grammar". In Á.J. Gallego and R. Martin (eds.), *Language, Syntax, and the Natural Languages*, 164-181. Cambridge, UK: Cambridge University Press.
- Hornstein, N. 1995. *Logical Form: From GB to minimalism*. Oxford: Blackwell.
- Hornstein, N. 1999. "Movement and Control". *Linguistic Inquiry* 30: 69-96.
- Iatridou, S. and I. Sichel. 2011. "Negative DPs, A-movement, and scope diminishment". *Linguistic Inquiry* 42: 595-629.
- Kayne, R. S. 2010. "Antisymmetry and the lexicon." In A. M. Di Sciullo and C. Boeckx (eds.), *The Biolinguistic Enterprise*, 329-353. Oxford, UK: Oxford University Press.
- Koizumi, M. 1995. *Phrase structure in minimalist syntax*. Ph.D. Dissertation, MIT.
- Larson, R. 1988. On the Double Object Construction, *Linguistic Inquiry*, 19-3.
- Landau, I. 2009. "This Construction Looks like a Copy Is Optional". *Linguistic Inquiry* 40.2: 343-346.
- Lasnik, H. 2002. "Clause-mate conditions revisited." *Glott International* 6: 94-96.
- Lasnik, H. and M. Saito. 1991. "On the subject of infinitives." In *Papers from the 27<sup>th</sup> Regional Meeting of the Chicago Linguistic Society, Part I: The general session*, L.M. Dobrin, L.Nichols, and R.M. Rodriguez (eds.), 324-343. Chicago Linguistic Society. Chicago (Ill.): University of Chicago.

- Lasnik, H. and K. Funakoshi. 2011. "On the Single Copy Condition." Ms. University of Maryland.
- Lasnik, H. and J. Uriagereka. 1988. *A Course in GB Syntax: Lectures on Binding and Empty Categories*. Cambridge, MA: MIT Press.
- Lasnik, H. and J. Uriagereka. 2005. *A Course in Minimalist Syntax*. London: Blackwell.
- Lasnik, H. and J. Uriagereka. forthcoming. *Structure*. Cambridge (MA): MIT Press.
- Lebeaux, D. 1990. "Relative clauses, licensing, and the nature of the derivation." In J. Carter, R. M. Dechaine, B. Philip, and T. Sherer (eds.), *Proceedings of the North East Linguistic Society* 20, 318-332. UMass, Amherst.
- Lebeaux, D. 2009. *Where does the binding theory apply?* Cambridge, MA: MIT Press.
- Linebarger, M. 1980. The grammar of negative polarity. Ph.D. dissertation, MIT.
- Manzini M.R. and A. Roussou. 2000. "A minimalist theory of A-movement and control". *Lingua* 110: 409-447.