Remarks on *Recursive Misrepresentations* by Legate et al. (2013)

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July 2013

1. Introduction

Even though the term biolinguistics has been coined decades ago much of the work done in the Chomskyan framework has remained focused exclusively on non-biological linguistics. And little, if any, work in the biological sciences has focused on the search for language specific genes or brain structures. In an attempt "to promote a stronger connection between biology and linguistics …[and] to clarify the biolinguistic perspective on language and its evolution" (Hauser, Chomsky, & Fitch, 2002, 1570), "bio" linguist Noam Chomsky, psychologist Marc Hauser, and biologist Tecumseh Fitch published a paper in *Science*. They proposed a conceptual distinction between two senses of the faculty of language, broad and narrow (FLB/FLN). Only FLN is specific to language and to the human species. It "is the abstract linguistic computational system alone, independent of the other systems with which it interacts and interfaces...The core property of FLN is recursion... it takes a finite set of elements and yields a potentially infinite array of discrete expressions" (Hauser, Chomsky, & Fitch, 2002, p. 1571). In a follow up paper they reiterated that the "terminological distinction between FLN and FLB is intended to help clarify misunderstandings and aid interdisciplinary rapprochement" (Fitch, Hauser, & Chomsky, 2005, 179), and expressed "hope that research into the biology and evolution of language will not continue to be mired down by the misunderstandings that have so long plagued this field" (Fitch, Hauser, &

Chomsky, 2005, 206).

Both papers received considerable attention but they did not eliminate or even reduce misunderstandings. The field remains so far from mutual agreement about the aim of linguistic research, that it has been proposed that researchers agree to disagree: "These then are simply different enterprises – Chomsky is concerned with the nature of recursive thought capacities, whereas linguistic typology and the non-generative linguists are concerned with what external language behavior indicates about the nature of cognition and its evolution" (Evans & Levinson, 2009, 477). Given the *status quo* that seemed a sensible proposal but it has been rejected by at least some Chomskyans, and their debating style has become increasingly acrimonious in recent years (e.g., Pesetsky, 2009; Freidin, 2009; Nevins et al., 2007/2009; Hornstein, 2012, 2013; Pesetsky, 2013a). In this paper I analyze the most recent contribution to the recursion dispute (Legate et al., 2013), and argue that it is difficult to maintain that these Chomskyan biolinguists merely misunderstand the other side.

2. Misrepresentation of what?

Recently Stephen Levinson published a short report with the unremarkable title *Recursion in pragmatics*. He set himself the goal "to clarify that there is *one* central sense of the term recursion—namely embedding … —that clearly is not exclusive to syntax, and that is exhibited in a much more fulsome way outside of sentential syntax" (Levinson, 2013, 149, emphasis added). Like Hauser, Chomsky, and Fitch before him,

Levinson failed to achieve his goal. His report elicited a very hostile response[1] with the

judgmental title *Recursive Misrepresentation*[2] (Legate et al., 2013).

Apparently, much of the hostility has been caused by the claim "that there is little

evidence that unlimited recursion, understood as center-embedding, is typical of natural

language syntax" (Levinson, 2013, 149). This claim has been taken as "presumption that

center-embedding can serve as a proxy for embedding in general (and that clausal

embedding can stand proxy for recursion in general)" (Legate et al. 2013, 1). This would

be a gross misunderstanding indeed but there is no reason to attribute such

misunderstanding to Levinson. He clearly states that he is discussing *one sense of the*

term recursion, and he justifies throughout his paper why he is interested specifically in

center embedding.

Why, then, would Legate et al. accuse Levinson of misrepresentation? Given that

Levinson uses Hauser, Chomsky, and Fitch (2002) as point of departure, one would

assume that he is accused of misrepresenting assertions made there. However, one of the

authors of Legate et al. remarked recently "Most of the linguistics was excised from

Hauser, Chomsky and Fitch, for example, at the insistence of the journal[3]" (Pesetsky,

2013a, slide 107). Levinson can hardly be accused of misrepresenting something he was

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[1] Remarkably, Legate et al (2013) was celebrated and compared to the "evisceration" of Everett (2005) by Nevins et al. (2007/2009) on a biolinguistic blog that is "partly a labor of hate" (Hornstein, 2012/2013).

[2] Levinson (2013) contains several inaccuracies and ambiguities. However, they are not subject of this article because they pale in comparison to the misrepresentations, mischaracterizations, and double standards that make the title of Legate et al. (2013) mostly self-referential.

[3] I am not aware that Pesetsky has provided any documentation supporting the veracity of this accusation. He also never complained in print that "most of the linguistics was excised" from Chomsky (2009a,b; 2012). These are publications aimed to introduce Chomsky's "cutting-edge theorizing" to the general public. They have been edited by individuals who are very supportive of Chomsky's work. Yet, one finds even less "linguistics" in them than in Hauser, Chomsky, & Fitch (2002).

unaware of (the excised linguistics of Hauser et al, 2002). Legate et al. claim specifically that Levinson's focus on center embeddings is unjustified because "center embedding enjoys no special pride of place in linguistic analysis" (Legate et al., 2013, 6). This is a very odd claim coming from Chomskyan biolinguists. Levinson discusses in detail the implications of center embeddings for placement of a language on the Chomsky hierarchy (pp. 149-151). Further, Chomsky's co-authors reported that non-human primates (cotton-top tamarins (*Saguinus oedipus*)) can be trained to learn a finite state grammar $((AB)^n)$, but not the corresponding phrase structure grammar $(A^n B^n)$. "The A^nB^n grammar produces center-embedded constructions that [are] less common in human language than other (e.g., right-branching) structures" (Fitch & Hauser, 2004, 378). Given that evidence from formal grammar theory and from research on non-human primates suggests that center embeddings play a special role in human language it seems justified that Levinson focuses on their role. Is not clear why Legate et al. deny that center embeddings are of special importance.

3. Lacking Evidence?

Legate et al. (2013) accuse Levinson repeatedly of making unsupported or insufficiently supported claims: "No evidence is cited for these assertions" (2), "no substantial corpora of Pirahã texts… are cited"(2), "later research on Warlpiri, uncited by L13" (2), "Strikingly, no evidence is offered" (9), the absence of any evidence" (9), "without proper evidence" (10), "this proposal is pure speculation: an intriguing start for a future

research program, but unsupported (for now) by evidence or argument" (12).

This collection of quotes suggests that Legate et al. consider it problematic if a researcher makes insufficiently supported proposals in print[4]. This attitude is, of course, what one would expect from ordinary scientists. Yet, according to Chomsky, biolinguistic science is no ordinary science. Instead it follows a "Galilean style" of theorizing, unimpeded by inconvenient data interference: "[Galileo] dismissed a lot of data; he was willing to say: "Look, if the data refute the theory, the data are probably wrong." And the data that he threw out were not minor" (Chomsky, 2002, 98). This call for massive data-dismissal has been repeated numerous times in Chomsky's publications, culminating in the argument from the Norman Conquest: "… if you want to study distinctive properties of language - what really makes it different from the digestive system ... you're going to abstract away from the Norman Conquest. But *that means abstracting away from the whole mass of data* that interests the linguist who wants to work on a particular language" (Chomsky, 2012, 84, emphasis added).

To my knowledge, Legate et al. never objected to the "Galilean style". And, according to Chomsky, this style is not idiosyncratic to his own work but was accepted by famous scientists (e.g., Copernicus, Newton, Einstein, Monod) and "is pretty much the way science often seems to work …You just see that some ideas simply look right, and then you sort of put aside the data that refute them" (Chomsky, 2009a, 36). Given Chomsky's assertion (never challenged by Legate et al.) that scientists "just see" that some ideas look right and "put aside" data that refute them, Levinson's approach would be perfectly

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[4] For the following discussion it is irrelevant whether Levinson provided insufficient evidence for any of his proposals. For argument's sake I assume the strongest possible case for Legate et al. (that Levinson is guilty as charged). I am not proposing that the strongest case actually holds.

justified. He proposed (P1): "Linguistic typologists are well aware of many languages that show little evidence of indefinite embedding" (Levinson, 2013, 151), and provided examples of languages which, in his opinion support P1.

Legate et al. object to several specific claims about these languages, and cite evidence that Levinson either did not discuss or interpreted differently. For example, they object to the claim that Warlpiri is a language lacking evidence of indefinite recursion because (among other things) "later research on Warlpiri, uncited by L13, offers clear examples of clausal embedding" (Legate et al., 2013, 2). Yet, given that Legate et al. never objected to the "Galilean Style" they would have to accept that Levinson is doing legitimate Chomskyan science: after proposing P1 he put aside (or left uncited) evidence that refutes P1. Suggesting otherwise indicates that Legate et al. apply different standards to the work of Chomsky and Levinson[5].

4. To use or not to use: Corpus statistics

The second serious criticism leveled against Levinson concerns his proposal that higher degree center-embeddings are extremely rare in spoken and written texts (P2). Levinson cites research showing that roughly 11% of embeddings were center-embeddings for European languages (Karlsson, 2007), and even below that for polysynthetic languages

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[5] It is not necessary here to discuss the other examples Legate et al. provide because the same conclusion applies to all of them. It is worth noting, however, that Legate et al. never consider the possibility that an interpretation of the data different from their own could be correct.

(Mithun, 1984). In all languages only a very small degree of recorded center embeddings were higher degree center-embeddings: "the psycholinguistic findings and the corpus findings converge: after degree 2 embedding, performance rapidly degrades to a point where degree 3 embeddings hardly occur" (Levinson, 2013, 154). Even though P2 appears well supported by the evidence provided Legate et al. object.

Their argument is interesting for two reasons. First, it reveals again the double standards evident throughout their article. Second, it fails to support the conclusion that "the significance that L13 attaches to the subjective rarity of multiple center embeddings is at best premature and *most likely misplaced*" (Legate et al., 2013, 8, emphasis added). I discuss both points in turn.

Legate et al. criticize Levinson's use of Karlsson (2007) data because "Corpus statistics … must always be evaluated against a baseline, before concluding that the relative rarity of a given phenomenon requires special explanation. Neither L13 nor Karlsson provides such an evaluation" (Legate et al., 2013, 7). This is a surprising criticism given Chomsky's well known disdain for *any* work on corpus linguistics: "If you want to get a grant, what you say is "I want to do corpus linguistics" - collect a huge mass of data and throw a computer at it, and maybe something will happen. That was given up in the hard sciences centuries ago" (Chomsky, 2012, 19). To my knowledge Legate et al. have never objected to Chomsky's claim that the collection of a huge mass of data has been given up in the hard sciences centuries ago. Yet, in an argument directed at an opponent they cite "Bader (2012) [who] estimated the frequencies of these [= degree 2 center embeddings, CB] structures in a very large German corpus of 92 million sentences" (Ibid.). Apparently, to support an argument *against* Levinson the collection of such a huge mass

of data is justified.

Putting aside the issue of double standards, Bader's "proper assessment of quantitative data concerning embedding" (Legate et al., 2013, 7) does not challenge Levinson's claim. He had claimed that degree 2 center embedding "occurs vanishingly rarely in spoken language syntax" (Levinson, 2013, 155). Bader's corpus analysis has shown that double center embedding "occurs at a frequency of just above 4.6 per million sentences; a figure which one might indeed be tempted to describe informally[6] as "vanishingly rare"" (Legate et al., 2013, 7).

The following discussion suggests that Legate et al. misunderstand Levinson's factual claim about the low frequency of double center embeddings as a claim about *unexpectedly* low frequency. At least this is the claim the analysis by Bader would refute: "Most important, though the frequency of double center-embedding in Bader's corpus is low, it is close to its expected frequency if the grammar and processing factors are independent (cf. Chomsky and Miller 1963a, b)" (Ibid.). Yet, Levinson had never expressed surprise about the low frequency of double center embeddings in spoken language syntax. Instead, he called surprising the fact that even though such embeddings are rare in sentential syntax they are not rare in discourse: "We are now in a position to appreciate some very surprising facts. There are embeddings in interactive discourse that have the same basic properties exhibited in sentential syntax, but that are distributed over two (or more speakers). But in this case there is no parallel limit on embedding—multiple embeddings seem in principle indefinite, certainly at least to degree 6"

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[6] It seems difficult to conceive of a term that would be more appropriate than 'vanishingly rare' for this frequency. Charitably, one might assume that Legate et al. imply that vanishingly implies surprising but it is not plausible that Levinson had such an implication in mind.

(Levinson, 2013, 154). Clearly, Levinson expresses surprise about the fact that embeddings that have the same basic properties as those in sentential syntax are not equally limited to level 2 in interactive discourse. One may challenge this claim but Bader's work would be irrelevant for such a challenge because Bader did not report findings on interactive discourse.

5. Special Treatment – Everett 1986

Next, an allegation by Legate at al. involving the Amazonian language Pirahã deserves attention. Levinson had briefly discussed evidence from the literature suggesting (P3) that Pirahã either lacks evidence for any recursive structures (Everett, 2005) or "that embedding is very limited, and at most seems capped at one level deep" (Levinson, 2013, 151). Legate et al. object to P3 because (among other things): "… an example of possible double embedding is cited in Everett's own grammatical sketch (Everett 1986, 260, ex. 226; though with complications noted by Nevins et al. 2007, 27 fn. 38)" (Legate et al., 2013, 2). The use of quotes from Everett (1986) is noteworthy because Everett has explained more than once why he now rejects his earlier account. Consider:

> I told Chomsky in 1984…that I could find no evidence for
> embedding in Pirahã other than the –sai nominalizer…. It turns
> out that –sai has functions that overlap with nominalization but
> that that is not the best analysis of it. Experiments by Mike Frank
> and others, and the new paper by Sakel and Stapert, show this
> clearly. With that gone there is just no evidence for recursion in
> Pirahã. As often happens in field research, a minor difference in
> the way this or that morpheme or construction is analyzed can

have profound effects on the grammar as a whole. One doesn't
see all of this at first. (Everett, 2009, 219-220)

The explanation given here appears plausible, and Everett is certainly not the first researcher who has changed his mind in light of new evidence. It has been claimed that it is rather rare that a first proposal does not have to be changed in light of new data: "… the fact that [something is] the first thing that comes to mind doesn't make it true…. It is not necessarily wrong, but most first guesses are. Take a look at the history of the advanced sciences. No matter how well established they are, they almost always turned out to be wrong" (Chomsky, 2012, 38).

In the current context two questions arise. First, why did Legate at al. never mention Everett (2009) and other publications that defend the new interpretation of Pirahã data in Everett (2005), and, relevantly, the conclusion drawn by Levinson (2013)? Given how harshly Legate at al. criticize Levinson for not citing work that challenges his view, one would have expected that they would lead by example, and cite meticulously all work that is relevant to their claims. Second, why are Legate et al. citing a claim that has been corrected subsequently by the researcher? The answer to this question appears straight forward: the claim from Everett (1986) is the *only* positive evidence[7] Legate et al. can provide to challenge P3 that "that embedding is very limited, and at most seems capped at one level deep" (Levinson, 2013, 151).

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[7] It is further remarkable that Legate et al. cite their only example of possible double embedding in Pirahã referring to a footnote in Nevins et al. (2007) and not from the published version of the same paper Nevins et al. (2009). They use this example to establish one more instance of Levinson's ignorance of the literature he cites. Given that it was reasonable to expect that Levinson would only cite from a published version one would expect that Legate et al. explain why, apparently, they thought Levinson should have also consulted the unpublished version. No explanation is offered. The fact that one author of Legate et al. (2013), David Pesetsky, is also author of Nevins et al. (2007/2009) is noteworthy.

Legate et al. further criticize Levinson for making an "utterly unverified" claim because he cites no substantial corpora of Pirahã texts or studies that report on attempts to elicit multiple levels of embeddings. Given that a researcher who is fluent in Pirahã has reported that this language has *no* embeddings, there is no compelling reason for attempting to elicit multiple levels of embedding. Legate et al. do not doubt Everett's findings based on independent data showing he is mistaken. Instead they are taking sides in a currently unresolved dispute and doubt what an expert who knows the language has reported, based on *a priori* beliefs about what he should have found[8]. Compare this to the repeated allegation that Levinson does not cite all evidence relevant to his claims and/or that he interprets the evidence he cites in a biased manner.

6. Open Questions

A particularly problematic aspect of Legate et al. (2013) is that they are guilty of the shortcomings they accuse Levinson of. Due to space considerations I discuss only one of several examples in detail. Levinson proposed (P4) "it remains an interesting question whether treating, say, English as regular (with large numbers of simple rules) is more complex than treating it as context-free (with fewer, more complex rules; see Perfors et al. 2010)" (Levinson, 2013, 154).

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[8] The attitude towards Everett (evident also in Nevins et al. 2007/2009) could be described as a form of intellectual colonialism: the field work of another researcher is exploited in support of one's own theories, and findings that do not confirm to these theories are "reinterpreted". The reinterpretation is considered to be justified because the fieldworker is allegedly intellectually inferior and does not know how to interpret his own work correctly.

Legate et al. attack P4 for two reasons. First they claim "even if a more principled application of statistics were to show that multiple center-embedding is unexpectedly rare in some corpus, such a finding would not … support [P4]" (Legate et al., 2013, 8). Given that P4 merely states that a certain question is interesting Legate at al. must feel entitled to determine which questions are interesting for everyone. They consider this imposition justified because "there is *now* broad consensus that a variety of syntactic models (including Tree Adjoining Grammar, Combinatorial Categorial [sic] Grammar, Minimalist Grammar and others) converge onto the "mildly context sensitive" class, which appears to have the appropriate descriptive power for natural language syntax (Joshi *1985*)" (Legate et al., 2013, 8, emphasis added).

It is remarkable that, in the face of obvious disagreement, they base their claim about *current* consensus on a single source that had been published before the term minimalist grammar had been coined. And even a very superficial survey of the literature reveals that the Levinson is not the only researcher challenging the alleged consensus. It has been proposed that it is possible to "sketch a model to account for human language behaviour without relying on hierarchical structure" (Frank et al[9]., 2012, 4).

Further, the list of potential grammars for natural languages provided by Legate et al. is far from complete. Some researchers reject the Chomskyan Universal Grammar model (e.g. Sampson, 2002; MacWhinney, 2004; Tomasello, 2008; Everett, 2012). Others have challenged the legitimacy of the current minimalist model (e.g., Seuren, 2004; Johnson & Lappin, 1997), or the internal coherence of bio-linguistics (Behme, 2011; Postal, 2009,

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[9] It cannot be assumed that Legate et al. were unaware of this work because it has been discussed by one of the authors (Pesetsky, 2013a, slide 82). Given that this work partly undermines the charge against Levinson it is remarkable that Legate et al. fail to mention it.

2012, Neef, 2013). Legate et al. are of course free to disagree with any of these (and other) researchers, and promote their own biolinguistic proposals. But, given the widely diverging views on the nature of natural language, it is grossly misleading to speak of a "broad consensus".

The next claim is even more problematic: "Furthermore, L13 seems to suggest that regular grammars may be in effect sufficient despite being less powerful than context free grammars, since center-embedding structures are very rare. But the paper (Perfors et al. 2010) that L13 cites in support of this claim shows no such result. … it is puzzling how L13 could interpret Perfors et al. (2010) as in favor of regular languages" (Legate et al., 2013, 8). Perfors et al. have indeed not reported that regular grammars "are sufficient because center embeddings are very rare". But Levinson has nowhere suggested that these researchers reported such findings. He has cited Perfors et al. only once in a slightly ambiguous sentence. He could mean either (i) that Perfors et al. treat language as context free, with fewer, more complex rules (than regular grammars) or (ii) that Perfors et al. have conducted research on the different types of grammars[10]. It *is* truly puzzling that Legate et al. attempt to impose a claim on Levinson that is not supported by anything he wrote.

7. Conclusions

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[10] I am indebted to Paul Postal for confirming that these are the two most natural readings of Levinson's sentence. Both readings would be consistent with what has been reported by Perfors et al. (2010). It seems unlikely that Levinson had the self-incriminating interpretation in mind that Legate at al. attribute to him.

In spite of numerous attempts to clarify what is at issue in the recursion debates misunderstandings continue to be the rule, not rare exceptions. Given that the issue is not incomprehensibly complicated, and that over a decade of debate has not resulted in measurable progress towards mutual understanding (or in some cases even respect for an opposing position), one has to suspect that not all researchers are genuinely interested in finding a mutually acceptable solution. Legate et al. claim that they have written their article to "express our concern at the pervasive misrepresentations of fact and faulty reasoning presented" (Legate, et al. 2013, 1). However, their own article raises even greater concerns. Legate et al. never *justify* why the biolinguistic position is the only legitimate view in the recursion debate. Instead, the article documents unscholarly dogmatism and an unwillingness to interact fairly with those who defend a different position in the recursion debates. Given that Pesetsky discussed in detail a passage from Hornstein (2013) but did not object to the evaluation that Legate et al. (2013) "is devastating. There's nothing left of the Levinson (2013) article" (Hornstein, 2013), one has to assume that "evisceration of an opponent" was the true goal of this paper. One can hardly expect any progress in the recursion debate before such disrespectful attitude is eliminated from bio-linguistics.

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