Roger Martin, David Michaels & Juan Uriagereka (eds.)
Step by step: Essays on minimalist syntax in honor of Howard Lasnik. Cambridge, Massachusetts and London, England: The MIT Press, 2000. Pp. xxiv+357.

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This book contains 10 essays written in honor of Howard Lasnik, as well as an introduction by two of the editors, a list of Lasnik's publications, a list of dissertations supervised by Lasnik, and an index. The dust jacket describes the contents as 'an up-to-date overview of research in the minimalist program of linguistic theory'. As such, this review considers the book as a window onto current minimalist research.[1]

The quality of the writing in this book varies enormously, from Chomsky's self-described 'first part of an unfinished manuscript' (89) which reads as (brilliant) stream-of-consciousness, through Willim's thorough but dense and hard to follow overview of kinds of evidence for DP, to some very clear papers, such as those by Bošković, Demirdache & Uribe-Etxebarria, and Takahashi. The introduction by Martin & Uriagereka is also generally clear, but not obviously about linguistics. In the course of trying to prove the scientific legitimacy of the Minimalist Program, Martin & Uriagereka make reference to physics (including magnetism, thermodynamics and fluid dynamics), chemistry, and biology (including anatomy and evolution). No reference is made, however, to psychology, psycholinguistics, or neurolinguistics. In general, the presentation would have been made clearer if the papers had more structure (sections and subsections) and authors provided more tree diagrams. Finally, using footnotes instead of endnotes would have enhanced readability.

In his chapter, Chomsky lays out what he calls the 'strongest minimalist thesis':

(1) Language is an optimal solution to legibility conditions. (96)

Chomsky presents this thesis as something that would be surprising, and therefore interesting, if true. And indeed, it does seem highly implausible. (For an extended discussion of its implausibility, see Johnson & Lappin 1999.) However, science often proceeds by researchers pushing implausible ideas to their limits, either proving them true or learning more about the domain of inquiry in the process of proving them false. We are a long way from seeing the minimalist thesis proven true, but in the meantime, we can ask whether we are finding out interesting things about the domain of inquiry. In this light, let us examine the questions that are asked in the other chapters of this book.

Barss (Chapter 1) investigates which interpretation mechanisms are needed for wh in situ: LF movement to scope position, existential choice functions, or both? Bošković (Chapter 2) asks how we can account for the distribution of wh movement across question types and across languages, and what theoretical implications the distribution has. This question leads to the observation of an interesting correlation: phonologically null complementizers correlate with required wh movement in embedded contexts but no wh movement in matrix contexts. In Chapter 4, Demirdache & Uribe-Etxebarria ask how X′ theory can provide a compositional representation of tense/aspect systems. Johnson (Chapter 5) investigates which kind of overt movement quantifier scope is most similar to: scrambling or extraposition from NP. Murasugi (Chapter 6) asks whether Japanese has relative clauses, and if not, why not?

Ormazabal (Chapter 7) looks into whether movement out of VP can be motivated by something other than Case. More specifically, he explores an account for gaps in the paradigm of ditransitives

[1] I would like to thank Jeff Good, Jeffrey Parrott, Susanne Riehemann, Ivan Sag, and Tom Wasow for helpful discussion in the preparation of this review. They do not necessarily agree with the opinions expressed here.

in Romance and Basque, without appeal to Case. Saito & Hoshi (Chapter 8) explore how to account for the properties of Japanese verbal nouns in minimalist terms, and what they tell us about the nature of the faculty of language. In Chapter 9, Takahashi asks whether, under Move F, empty subjects get pied-piped when their features get attracted. This leads to interesting observations about control of open adjuncts in various constructions and about kinds of arguments in Japanese. Finally, Willim (Chapter 10) explores whether there is any evidence in Polish for a DP projection.

How interesting one finds these questions probably depends on how one defines the domain of inquiry. The Minimalist Program defines the domain of inquiry as the nature of the faculty of language (FL), and patterns within and across languages are seen as a window onto FL. Outside the MP, there are linguists who share the goal of eventually understanding the human capacity for language but take the view that we do not yet know enough about the (neuro-)psychological mechanisms involved to ask such questions directly. For such linguists, the current domain of inquiry is the patterns within and across languages. The results of this work will (hopefully) form the foundation of inquiry into the nature of the cognitive systems behind language acquisition and use.

Unsurprisingly, the questions asked in this book are more likely to be interesting to linguists working within the MP, although linguists who take the second view described above might find the papers by Bošković and Takahashi interesting for the patterns they observe. (In the case of Bošković's paper, however, the correlation only obtains with some fairly substantial abstraction. In particular, movement of some wh words in Serbo-Croatian is interpreted as PF movement, and subject-auxiliary inversion in English is counted as a phonological realization of a complementizer.)

Regardless of what one thinks of the questions asked, the papers in this book suggest that work in the Minimalist Program is generally hampered by the fact that the program is anchored neither in a systematic body of verifiable empirical observations nor in a worked-out formal theory. As a result, the authors end up reasoning about reified metaphors and/or producing or trying to argue against unfalsifiable claims.

The lack of verifiable empirical observations is the result of the data collection methodology. In these papers, the data appears to be collected in an off-hand, unsystematic way, with unconfirmed questionable judgments often used at crucial points in the argumentation.

For example, the punch-line of Takahashi's paper is the lack of ambiguity in examples involving open adjuncts controlled by quasi-argument subjects. Such judgments appear quite subtle, and the paper (already one of the best in the book) would be much stronger if such conclusions were supported by evidence of systematic data collection (e.g., survey results, or at least some indication of how many speakers the author consulted).

Barss's paper is similarly concerned with extremely subtle judgments, for example, whether or not each of (2a-b) have both of the readings in (3a-b):

(2) (a) [Which professor]$_2$ did everyone tell e_2 to grade [which student]$_1$ strictly?
    (b) [Which student]$_1$ did everyone tell [which professor]$_2$ to grade e_1 strictly?

(3) (a) which pairs $\langle x, y\rangle$, x a student & y a professor, are such that for every person z: z told y to grade x strictly
    (b) for every person z: which pairs $\langle x, y\rangle$, x a student & y a professor, are such that z told y to grade x strictly

In this case, the author reports on only his own judgments. There is really no data to account for until such judgments have been replicated by other speakers not working on the problem and not alerted to what the author is researching.

Johnson provides several instances of questionable data collection, remarking with respect to some German examples, 'With some grilling, however, I have managed to squeeze from speakers approval of examples such as (38)' (202). In addition, on page 205, he bases some arguments on the readings available for the sentence in (4), which he rates as ??.

(4) ??It's Saturn$_1$ that I dissected [an alleged insect from t_1].

He is reporting his own intuitions here, but it is not clear how he could simultaneously have the intuition that the sentence is just about unacceptable and have clear intuitions about which scopal ambiguities it has.

This kind of collection and presentation of the data is presumably common only because readers (and reviewers) don't demand better. The assumption that readers will take the data as given and focus on the argument is also apparent in Chomsky's contribution. On page 103, he cites a patently ungrammatical string of English words (*there read-pl the book (never) any students*) as grammatical. Only in endnote 36 does the reader learn that this example is actually an English gloss of an Icelandic sentence.

Such cavalier treatment of the data is an embarrassment to the field and should be embarrassing to the authors. Why should other sciences pay attention to the 'results' of linguistics (as Marin & Uriagereka (27) hope) when we handle our data so irresponsibly?

Such examples are hardly restricted to minimalist work, and indeed predate the MP (see Schütze 1996 and references cited there for discussion, and Cowart 1997 for a suggestion on how to do better). However, the MP does encourage lack of concern for the data, above and beyond what is unfortunately already the norm in formal syntax, because the connection between analysis and data is allowed to be remote. For example, the papers in this book often gloss over such details as the word order of the sentences being analyzed, even when the argumentation concerns movement various elements of those sentences.

Ormazabal, arguing for replacing Case-driven movement with movement driven by a feature [+animate], has nothing to say about the word order differences in these two Spanish examples that he cites:

(5) (a) Te la llevamos
CL.2DAT CL.3ACC brought.1PL.NOM
'We brought her to you.' (247)

(b) Te llevamos (a) ésta/ésa
CL2.DAT brought.1PL.NOM (to) this/that.FEM
'We brought this/that one to you.' (249)

Of third person clitics (such as *la*) and demonstrative pronouns (such as *ésta/ésa*), Ormazabal writes:

> [They] are not [+animate] and consequently cannot check the relevant feature with T; they must therefore remain in their base-generated positions unless, of course, they are attracted to check some other features (e.g., a *wh*-feature). (250)

Since *la* and *ésta/ésa* appear in different positions in (5), either they have different base-generated positions or one but not the other is moving to check some other feature. Without any discussion of what's behind this difference, it is difficult to evaluate Ormazabal's claim quoted above.

More generally, the analyses in this book are often incomplete or underformalized. For example, one aspect of Bošković's analysis relies on a particular version of the Head Movement Constraint which hasn't been integrated into the general system that Bošković assumes (that of Chomsky 1995). Murasugi concludes her study of whether Japanese has relative clauses with the statement:

> [T]he most distinctive property of Japanese then is that pure sentential modifiers can modify the nominal heads in ways that are not allowed in other languages. (231)

She doesn't provide any indication of how the structures allowed in Japanese would be ruled out in another language. Since the MP assumes general crosslinguistic similarity, any statement of this kind must be backed up with a formal way of making the distinction for the analysis to be complete. Finally, Demirdache & Uribe-Etxebarria (163ff) assume that arguments of one abstract head (T or Asp) appear in the specifier position of the complement of that head, with no explanation of how this fits in with general assumptions.

In another kind of incomplete analysis, the authors assume something to be true by fiat, without making sure the formal system actually enforces the conditions assumed. For example, Bošković assumes that there always has to be a complementizer in any wh question, but doesn't specify what would happen otherwise. Demirdache & Uribe-Etxebarria propose a 'Constraint on Aspect Recursion' (173), but don't discuss how such a constraint could be implemented. Saito & Hoshi argue that 'The only requirement on θ-roles ... is that they be properly assigned at LF' (268). They provide no discussion of how this requirement is enforced. Saito & Hoshi also don't discuss what formal status θ roles have in the MP. Are they a species of feature? Is θ role assignment some kind of feature checking? How is the information contained in a θ role—which semantic argument a syntactic argument is associated with—preserved and interpreted?

In general, much of the theoretical discussion in this book appears to take place at a metaphorical level. Metaphors are certainly useful as heuristics in generating new hypotheses to explore. However, in order for a community of researchers to collaborate in building a large theory, the metaphors need to be grounded in some descriptive system whose properties are more readily agreed upon and less open to interpretation. This is particularly important in enterprises like the MP, where the nature of computational system is the main focus of inquiry. When most of the technical proposals are stated in terms of metaphors, researchers don't necessarily interpret the properties of the operations, constraints, and formal entities proposed in the same way. Without an agreed-upon set of properties, the discourse about the proposals becomes disjointed, and researchers seeking to construct an argument about some aspect of FL have little to base their argument on but the properties of the objects the theoretical constructs are metaphorically related to.

Saito & Hoshi provide a clear example of this, in the following argumentation about chains:

> Let us assume that a chain consists LITERALLY of the head and its copies. It is then possible that an operation on the head also automatically affects the other members of the chain, which are by definition copies of the head. On the other hand, an operation on a copy is illegitimate, since it does not affect the "original." (284, emphasis added)

Here, they are making an argument for one interpretation of chains based on properties of 'copies'. These properties of copies are not agreed-upon properties of the formal entities, but rather properties of things that go by the same name in other (non-linguistic) domains. The use of the word 'literally' signifies a reified metaphor: The word 'copy' was chosen for this theoretical construct based on some similarity between the theoretical 'copies' and other kinds of copies (say, photocopies). However, nothing in that naming should license the inference that theoretical 'copies' have any particular properties of photocopies.

Note that Chomsky in fact directly contradicts Saito & Hoshi's interpretation of the metaphor: Regarding the use of the term 'copy' for non-head elements of a chain, he writes: 'The terminology is misleading ... each of the elements is a "copy" of the other.' (114)

The hypothesis Saito & Hoshi are putting forward about chains is a perfectly legitimate one. It is not, however, supported by the reasoning about properties of other kinds of copies. Progress toward the goals of the MP might be faster if technical proposals were accompanied by more precise descriptions of the formal properties involved. Ideally, this would make it easier for researchers to apply and/or argue against each other's proposals.

A common style of argumentation in these papers is to make a proposal at the outset (e.g., insertion of complementizers at LF (Bošković) or LF incorporation of Japanese verbal nouns into light verbs (Saito & Hoshi)), build an analysis incorporating that proposal, then conclude that since the analysis is successful in accounting for the data, the original proposal is supported. In the abstract, there's nothing wrong with this style of argumentation, especially since the authors don't claim to have proven anything. In the details, however, it turns out to be quite unsatisfying.

For one thing, such argumentation is convincing to the extent that there is substantial, accepted data for the analyses to account for. Given the dubious treatment of data in our field and in MP work in particular (see above), the empirical base required for this style of argumentation is rickety at best and most likely illusory. Furthermore, the range of data considered is usually extremely narrow, so that a few linguistic contrasts end up supporting a disproportionately large number of theoretical constructs and claims.

A second problem is that the analyses always include a host of other assumptions along with the proposal being argued for: In Saito & Hoshi's case, those assumptions include the stipulation that LF incorporation licenses accusative Case on the incorporated noun, the assumptions that LF incorporation is driven by the requirement of the noun to assign its θ roles, and the assumption that nouns need not discharge their external θ role. In Bošković's case, the ancillary assumptions include the idea that some phonologically 'overt' complementizers take the form of 'phonological affixes', the assumption that Merge is exempt from Procrastinate (or that Procrastinate is eliminated from the formal system altogether), and the assumption that some operation other than wh movement can move wh elements to be beginning of a sentence, at least phonologically.

The success of the analysis only lends support to the combination of assumptions used in the analysis. This decreases the apparent confirmation of any one of those assumptions: With so many variables in play, it seems extremely likely that another analysis that differs in one or more assumptions would be equally successful. And there is no apparent difference between the assumptions foregrounded as the 'proposal' and the other ancillary assumptions, other than theoretical interest.

A third problem is that the analyses offered (whose success is meant to support the original premise) are often incomplete (see above). In effect, this means that no account of the facts incorporating the theoretical proposal has been provided, for an incomplete analysis can't be said to account for anything.

Thus the arguments from the success of the analysis end up far from convincing. However, since Minimalist work is proceeding with neither a strong connection to data nor a well worked-out formal theory, arguments from the success of the analysis are often all that is available. When authors try to construct convincing arguments for or against any particular theoretical position, they are often impeded by the abstractness of the program, which allows too much wiggle room. This, in turn, means that many minimalist claims are effectively unfalsifiable.

For example, on page 74, Bošković summarizes the intersecting factors in his analysis: apparently null complementizers might really be null or actually be phonological affixes (e.g., things that trigger subject-auxiliary inversion), and fronting of wh words may be wh movement or some other operation. Add in the possibility of covert movement, and it's hard to see how how Bošković can be certain of the correlation he reports: that phonologically null complementizers correlate with required wh movement in embedded contexts and lack of wh movement in matrix contexts.

Willim's paper is a remarkably thorough search for possible evidence for a DP projection in Polish. However, the effort is significantly hampered by the wiggle room provided by the theory. She states that even if D has no phonological content in a language, its presence could be known if it were syntactically active. Finding no evidence of overt N-to-D movement, she concludes that there is no 'evidence for a phonologically unrealized but syntactically active D head in the language' (330). In a less abstract framework, this would be reasonably convincing. Given usual minimalist assumptions, however, one is left wondering if there is any evidence of COVERT N-to-D movement. Since such evidence is necessarily indirect, it is extremely difficult to anticipate and argue against.

Later on, Willim argues that there is no evidence for a syntactically active [Spec, DP] in Polish either, on the basis of the lack of noun phrases with two genitive arguments (332). Her explanation for this fact is that the absence of the DP layer leaves the second genitive argument no place to go to be 'licensed and identified' (333). However, Willim has already argued that Polish nominals involve multiple KP projections, and it is not clear why those layers wouldn't serve the purpose as well as a DP layer. With this loophole still open, Willim has no real account of the facts she's interested in, and therefore it can't serve as evidence against DP in Polish.

In summary, in order for the MP to achieve its goals, a balance needs to be found between the important work of making new proposals about the nature of FL and the equally vital work of fleshing out those proposals in terms of defining the formal system and connecting theoretical ideas to a strong empirical base. In this book, there is too much emphasis on the former, and its success is impeded by the lack of attention paid to the latter.

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