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Author's address: L2C2 CNRS-UMR5230, Institute for Cognitive Sciences, 67 bd Pinel, 69675 Bron cedex, France.
reboul@isc.cnrs.fr

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Samuel David Epstein & T. Daniel Seely, *Derivations in Minimalism* (Cambridge Studies in Linguistics 111). Cambridge: Cambridge University Press, 2006. Pp. xvi + 217.

Reviewed by M. RITA MANZINI, Università di Firenze

There is much to be admired in this book and much to be discussed, and as it happens with interesting work, the two things often go hand in hand.

Derivations in Minimalism is organized into five chapters. Following chapter 1, 'Orientation and goals', which outlines the book's aims, scope and organization, the book proper opens with chapter 2, entitled 'On the elimination of A-chains'. This chapter is entirely devoted to a clarification of the notion of chain – for, as Samuel David Epstein & T. Daniel Seely point out at the outset, 'unclarity is an empirical issue, and empirical issues are extremely (but not uniquely) important' (4). Salient conclusions reached in this chapter are that 'chains are not Syntactic Objects'; that 'chains "contain" an invisible X′ [i.e. the X′ necessary for the definition of the derived specifier position SpecXP], which renders them unspecifiable to … LF'; and that 'the information encoded in chains is in fact already contained in the syntactic operations Merge and Move' (29). In other words, the notion of chain is both ill-defined and redundant. Suppose these results are correct – as I take them to be. While they are certainly important results for Minimalist theorizing, and while Epstein & Seely are very explicit about the limits of their discussion, one would wish that the authors applied their knowledge and insight to an investigation that takes us beyond purely theory-internal questions.

The empirical observation underlying the notion of A-movement/chain is the non-isomorphism of theta-interpretation with respect to other grammatical properties (Case, agreement, etc.). This is just a fact, which as such forms part of any grammatical description, as, for example, in the case of passive. Transformational grammar holds that this superficial non-isomorphism corresponds to an isomorphism at a sufficiently abstract level of structure (D-structure or first Merge) and that the relation between underlying and surface representations is best captured by movement. For Epstein & Seely, '"representational" theories are in fact, just one kind of derivational theory' (45) – a statement which mainly refers to the approach espoused by Brody (1995).

But consider passive. While in English it has the familiar *be* + past participle structure, in other languages it may be formed with a clitic that is homophonous with the reflexive, as in the well-known case of Romance (cf. Italian *si*) and, less well known, Albanian (where the clitic is *u*). The problem is that the syntax of these passive constructions containing a clitic is transitive, with *si* and the like behaving in more than one respect identical to (accusative) object clitics. A theory in which the LF (and hence interpretive) notion of chain was a primitive would have no special problem in constructing the bound element of the chain as either a copy/trace or as a lexical variable, which in the analysis of Manzini & Savoia (2007) corresponds to the clitic. By contrast, derivational theories of *si*-passives (of which many are found in the literature) end up getting the properties of the *si*-clitic wrong, treating it, for example, as part of the verb's inflection or as a subject clitic.

Of course, there is a standard answer to the family of problems that I have in mind, aptly characterized by Culicover & Jackendoff (2005) as Interface Uniformity. Interface Uniformity assumes that it is of paramount importance that 'the syntax–semantics interface is maximally simple, in that meaning maps transparently into syntactic structure; and it is maximally uniform, so that the same meaning always maps onto the same syntactic structure' (Culicover & Jackendoff 2005: 6). From such a perspective, capturing the object clitic status of *si* may be considered of small import when weighed against the possibility of maintaining a 'uniform' movement analysis for passives. But evidently neither Culicover & Jackendoff (2005) nor Manzini & Savoia (2007), in their very different and in fact divergent ways, agree with this perspective.

By far the longest part of Epstein & Seely's book is devoted to discussion of another important topic, namely the elimination of the Extended Projection Principle (EPP). Epstein & Seely begin by noting that the EPP has been deemed both 'mysterious' and 'redundant with other principles of Universal Grammar' (51). This is an accurate observation, urging serious consideration of the question whether such a principle is real or, at least, what its appropriate characterization may be. On a personal note, it is true that 'Manzini & Roussou (2000) propose analyses inconsistent with the EPP' (53). However, as it turns out, Roussou and I have independently revised our conceptions, making the analysis of control compatible with the EPP (see Manzini, to appear), for reasons that are directly relevant to raising – the phenomenon which chapters 3 ('On the elimination of the EPP') and 4 ('More challenges to the elimination of the EPP: Some movement cases') of Epstein & Seely's book address. Here, the central question is whether raising takes place in one step from the embedded thematic position of first Merge to the matrix subject position, or whether there is stepwise movement through the embedded subject position, enforced by the EPP. Another construction that Epstein &

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Seely consider in this context is Exceptional Case Marking (ECM), for which they conclude that 'the ECM subject does not occupy the Spec of *to* at Spell-out' and that 'the ECM DP has overtly "object raised/shifted upstairs"' (69). Crucially, the authors, 'unlike others, would claim in addition that nothing even moves through Spec of *to*' (69).

Epstein & Seely put forth various arguments as to why raising does not proceed stepwise, and hence, why the EPP can be eliminated. Their central argument is that 'if there is no successive cyclic A-movement, then there can be no "partial A-movement"' (56); in practice, they exclude movement of the raised element to the specifier position of the embedded Inflection Phrase (SpecIP), as in **There seems a man to be outside*. The problem is that in the Balkan languages, which lack infinitives, raising is possible from an embedded inflected clause. What interests me is not so much how raising can occur at all in these languages (an obvious problem in itself), but the fact that, despite the presence of an agreeing verb in the embedded clause, the embedded SpecIP position must remain empty; the only possible embedded position for a nominative argument is the thematic (post-verbal) position. In other words, 'partial raising' is impossible even in languages which have an overt checking position into which the subject noun phrase can move. Of course, one may take this fact and the ungrammaticality of a subject in the infinitival SpecIP position in English to be merely coincidental, or one may conclude that the non-existence of the EPP cannot be the explanation for the cross-linguistic lack of 'partial raising'.

The proposal that there is no EPP (and hence that there is no stepwise raising) faces problems in light of the various types of evidence generally adduced in favour of a stepwise movement account. This evidence is discussed in chapter 4, 'More challenges to the elimination of the EPP: Some movement cases'. Here Epstein & Seely make specific reference to the work of Bošković (2002), who like Epstein & Seely abandons the EPP, but unlike them requires movement to proceed in a stepwise fashion on grounds of locality. Consider Condition A of the Binding Theory. The question is what prevents a noun phrase other than the subject from binding into an embedded raising sentence, as in *Bill appears to Mary to seem to himself/*herself to be ill*. The classical answer is that the embedded subject trace acts as an intervener for purposes of locality. However, if there is no EPP and no stepwise raising, this answer becomes unavailable. Epstein & Seely propose an alternative account based on Torrego's (2002) analysis, according to which the complement of the preposition *to* (abstractly) moves to a functional projection P, associated with 'point of view', which is reserved for experiencers and located above the raised subject. Thus, they exclude the ungrammatical example above by assuming that

> before experiencer movement *Mary* fails to c-command *herself* and so is not a licit binder at an 'early' point in the derivation; after experiencer

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> raising to Spec, P (i.e. the 'point of view' position), although *Mary* now c-commands *herself*, *Bill*, having raised to spec of matrix T, functions as an intervening specified subject. (143)

However, there is a problem with assuming abstract movement of the experiencer, given Epstein & Seely's later argumentation (on which more below), which would require the covert movement of the experiencer to result in some form of long-distance agreement.

Epstein & Seely also consider the fact that a floating quantifier can appear immediately before the embedded infinitival marker *to* in raising contexts, which under Sportiche's (1988) stranding analysis of Q-float must mean that at some stage of the derivation, the subject occupies the specifier of the phrase headed by *to*, and hence that raising is stepwise. Not surprisingly, Epstein & Seely revert to what is in fact a more traditional theory of Q-float, according to which the floating quantifier is treated as an adverb of quantification – an analysis that they attribute to Williams (1982, 1989, 1994) and that to my mind is in fact much more convincing. They acknowledge that Lebeaux's (1991) example **Her mother's bread seems to her to be known by every man to be the best* is problematic for this account, but point out (correctly, in my view) that Lebeaux's notion that *her mother's bread* is inserted 'late' in the derivation cannot be easily translated into their theory, nor into derivational Minimalist theories in general, such as Bošković's approach (2002), which they consider in some detail.

Chapter 5, 'Exploring architecture', relates to the discussion of the EPP (as we shall see shortly) but also addresses a much more general, architectural issue, as its title aptly indicates. The question is how a derivational model, such as the one Epstein & Seely are seeking to advance, connects to interpretation, both at the LF and at the PF interface. The proposal is that

> LF does not wait until the 'end of the line' to interpret [syntactic] … objects. Nor is *interpretation* postponed until the phases vP and CP are built … after each transformation applies (be it Merge or Remerge), the derived representation is fed into both PF and LF. (178)

An important consequence of this view is that Epstein & Seely abandon what they dub a Government-and-Binding-type characterization of well-formedness, according to which 'a derivation is well-formed only if at every point in the derivation no principle is violated' (179). Government-and-Binding-type well-formedness is not tenable because 'if LF and PF access each derivational point … no well-formed derivations of well-formed sentences would ever be generable' (179). This means that 'if α is non-convergent, it does not follow that β containing α … will also crash' (182); in other words, each syntactic object generated at each derivational point has its own PF and LF properties.

Any attempt at simplifying current phase-based conceptions of interpretation is surely welcome. I for one have the same question as Epstein &

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Seely: 'Is the naming of *v*P and CP as phases purely descriptive and ad hoc?' (61). Yet, presumably because of the very compact nature of chapter 5, various aspects of Epstein & Seely's proposal remain unclear to me. For instance, would we not end up with an enormous series of PF and LF evaluations that are entirely irrelevant to the output, and are we therefore not placing an unnecessary burden on the computational system? More importantly, does the system contain the notion of 'closure' for certain evaluation procedures, so that we will not keep evaluating, say, the theta-criterion with respect to an embedded predicate once the derivation of the embedded predicate is 'closed'? If the answer to this question is positive, it seems to me that the essence of the notion of phase is surreptitiously reintroduced. If the answer is negative, then what counts is the final syntactic object, that is, we are back to a representational model. One may then retort to Epstein & Seely that derivational models are just a kind of interpretive model.

What Epstein & Seely are dealing with here is the fact that in their model, there can be no covert movement. By covert movement, they mean 'movement the output of which is not fed to PF' (184). Under strong derivationality, 'this simply can't happen' (184). This becomes relevant for their analysis of what must count as one of the hardest problems for EPP-less theories, namely the presence of expletive subjects in existential (and unaccusative) constructions. Epstein & Seely propose that the English expletive *there* is inserted for Case-theoretic reasons (i.e. Case-checking), while the post-verbal argument is assigned partitive Case, as originally proposed by Belletti (1988). This leaves checking of the agreement features to be accounted for. As far I understand, Epstein & Seely resolve this problem by assuming long-distance agreement of exactly the same kind contemplated by the current Chomskyan model.

Of course the two analyses differ in one major point. For Chomsky, not only are all features checked by long-distance agreement between the correlate and I, but the Case on the correlate, which he takes to be nominative, is a reflex of agreement. This analysis is unavailable to Epstein & Seely for the reasons already mentioned: in the absence of the EPP, they need Case to enforce insertion of the expletive, so that the correlate ends up with partitive Case; Case and agreement are then checked separately. I believe that there are empirical reasons why, *contra* Chomsky, nominative Case cannot be reduced to agreement – for instance, ECM-like constructions in Balkan languages, where the accusative argument agrees with an embedded finite verb (despite Iatridou's 1993 attempts at circumventing the problem). Yet, to postulate partitive Case for the correlate seems to be just another example of the Interface Uniformity bias, which I have already questioned above. Epstein & Seely argue for the superiority of their analysis over Chomsky's by claiming that under partitive Case-checking, 'the definiteness effect in existentials … and more generally … for unaccusatives follows' (192). Rather, what they should say is that partitive Case provides a syntactic annotation for the observed interpretive constraint, because it is not clear that there is any independent morpholexical (PF interface) evidence for partitive Case.

Given that current hypotheses (independent of the hypotheses being debated in this book) locate the source of linguistic variation in the lexicon (more precisely, in the morphology), considerations relating to the morpholexical (PF) interface go hand in hand with considerations pertaining to parameterization. To restate my query concerning the partitive in this mode, what exactly do we predict about partitive case cross-linguistically? As far as I understand, the expectation is that any language or construction that has an obligatory expletive and an agreeing correlate will have partitive Case as well as the definiteness effect for the correlate. But what about German, a non-null subject language where the correlate agrees with the verb, but which has a morphologically nominative postverbal subject and no clear definiteness effect (cf. *Es kam der Frühling* 'Spring came', literally 'It came the spring')? Though the correlation between an overt nominative and lack of definiteness would seem to lend support to the partitive theory, the problem is that this is an example of obligatory expletive insertion despite the fact that the correlate is able to check nominative Case, which is unexplained on Epstein & Seely's account.

Since a number of my remarks involve comparison between the English examples considered by Epstein & Seely and examples from other (fairly closely related) languages, it is perhaps appropriate that I conclude by mentioning the disclaimer that Epstein & Seely make at the very beginning, where they state: 'our primary goal … is … not empirical coverage by (re-)description or stipulation' (1). It should have become obvious that I hold that cross-linguistic comparison is not a mere matter of empirical coverage but that, on the contrary, any linguistic theory is (also) a theory of variation. On this, as on other matters, I will certainly be looking forward to further installments of work by these authors.

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Author's address: *Dipartimento di Linguistica, Università di Firenze, via Alfani 31,*
50121 Firenze, Italy.
rmanzini@unifi.it

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Yehuda N. Falk, *Subjects and Universal Grammar: An explanatory theory* (Cambridge Studies in Linguistics **113**). Cambridge: Cambridge University Press, 2006. Pp. xix + 237.

Reviewed by ANNA SIEWIERSKA, Lancaster University

In this monograph, Yehuda N. Falk presents a view of subjecthood which seeks to reconcile the major insights stemming from both the functional-typological and formal approaches to subjects developed within the last thirty years or so. Working within the framework of Lexical Functional Grammar (LFG), he takes grammatical functions (GFs), of which the subject is a major manifestation, to be a universally relevant level of syntactic representation, parallel to the levels of constituent structure and argument structure. The subject GF thus emerges as relevant for all languages (165). The subject GF, however, does not correspond to the traditional notion of subject. The latter, according to Falk, reflects the intersection of the subject GF with another type of grammatical function, an 'overlay' or secondary function, called the syntactic pivot (PIV). The PIV, unlike the subject GF, is not universal.

The subject GF itself is characterised as the element with the function of expressing as a core argument the hierarchically most prominent argument on the argument structure hierarchy (39), which in LFG is called θ. In the unmarked case, θ corresponds to the highest-ranking argument on the thematic hierarchy, which will generally be the agent, provided there is one, otherwise the patient/theme, etc. However, since the level of argument structure in LFG is a syntactic rather than a semantic level and thus contains both thematic elements and non-thematic ones (e.g. expletives and idiom chunks), θ may also be non-thematic, as *there* is in *There is a book on the table*. Further, if θ is displaced from core argumenthood, as, for example, in