Biolinguistics: Fact, Fiction, and Forecast

Cedric Boeckx

1   Introduction

Biolinguistics, as I understand it, refers to a branch of the cognitive sciences that seeks to uncover the biological underpinnings of the human capacity to support language acquisition (the development of an I-language, where 'I-' is meant as 'internal', 'individual', and 'intensional', following Chomsky 1986). That language acquisition requires a (possibly complex and multi-faceted) biological foundation cannot be seriously put into doubt, and biolinguistics, in the wake of early works by Chomsky and Lenneberg, takes that fundamental facet of human biology as its subject matter.

In his 'discussion note', in which he reviews *The Biolinguistic Enterprise*, which I co-edited with Anna Maria Di Sciullo (Di Sciullo and Boeckx 2011), Jackendoff (2011) goes through a series of important issues conceding the field, and makes several points worth highlighting, but he also commits several errors worth pointing out. This is the object of the present piece. Specifically, my aim in the pages that follow is to tease apart the real issues ('fact'), from the rhetoric ('fiction') and from the different bets various researchers make concerning the future ('forecast').

Ray Jackendoff is eminently well placed to speak about biolinguistics, since he has made seminal contributions to the field. Indeed, he is among the most committed theoretical linguists I know when it comes to establishing interdisciplinary bridges (a necessary step towards a productive biolinguistics), and has been for many years before other biolinguists joined forces (witness Jackendoff 1983, 1987, 2007). Given his stature in the field, Jackendoff's opinion cannot be ignored. Inaccuracies, if any, should be corrected, lest beginning students of the field receive a distorted picture of the enterprise.

As the title of his paper indicates, Jackendoff contrasts two views of the language faculty. As he puts it in the abstract, his aim is to "compare the theoretical stance of biolinguistics" with a constraint-based Parallel Architecture of the sort he has been advocating for decades (see the pieces collected in Jackendoff 2010, and especially Jackendoff 1997, 2002, Culicover and Jackendoff 2005). As we will see shortly, however, the contrast between his approach and "biolinguistics" conflates 'biolinguistics', 'minimalism' and 'Chomsky's specific proposals within minimalism and biolinguistics', which are related, but nonetheless distinct targets.[1] This I

[1]   To be clear, 'biolinguistics', 'minimalism', and 'Chomsky' are related in the sense that Chomsky is at the heart of the cognitive revolution that provides the source for biolinguistics, and, among many other things, he is responsible for initiating and articulating the minimalist pro-

take to be one of the major problems of Jackendoff's paper. The other one concerns the fact that a careful reading of Jackendoff (2011) reveals points of convergence that are far more significant than the contrasts that appear to dominate the paper.

Characteristically, Jackendoff's article touches on a wide range of issues (lexical redundancy, recursion, phonology, semantics, thought in other species, language-vision interfaces, evolutionary scenarios, etc.), all of which, according to him, provide evidence for the superiority of his approach over alternatives that he variously attributes to Chomsky, the minimalist program, and biolinguistics. Jackendoff also touches on the nature of linguistic inquiry more generally, highlighting the continuity with the Chomskyan program broadly construed, viz. a research paradigm that focuses on linguistic competence, its development in the individual and its emergence in the species. Jackendoff usefully discusses points of agreement concerning these general issues (in passing offering cogent responses to UG-critics, such as Evans and Levinson (2009); see Jackendoff's footnote 2, p. 587. In this part of his paper, Jackendoff points out that "of course, there are good reasons to want to minimize UG": "a leaner UG gives us a better chance of succeeding" in figuring out how UG is implemented in the brain and evolved in the species. (Jackendoff talks a lot about UG being encoded "somehow on the genome", but I think we should keep an open mind about the options nativism has to offer: there are many sources for innate ideas, the genome being only one of them; see Cherniak (2005), Longa and Lorenzo (2012), Chomsky (2005), and Lewontin (1993, 2000) for relevant discussion).

In this context Jackendoff provides one of the best descriptions I've come across of current biolinguistics: a field "...where linguistics makes contact with biology, taking the study of language beyond just the description of languages and language universals". But even in this early part of the paper, Jackendoff stresses a few biases characteristic of his general position, such as (i) the idea that language evolution is intimately tied to communication (see already Pinker and Jackendoff 2005, Jackendoff and Pinker 2005): "this is a problem ...not unique to the evolution of language. It arises in trying to account for any communicative capacity in any organism;" (ii) the idea that "explaining the origins of the language faculty depends of course on what one thinks the human language faculty *is*" ; and (iii) the idea that "the evidence from biological development is so far removed from the details of language structure that I find it hard to give these sorts of evidence priority over far more clearly established facts of modern linguistic structure in deciding which theory of linguistic knowledge to pursue".

I will come back to each of these points in the course of this paper. For now, let me concentrate on Jackendoff's description of biolinguistics (section 2 of Jackendoff 2011), where we begin to move from facts to fiction. (In what follows, I will

---

gram; and minimalism certainly feeds on biolinguistic concerns, but biolinguistics itself is not a theoretical framework (on a par with, say, Government-and-Binding or minimalism). Unfortunately, Jackendoff is not the only one to err in this context. Consider, for example, the following passage from the Linguistic Society of America Special Interest Group [SIG] on Biolinguistics, founded in 2009, which "seeks to help the field of biolinguistics define itself by, as stated in the SIG description, "helping to identify what makes biolinguistics 'bio' (and 'linguistic'), initiate discussions on how it differs from previous models of generative grammar (and how it doesnt), debate whether generative grammar is actually a prerequisite [...] and so on." Asking how biolinguistics differs from previous models of generative grammar is, in my view, a category mistake.

back up my arguments by quoting extensively from Jackendoff's paper to help the reader identify what I am after, so I ask the reader's indulgence if at times, both a quote, and my paraphrase of it appear side by side.)

2   "Biolinguistics"

Jackendoff begins his section on "biolinguistics" with the following:

> In recognition of the goal of interfacing linguistic theory with biology, practitioners of the Minimalist Program have begun calling the enterprise "biolinguistics" (e.g., Jenkins 2000, Larson, Déprez, and Yamakido 2010, Di Sciullo and Boeckx 2011, and the online journal Biolinguistics [www.biolinguistics.eu])

He then goes on to say that "biolinguists have for the most part focused on three issues. The first is the genetic basis of the language faculty, including extensive discussion of the possible role of the FOXP2 gene". Here Jackendoff points out that "[t]his issue has been part of the literature for decades (even if much of the evidence is new) and is not specific to the biolinguists". "The second issue is what principles of evolutionary and developmental biology could have led to a language faculty", where Jackendoff adds that "here, it seems to me, the distance between the nature of linguistic structure and what is known about the genetic basis of biological development is so great that the best that can be attempted at the moment is informed speculation, and I will have nothing to say about this issue here." Finally, "The third issue is the "first-principles" question posed by Chomsky (1995): "How perfect is language?", where perfection is defined in terms of elegance, lack of redundancy, and computational efficiency. Of the three, this issue has been the most central to biolinguistic inquiry into the actual form of the language faculty."

With this passage, Jackendoff's critique begins: "in stressing the deep questions of genetics and optimal design, the biolinguists have bypassed an important biological and psychological issue". Specifically,

> Evolution probably did not come up with radically new kinds of neurons; and if special kinds of neurons had been discovered in the language areas of the brain, we would have heard about them. Moreover, although it is important to bear in mind how little we know rather little about how assemblies of neurons encode and compute information, we might still be inclined to guess that evolution did not come up with radically new kinds of mental computation, mental processes, and mental architecture in inventing a language faculty. (See Gallistel and King 2009 for extensive discussion of the conservatism of neural mechanisms.) So to the extent that a theory of language permits a graceful integration with a plausible picture of the structure and function of the rest of the mind/brain, it places fewer demands on the genome, and therefore it is a better theory. I would contend that this too should be considered a criterion on linguistic theory from a biolinguistic perspective. (p. 590)

The passages just quoted provides enough material to make a first series of comment. First, Jackendoff errs in saying that practitioners of the minimalist program

have begin calling the enterprise biolinguistics. As has been pointed out on numerous occasions (including in the introduction of Di Sciullo and Boeckx (2011), which Jackendoff takes as a point of reference in his article), the term 'biolinguistics' (in its current sense) goes back to the mid-1970s. It is true that minimalist practitioners are using the term, because many (including Jackendoff, as pointed above) have seen points of contact between a minimal UG (the focus of minimalism, Chomsky 2007) and the attempt to uncover the biological (i.e. neural, genomic, etc.) basis of the language faculty. But as the manifesto of the online journal Biolinguistics (cited by Jackendoff) makes clear (Boeckx and Grohmann 2007: 3): "It is important for us to stress that biolinguistics is independent of the minimalist program. As Lenneberg's work makes clear, biolinguistic questions can be fruitfully addressed outside of a minimalist context."

It is therefore wrong to say that "[inquiry into the genetic basis of the language faculty] has been part of the literature for decades (even if much of the evidence is new) and is not specific to the biolinguists". It *is* specific to biolinguistics (as opposed to, say, sociolinguistics), but not, of course, specific to minimalism, or any other theoretical framework.

At times, Jackendoff appears to realize that biolinguistics and minimalism are not the same thing, since he writes (in a passage quoted above): "So to the extent that a theory of language permits a graceful integration with a plausible picture of the structure and function of the rest of the mind/brain, it places fewer demands on the genome, and therefore it is a better theory. I would contend that this too should be considered a criterion on linguistic theory from a biolinguistic perspective." (p. 590)[2]

The issue of graceful integration is indeed central to biolinguistics, but Jackendoff is not the only one to realize this. Numerous authors (including those of a minimalist background) have written about this in biolinguistic venues, as the frequent references to David Poeppel's reflections on this matter (Poeppel 2005, Poeppel and Embick 2005, Poeppel 2011, 2012) attest (see Hornstein 2009, Boeckx To appear, Samuels 2011, Di Sciullo and Boeckx 2011). It is simply not true that "biolinguists have bypassed an important biological and psychological issue", for this

---

[2]   See also the following passage:

Although these points all violate the preconceptions of "efficient computation" that gave rise to the notion of Merge, each of them is supported both within the theory of language and elsewhere in the mind/brain. Thus, they should be seen as welcome advances in pursuing a biolinguistic agenda, at once accounting for a wider range of phenomena in language and bringing the language faculty closer to graceful integration with the rest of the mind/brain. (p. 603)

And the concluding paragraph of his review (p. 617):

In each case the proposed answer is also motivated on grounds internal to language, and in each case it differs from the proposals of the Minimalist Program and biolinguistics, which are based on criteria of perfection, optimal design, and efficient computation. I conclude that a constraint-based and Unification-based Parallel Architecture leads to more satisfactory accounts of the linguistic phenomena in question, incorporating the insights of many other constraint-based frameworks. At the same time it provides a far more promising approach to the criterion of graceful integration, offering a quite different direction for biolinguistic research.

is what defines the field. True, Jackendoff is entitled to express skepticism about the results obtained so far, but the same could be said of his own approach. In fact, one could say that Jackendoff 'bypasses' important evo-devo issues when he writes "it seems to me, the distance between the nature of linguistic structure and what is known about the genetic basis of biological development is so great that the best that can be attempted at the moment is informed speculation, and I will have nothing to say about this issue here". Are we so sure that the distance between the nature of linguistic structure and what is known about the brain, or between the nature of linguistic structure and what is known about other cognitive systems, is less great?

While I personally agree with Jackendoff that we should try to assimilate what we can learn from other cognitive systems, I also think Jackendoff is right in stressing that we know far more about the structural properties of language than any other system. So how can we be sure in which way integration will proceed? As Newport (2010282) has written in her thoughtful review of modularity issues, "The generative tradition in language has given us an elegant and detailed articulation of how these principles work themselves out in language; whether the same principles apply in detail to any other domain remains to be seen, since few comparably sophisticated analyses have ever been done of other complex cognitive domains."

At this point, it seems fair to say that we know so little about so many things that appear necessary for a truly integrated biolinguistics that favoring some directions over others is a matter of placing one's bets (a necessary part in any scientific inquiry)—more a matter of gut feelings than anything else ('forecast' rather than 'fact'). But as Yogi Berra told us, it's hard to make predictions, especially about the future. Certainly, one cannot at present argue in favor of the superiority of one bet over another. Jackendoff may be more attracted towards an alignment with other cognitive systems because he has worked extensively on the relation between linguistic cognition and visual cognition, but I may be more attracted towards development because of results like Gunz et al. (2010, 2012), Neubauer et al. (2010) that suggest that species-specific patterns of brain configuration that may well give rise to a language-ready brain arise early in development (hence the relevance of evo-devo considerations). Wouldn't ignoring that amount to bypassing issues concerning the human brain?

The last comment I'd like to make in the context of the passages quoted at the beginning of this section concerns what Jackendoff takes to be central issues of "biolinguistics". Specifically, I think he is wrong about "perfection" being so important. The issue of optimal design was central at the beginning of the minimalist program, but numerous authors have pointed out that this issue has lost its centrality in later implementations of the program. My own feeling is that it has played a less direct role in the revival of biolinguistic concerns than other factors, such as results from comparative psychology or genomic studies (the term 'perfection' is not even part of the index of Di Sciullo and Boeckx (2011), as none of the chapters of the book elaborate on this issue). Moreover, Jackendoff is wrong in keeping separate the 'second' central issue in "biolinguistics" (which I take to be evo-devo concerns) and the 'third' issue ('perfection', or perhaps more generally, 'third factor principles of the sort discussed in Chomsky 2005), because they are clearly related,

as is evident from the evo-devo literature.[3] So, one cannot set one aside ("I will have nothing to say about this issue here") and try to focus on the other.

3 Architectural concerns

When it comes to offering arguments in favor of his own framework, Jackendoff offers very little new. Most of the themes, and specific examples used as illustrations, are to be found in his previous publications. Accordingly, I will not go through each and every one of them, as I have already done so elsewhere (Boeckx and Piattelli-Palmarini 2007, Boeckx et al. 2010).[4] Instead, I will focus on a few new points made by Jackendoff, and show that in each case they miss their targets.

As is well-known, Jackendoff is very critical of what he calls "syntactocentric" models, which place emphasis on the role of (narrow) syntax in structuring the modern language faculty, and favors his (and others') "parallel" models, where every component of the grammar is said to contribute equally to the nature of the language faculty. It is therefore no surprise to find in Jackendoff (2011) a series of attacks on the centrality of Merge, and recursion in language (familiar from Pinker and Jackendoff 2005 and Jackendoff and Pinker 2005), and a favorable treatment of "unification" as the core mechanism in language, an emphasis on words and on the redundancy of the lexicon, a demand for attention to phonology and semantics, and a discussion of possible evolutionary stages where syntax was missing (protolanguage).

Let me stress that all of these are important themes, and that in some cases Jackendoff is right to criticize some particular research directions (e.g., the position in Hauser et al. 2002; for my own critical remarks on the latter, see Boeckx (2013)), but wrong to say that these are representative of biolinguistics as a whole. But I cannot fail to find it ironic that in several places Jackendoff is led to conclude (expressing surprise!) that his position converges with the one he is attacking:

> "In a curious way, one might see Marantz 1998 as arriving at the same conclusion, though with a quite different twist" (note 22, p. 610),
> "in a curious way this story is compatible with Chomsky's speculations" (p. 616),
> "So we might want to say that the digital property of phonology comes by virtue of "natural law," in (I think) precisely the sense Chomsky intends" (p. 604)

---

[3] Design considerations, including issues like 'perfection' or 'optimality' are related to 'third factors' considerations in the following sense: third factor principles *on their own* lead to systems that are optimized systems because they ultimately instantiate the workings of laws. But biological systems are not the result of third factors alone. When I say that 'perfection' considerations no longer dominate minimalist writings, while third factors do, I mean that one can choose to study the contributions of third factors without elaborating on the qualitative ('optimization') consequences they have.

[4] I cannot resist mentioning that, quite apart from what he wants to say about biolinguistics, if he wants to criticize minimalism (which I recommend he do in separate pieces), Jackendoff should update his target. For example, he insists on the important of a "numeration" (p. 599) in the context of minimalist derivations, but numerations have long been left out of minimalist inquiries. In general, I think that one should keep the big picture from the details separate.

Such passages are in fact very frequent in Jackendoff's article, more so than in previous critiques by him, which makes Jackendoff (2011) particularly interesting. Thus, concerning the issue of redundancy (an issue that in fairness Chomsky and many others have rarely addressed), Jackendoff writes (p. 591), "[i]t is true that, as Chomsky reminds us, "the guiding intuition that redundancy in computational structure is a hint of error has proven to be productive and often verified." This intuition lies behind good scientific practice, and too easy an acceptance of redundancy can be a source of theoretical complacency. However, good science also ought to be able to consider redundancy as a solution when the facts patently point that way." In other words, it's an open issue.[5]

More eloquently still, when discussing the superiority of "unification"-based approaches over Merge-based approaches, Jackendoff addresses the obvious point: how do the elements to be unified get constructed in the first place? (In other words, what constructs the constructions?) To his credit, Jackendoff states the obvious (other construction grammarians are, unfortunately, far less explicit): "I should make clear that Unification alone cannot create constituent structure: it only creates a Boolean combination of pre-existing features and structures." (p. 602) Surprisingly, Jackendoff goes on to state "In order to build structure, one needs a skeletal constituent structure that can be unified with two or more items. Such a skeleton is of course already richly present in cognition: the part-whole schema. One formal realization of this schema is a set {x, y} with variable elements x and y as parts." But this is Merge, and Jackendoff knows it: "This can be unified with specific elements A and B to form the set {A, B}, in effect the output of Merge." "One might say then that these schemas are nothing but constraint-based counterparts of Merge, and this would be partly correct." Jackendoff quickly adds that "However, Merge per se is too limited". But he has just pointed out that unification alone is equally limited (it needs something like Merge). Accordingly, it strikes me as incorrect to oppose Merge and Unification. (I should point out that proponents of Merge-based systems would readily agree that Merge is not enough to capture all the facts in natural language grammars. All they are claiming is that in order to kick-start all the other operations, (all) you need (is) Merge.)

The ultimate irony is to be found on p. 616, where Jackendoff writes (in the context of language evolution), "perhaps beyond timing, the difference [between his and Chomsky's approaches–CB] is predominantly one of terminology". So much for "two views" about the human language faculty.[6]

---

[5]   If Gallistel and King (2009) are right (and they strike me as such), we have no idea about what the brain basis of memory is, so imagine how little we know to talk about redundancy ....

[6]   Incidentally, Jackendoff's insistence that his parallel model leads to evolutionary scenarios that are quite different from those to which Merge-based approaches lead is incorrect. As Clark (2013) shows in detail, there is not a simple dependency between syntactic theory and views on syntactic evolution. Such a conclusion should come as nor surprise, given the range of evolutionary scenarios compatible with Merge-based approaches, including those that Chomsky approves of, such as Lebeaux (1988), or those published in the *Biolinguistics* journal (works by Progovac and others, referred to in Jackendoff 2011).

In the context of evolutionary scenarios, I cannot fail to express my surprise about Jackendoff's vision. According to him (p. 615–6), "conceptual structure evolved first, long before language, in fact long before hominids. Then came phonology and its links to meaning, yielding protolanguage, and last came syntax and its links to both." But if semantics and phonology were already in place, and linked to one another, why was syntax needed at all?

Arguably the most unfair part of Jackendoff's overview concerns his (cursory) remarks about phonology and semantics. Jackendoff writes (p. 611):

> Some practitioners of the Minimalist Program (e.g., Phillips and Lau 2004, Marantz 2005) have commented that a theory with only one "generative engine" is preferable on first principles. This is taken to be sufficient to dismiss the Parallel Architecture, which has three generative engines. But the Minimalist Program addresses only syntax: it has no theory of phonology, no (generally accepted) theory of semantics, and no (generally accepted) theory of the lexicon. So it should be no surprise that it needs only one "generative engine."

But Jackendoff ignores that there are treatments of phonology within a minimalist-oriented biolinguistic approach, beginning with the rich tradition within Distributed Morphology (for the most comprehensive overview, see Samuels 2011, based on Samuels 2009, but see also Embick 2010, Marvin 2002, and Newell 2008). Although these may not constitute a "generally accepted" theory of (morpho-) phonology (has there been any since *Sound Patterns of English*?), they show how syntactocentric models can provide a solid foundation for models of phonology. True, Chomsky himself has not endorsed such a model (though some scattered remarks in his recent writings make me think he would favor it), but remember that Chomsky is not (or should not be) the target of Jackendoff's focus, if the latter is 'biolinguistics' at large. Within a field in the making like biolinguistics, there is no generally accepted theory of anything. Would Jackendoff's own writings on semantics or the lexicon, as rich as they may be, count as "generally accepted"? I don't think so. Theories, to count as such, should not only enable one to describe a rich set of facts, they should place constraints on what is possible, and here I think that Jackendoff's parallel architecture is too unconstrained. As I pointed out on other occasions (Boeckx and Piattelli-Palmarini 2007, Boeckx et al. 2010), it's hard to find the components of Jackendoff's vision that rule out structures that are known to be illicit.

When it comes to semantics,[7] Jackendoff rightly stresses the existence of 'thought' (conceptual structure) in other species, but I think he seriously minimizes the impact of syntax on these conceptual structures. Much like there is massive evidence for thought in other species, there is massive evidence that language affects conceptual combinatoriality. This is a message that comes from numerous domains (comparative psychology (Hauser 2009), archeology (Mithen 1996), developmental psychology (Spelke 2003)), and it is a finding that biolinguists are trying to capture (including in the book that Jackendoff takes as his target; witness my contribution to Di Sciullo and Boeckx (2011), but see also Pietroski 2007, Ott 2009). In essence, the finding amounts to the emergence, thanks to language, of robust cross-modular

---

[7]   Jackendoff completely misrepresents Hinzen's contribution to Di Sciullo and Boeckx (2011). He writes that Hinzen "proposes that in fact combinatorial syntax directly generates conceptual structure, so there is no need to map to the semantic interface; he has in effect reinvented Generative Semantics." Nothing could be further from the truth. Hinzen claims, along with— though perhaps more explicitly than—others, that syntax gives rise to meaningful representations that would be unavailable in the absence of language. Being syntactocentric, Hinzen's model is not Generative Semantics, but Generative Syntax. If anything, Jackendoff's separate generative semantic component is much closer to Generative Semantics, as Boeckx, Hornstein, and Nunes (2010) point out.

thinking (evidence for which is absent in other species). This is, in fact, a finding that is hard to capture in Jackendoff's framework, where conceptual structure in all its richness (and generatively) pre-dates the modern language faculty.

Jackendoff, of course, may disagree with this finding, but he cannot simply ignore it (doing so would be "bypass[ing] an important biological and psychological issue"). In fact, if Jackendoff had paid attention to that literature, he would have seen that many authors (myself included) attribute to words (or lexical items) an importance that he claims words have and that minimalists (he says) ignore (p. 599), "recursive structures cannot exist without units to combine"). He would have seen that as far as that literature is concerned, Merge is a generic combinatorial mechanism (coming "off the shelf, from [the] F[aculty of] L[anguage] B[road]").[8] He would in fact see (see Boeckx 2009, 2011, and especially Boeckx To appear) that some minimalists trying to contribute to biolinguistics blame standard (minimalist) models of being too "lexicocentric". In short, Jackendoff would have seen that Hauser et al. (2002) is not the only possibility on the table.

As I have written elsewhere (Boeckx Submitted), I agree with Jackendoff that Hauser et al. (2002) is problematic in many respects, but I also think that Jackendoff's argument in favor of recursion, to which a good portion of Jackendoff (2011) is devoted, in other cognitive domains is far from compelling. The crux of the matter is to be found on p. 591:

> Evaluating whether a particular domain of behavior or cognition is recursive requires a theory of mental representations in that domain. And unfortunately, outside of linguistics, there are virtually no theories of mental representation (with some exceptions that I will mention in a moment). In the absence of such theories, it is premature to assert that only language requires recursion.

This is correct, but by the same reasoning, is it not premature to assert that other cognitive systems require recursion? Here it seems to me Jackendoff minimizes the risks of making claims in the absence of comparably sophisticated analyses in other complex cognitive domains. Once again, I find it hard to argue for the superiority of one approach over the other.

4   Concluding remarks

Although the goals and ambitions of biolinguistics were first formulated over 50 years ago, too many pieces were missing to even guess what the puzzle would look like: we knew too little about the genome, about the cognitive profile of other

---

[8]   Jackendoff appears to be aware of this, as he points out in note 16, "Hornstein and Boeckx 2009 (89) make such an argument: "...it is reasonable to suppose that an operation like Merge, one that puts two elements together (by joining them or concatenating them or comprehending them in a common set), is not an operation unique to FL. It is a general cognitive operation, which when applied to linguistic objects, we dub 'Merge'." In other words, they acknowledge that recursion may be found elsewhere in cognition, but they do not call it Merge. Thus they reduce the notion that Merge is unique to language from an empirical hypothesis to a tautology." Jackendoff's comment is unfortunate, as he ascribes a claim derived from Hauser et al. (2002) "Merge is unique to language" to us. But the passage makes clear that we don't hold this position, so we are not putting forth any tautology.

species, about how evolution works, and also about what language is. Although we still know very little, we are now in a better position to seek integration, hence the rise (or revival) of biolinguistics. Being so young, the discipline is still in an exploratory phase, and I feel it would be seriously wrong to be dogmatic about anything. There are, of course, many views about the specifics of the language faculty, but I don't think there are two views about biolinguistics. It's the same program for everybody. And one should welcome theoretical pluralism.

Jackendoff is right about many things, but wrong to stress disagreements (especially superficial, "terminological" ones) at the expense of common goals and common hypotheses. When the rhetoric (fiction) is carefully separated from the facts, everyone turns out to be in the very same boat (like it or not, that's the boat Chomsky and Lenneberg constructed). We all "aspire to biological plausibility" (p. 617), but we all should recognize that we all face Poeppel's granularity mismatch problem. We also suffer from the absence of comparably sophisticated analyses outside of the language domain. So, it's very risky to claim that some of us are bypassing important biological and psychological issues. Being in the same boat, we all make slightly different bets regarding how to reach the land. These bets arise as a result of personal biases.

What I have tried to show in this paper is that Jackendoff is wrong to take his biases as a solid foundation to argue in favor of his own articulation of "biolinguistics". We all have to start somewhere, but is the essence of language really communication (consider how problematic this is: Balari and Lorenzo 2013)? Are we so sure that different theoretical articulations really make radically different evolutionary predictions, given the richness of an extended synthesis in biology (Pigliucci and Müller 2010)? Of course, as Jackendoff likes to say, one's theory of language evolution depends on one's theory of language. But one's theory of language evolution depends equally on one's theory of evolution. And are we so sure that evo-devo issues are too removed from current linguistics that we can set them aside?

In the end, Jackendoff points out (p. 615) that "All that could be argued is that it is plausible (though I can imagine that someday genetic evidence might be telling)". We could all imagine this regarding our pet theories. But imagination belongs to the realm of forecast, not fact. Pretending otherwise is entering the realm of fiction.

References

Balari, S., and G. Lorenzo. 2013. *Computational phenotypes: Towards an evolutionary developmental biolinguistics*. Oxford: Oxford University Press.

Boeckx, C. 2009. The locus of asymmetry in UG. *Catalan Journal of Linguistics* 8:41–53.

Boeckx, C. 2011. Review of Anna Kibort & Greville G. Corbett (eds.), *Features: Perspectives on a key notion in linguistics*. *Journal of Linguistics* 47:522–524.

Boeckx, C. 2013. Biolinguistics: Forays into Human Cognitive Biology. *Journal of Anthropological Sciences* 91:1–28.

Boeckx, C. To appear. *Elementary syntactic structures*. Cambridge: Cambridge University Press.

Boeckx, C., and K.K. Grohmann. 2007. The BIOLINGUISTICS manifesto. *Biolinguistics* 1:1–8.

Boeckx, C., N. Hornstein, and J. Nunes. 2010. *Control as movement*. Cambridge: Cambridge University Press.

Boeckx, C., and M. Piattelli-Palmarini. 2007. Linguistics in Cognitive Science: The state of the art amended. *The Linguistic Review* 24:403–415.

Cherniak, C. 2005. Innateness and brain-wiring optimization: Non-genomic nativism. In *Cognition, evolution, and rationality*, 103–112. Oxford: Blackwell.

Chomsky, N. 1986. *Knowledge of language*. New York: Praeger.

Chomsky, N. 1995. *The minimalist program*. Cambridge, Mass.: MIT Press.

Chomsky, N. 2005. Three factors in the language design. *Linguistic Inquiry* 36:1–22.

Chomsky, N. 2007. Approaching UG from below. In *Interfaces + recursion = language? Chomsky's minimalism and the view from semantics*, ed. U. Sauerland and H.-M. Gärtner, 1–30. Mouton de Gruyter.

Clark, B. 2013. Syntactic theory and the evolution of syntax. *Biolinguistics* 7.

Culicover, P.W., and R. Jackendoff. 2005. *Simpler syntax*. Oxford: Oxford University Press.

Di Sciullo, A.M., and C. Boeckx, ed. 2011. *The biolinguistic enterprise: New perspectives on the evolution and nature of the human language faculty*. Oxford: Oxford University Press.

Embick, D. 2010. *Localism versus globalism in morphology and phonology*. Cambridge, Mass.: MIT Press.

Evans, N., and S.C. Levinson. 2009. The myth of language universals: Language diversity and its importance for cognitive science. *Behavioral and Brain Sciences* 32:429–492.

Gallistel, C.R., and A.P. King. 2009. *Memory and the computational brain: why cognitive science will transform neuroscience*, volume 3. Wiley-Blackwell.

Gunz, P., S. Neubauer, L. Golovanova, V. Doronichev, B. Maureille, and J.-J. Hublin. 2012. A uniquely modern human pattern of endocranial development. insights from a new cranial reconstruction of the neandertal newborn from mezmaiskaya. *Journal of human evolution* 62:300–313.

Gunz, P., S. Neubauer, B. Maureille, and J.-J. Hublin. 2010. Brain development after birth differs between neanderthals and modern humans. *Current Biology* 20:R921–R922.

Hauser, M. D., N. Chomsky, and W. T. Fitch. 2002. The Faculty of Language: What is it, who has it, and how did it evolve? *Science* 298:1569–1579.

Hauser, M.D. 2009. The possibility of impossible cultures. *Nature* 460:190–196.

Hornstein, N. 2009. *A theory of syntax*. Cambridge: Cambridge University Press.

Jackendoff, R. 1983. *Semantics and cognition*. Cambridge, Mass.: MIT Press.

Jackendoff, R. 1987. *Consciousness and the computational mind*. Cambridge, Mass.: MIT Press.

Jackendoff, R. 1997. *The architecture of the language faculty*. Cambridge, Mass.: MIT Press.

Jackendoff, R. 2002. *Foundations of language*. Oxford: Oxford University Press New York.

Jackendoff, R. 2007. *Language, Consciousness, Culture: Essays on Mental Structure*. Cambridge, Mass.: MIT Press.

Jackendoff, R. 2010. *Meaning and the Lexicon: The Parallel Architecture 1975–2010*. Oxford: Oxford University Press.

Jackendoff, R. 2011. What is the human language faculty? Two views. *Language* 87:586–624.

Jackendoff, R., and S. Pinker. 2005. The nature of the language faculty and its implications for evolution of language (reply to Fitch, Hauser, and Chomsky). *Cognition* 97:211–225.

Lebeaux, D. 1988. Language acquisition and the form of the grammar. Doctoral Dissertation, University of Massachusetts, Amherst.

Lewontin, R.C. 1993. *Biology as ideology: The doctrine of dna*. Harper Perennial.

Lewontin, R.C. 2000. *The Triple Helix: Gene, Organism, and Environment*. Cambridge, Mass.: Harvard University Press.

Longa, V.M., and G. Lorenzo. 2012. Theoretical linguistics meets development: Explaining FL from an epigeniticist point of view. In *Language from a biological point of view: Current issues in Biolinguistics*, ed. C. Boeckx, M. Horno, and J.L. Mendívil Giró, 52–84. Cambridge Scholars Publishing.

Marvin, T. 2002. Topics in the stress and syntax of words. Doctoral Dissertation, MIT.

Mithen, S.J. 1996. *The prehistory of the mind*. London: Thames and Hudson.

Neubauer, S., P. Gunz, and J.-J. Hublin. 2010. Endocranial shape changes during growth in chimpanzees and humans: a morphometric analysis of unique and shared aspects. *Journal of human evolution* 59:555–566.

Newell, H. 2008. Aspects of the morphology and phonology of phases. Doctoral Dissertation, McGill University.

Newport, E. L. 2010. Plus or minus 30 years in the language sciences. *Topics in cognitive science* 2:367–373.

Ott, D. 2009. The Evolution of I-Language: Lexicalization as the Key Evolutionary Novelty. *Biolinguistics* 3:255–269.

Pietroski, P. M. 2007. Systematicity via monadicity. *Croatian Journal of Philosophy* 7:343–374.

Pigliucci, M., and G. Müller, ed. 2010. *Evolution—The Extended Synthesis*. Cambridge, Mass.: MIT Press.

Pinker, S., and R. Jackendoff. 2005. The faculty of language: what's special about it? *Cognition* 95:201–236.

Poeppel, D. 2005. The interdisciplinary study of language and its challenges. Technical report, Jahrbuch des Wissenschaftskollegs zu Berlin.

Poeppel, D. 2011. Genetics and language: a neurobiological perspective on the missing link (-ing hypotheses). *Journal of neurodevelopmental disorders* 1–7.

Poeppel, D. 2012. The maps problem and the mapping problem: Two challenges for a cognitive neuroscience of speech and language. *Cognitive Neuropsychology* 29:34–55.

Poeppel, D., and D. Embick. 2005. Defining the relation between linguistics and neuroscience. In *Twenty-first century psycholinguistics: Four cornerstones*, ed. A. Cutler, 173–189. Hillsdale, NJ:: Erlbaum.

Samuels, B. 2009. The structure of phonological theory. Doctoral Dissertation, Harvard University.

Samuels, B. 2011. *Phonological Architecture: A Biolinguistic Perspective*. Oxford: Ox-

ford University Press.

Spelke, E. 2003. What makes us smart? Core knowledge and natural language. In
    Language and Mind: Advances in the study of language and thought, ed. D. Gen-
    tner and S. Goldin-Meadow, 277–311. Cambridge, Mass.: MIT Press.

Cedric Boeckx
ICREA & Universitat de Barcelona
Department of Linguistics
Gran Via de les Corts Catalanes, 585
08007 Barcelona
Spain
cedric.boeckx@ub.edu