The Biolinguistic Enterprise: *New Perspectives on the Evolution and Nature of the Human Language Faculty.*

Edited by Anna Maria di Sciullo and Cedric Boeckx
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One of the most fascinating but at the same time still not very thoroughly investigated fields is the "Biolinguistic enterprise" of human language faculty as opposed to other means of communication between say animals or computers and humans. Anna Maria di Sciullo and Cedric Boeckx, the editors of the present issue, belong to the founders of the new type of interdisciplinary research that seeks to explore "the basic properties of human language and to investigate how it matures in the individual, how it is put to use in thought and communication, what brain circuits implement it, what combination of genes supports it, and how it emerged in our species." (General preface).

In addressing these questions the present volume aims to advance our understanding of the interactions of mind and brain in the production and reception of language, to discover the components of the brain that are unique to language (especially those that also seem unique to humans), and to distinguish them from those that are shared with other cognitive domains. Advances in theoretical linguistics, genetics, developmental and comparative psychology, the evo-devo program in biology, and cognitive neuroscience have made it possible to formulate novel, testable hypotheses concerning these basic questions.

The goal of this direction of Biolinguistics is rooted in the works of Eric Lenneberg (1967) who discovered a close link between mind, brain and language faculty and gave a new impetus for explanatory theories of language, the most prominent of which being that of Noam Chomsky (1955/57 et seq.). From the very beginning, these theories tried to integrate the study of language into the study of general physical systems, under the assumption that, if the thesis that Language is a "part of the natural world" is falsifiable, it is ultimately a physical system and, optimally, it must obey the same very basic principles. The goal of the most articles and chapters in the present volume is devoted to this aspect, but from three different, but close connected perspectives: Part I. Evolution, Part II. Variation, and Part III. Computation.

We shall try to give the spirit of this impressive volume without being able or even wanting to cover all issues in detail. More important will be to react to the most demanding non-trivial questions with hopefully reasonable and also non-trivial answers.

In the *Introduction: Contours of the Biolinguistic Research Agenda* by *Anna Maria Di Sciullo and Cedric Boeckx* (pp. 1-16) Anna Maria di Sciullo and Cedric Boeckx first give an impressive review of the development of then new agenda of Biolinguistic research, starting with the short remark that it was indeed a very important conference in the beginning of the 70ies that gave rise to the new research agenda of Biolinguistics: "It was used in the title of a conference "A Debate on Bio-Linguistics," organized by Massimo Piattelli-Palmarini in 1974, and it figures prominently in an address at the annual meeting of the American Association for the Advancement of Science made by Salvador Luria in 1976... After that, the term went underground and did not reappear until the turn of the millennium, as the title of a book by Lyle Jenkins (2000). A big deal goes back to the five conferences of the International Biolinguistics Network between 2007 and 2011, an initiative that has been consistently developed and launched by Anna Maria di Sciullo and her colleagues.
In the first part of the book "Evolution" Robert Berwick and Noam Chomsky's contribution "The Biolinguistic Program: The Current State of its Development" (pp. 19-41), a new perspective of language research is proposed, namely *human language* „as a particular object oft he biological world". Thus, the study of language is „the study of a biological world" within which language is only one of many physical entities or epiphenomena that have presumably evolved over a period of more than 100.000 years:

> According to a fairly general consensus among paleoanthropologists and archaeologists, these questions are very recent ones in evolutionary time. Roughly 100,000 years ago, the first question did not arise, because there were no languages. About 50,000 years ago, the answers to both questions were settled: our ancestors began their trek from Africa, spreading over the entire world, and as far as is known, the language faculty has remained essentially unchanged—which is not surprising in such a brief period. (Berwick & Chomsky 2011: 19).

Among the many puzzling questions about language, Berwick and Chomsky take two for being the most salient ones: "First, why are there any languages at all, evidently unique to the human lineage, what evolutionary biologists call an "autapomorphy"? Second, why are there so many languages?" (ibid.) These two salient questions, namely language's origin and variation, are also the major topics of the present book. It is crucial to explore how these questions may be informed by both recent biological work on evolution and development and the framework provided by the Minimalist Program. Human language appears to be best regarded as an internal computational system driven by a single recursive operator yielding

hierarchical structure, with its particular functional design shaped by the link to systems of thought and action, rather than by the secondary process of externalization, that is, sensory-motor parsing or speech/sign. What relevant biological and evolutionary results there are, including recent results on FOXP2 and its analogous effects in other species such as songbirds, as well as computational considerations of parsing efficiency, all point to this conclusion. This chapter also reviews how an account of the apparently recent and saltational evolutionary emergence of this recursive system of "discrete infinity," apparently unique to the human species, profits from an examination of the ongoing revision of evolutionary explanation in light of new research on the link between evolution and development, which highlights the possibility of relatively rapid shifts in an organism's biology, given even minor genomic variation.

In the next chapter, Cedric Boeckx (CB, pp. 42-64) develops some very crucial and basic ideas on the program of biolinguistics, the main concerns of this article being the notions of the "Darwin's problem" and so-called "Cartesian Biolinguistics" ("Some Reflections on Darwin's Problem in the Context of Cartesian Biolinguistics", pp.). In paragraph 3.3.1, entitled "The conceptual relationship between Plato's Problem and Darwin's Problem", CB addresses the central problem in generative grammar, as made explicit in Chomsky (1965, chapter 1), namely how to account for the human capacity for language acquisition:

> How any child, short of pathology or highly unusual environmental circumstances, acquires at least one language by the time they reach puberty (at the very latest) in a way that is remarkably uniform and relatively effortless. The acquisition of language is all the more remarkable when we take into account the enormous gap between what human adults (tacitly) know about their language and the evidence that is available to them during the acquisition process. It should be obvious to anyone that the linguistic input a child receives is radically impoverished and extremely fragmentary. It is in order to cope with this "poverty of stimulus" that Chomsky claimed that humans are biologically endowed with a capacity to develop a language. The biological equipment that makes language acquisition possible is called Universal Grammar (UG). (44)

Although even Noam Chomsky in his latest assumptions seems to be skeptical about the role of Principles and Parameters of UG (cf. Chomsky 2007) minimizing both notions more or less to two crucial operations that are not even only a part of FLB but maybe of FLN as such (cf. Hauser, Chomsky, Fitch 2002), namely *Recursion and Merge*, it remains unclear to us how the notion of UG, the role of learning during the process of language acquisition (LAD) and the so called third factor principle can be put into an appropriate relation to each other so that Plato's Problem and Darwin's Problem can be harmonized in one or another way. This very lasting and most demanding problem is still awaiting a solution. In the course of the article, CB mentions several possible ways and proposals how to approach the problem of selection

and variation posed by Darwin. With such concepts as modularity, evolvability, robustness, epigenetic inheritance, and phenotypic plasticity as key components, Pigliucci (2007) seems to propose an interesting broadening of our research program (cf. CB, p. 48).

Of great significance is the fact that the concerns that animate the return to the insights of the Rationalist Morphologists are the very same concerns that animate research in (Cartesian) Biolinguistics. By using Cartesian Biolinguistics, CB intends to point to an important distinction within those who conceive of linguistics as a branch of biology (at a suitable level of abstraction). CB calls them "neo-Darwinian biolinguists". By contrast, those that he would call Cartesian biolinguists follow Chomsky in (i) favoring internalist explanations, (ii) seeing design and topology where others would see tinkering, and (iii) focusing on Form over Function." Indeed, once the complexity of biology as a whole, and evolutionary biology in particular, is clear, any perceived conflict between "Chomsky and Darwin" (Dennett, 1995), or any need to reconcile them (Calvin and Bickerton 2000), quickly evaporates.

In 3.2., entitled "Key Novelty", CB underlines the important role Minimalist program has played with respect to the problem of Evolution, Variation and Ontogenesis simplifying the system of UG, reducing redundancy and raising economy as an important key issue of parsimony (following the principle of Occam's razor). Two operations are central here: *Recursion* (from Hauser, Chomsky and Fitch 2002) and *Merge.* Even if we do not agree with CB and Chomsky (2005) that Mergeability of two lexical items must be ascribed to *Edge* features (cf. Krivochen 2011), we still believe that concepts and mapping of two single concepts to syntax via lexicalization must follow a procedure not yet quite clear that is unique for human language faculty (FLN) only. In any case, the opposite of Mergeability (called "delayed Merge") caused e.g. by genetic mutations of FOXP2 and other genes shows that this operation belongs to the core properties of computation and mapping of concepts of C-I onto S-M interfaces and remains illegible in case of severe impairments of e.g. individuals with verbal dyspraxia or specific language impairment (cf. Kosta, Krivochen, Peters, 2012; Kosta, Friedrich, Krivochen, Radeva-Bork, Robinson & Schaner-Wolles, work in progress).

Chapter 4 entitled "Syntax Facit Saltum Redux: Biolinguistics and the Leap to Syntax" is a fascinating contribution developed and neatly argued by Robert C. Berwick (RCB, pp. 65-99). The motto is attributed to the famous dictum *Natura non facit saltum* "Nature makes no

leaps" or better: "the nature does not make jumps", the latter being a principle of natural philosophy since at least Aristotle's time (exactly, it has been developed in Eleaten: Old Greek. Ἡ φύσις οὐδὲν ποιεῖ ἅλματα). Much later it appears as an important axiom of Gottfried Leibniz (New Essays, IV, 16) and Isaac Newton, the co-inventors of the infinitesimal calculus (cf. Law of Continuity). It is also an essential element of Charles Darwin's treatment of natural selection in his *Origin of Species* (1859) and his *Descent of Man* (1871). This natural principle or even law takes up the initial idea that natural things and properties change gradually, rather than suddenly. In a mathematical context, this allows one to assume that the governing equations are continuous rather than abrupt and also differentiable to some degree. The quantum mechanics is sometimes seen as violating this principle, with the major idea of a quantum leap. Erwin Schrödinger in his objections to quantum jumps supported the principle. In the biological context, the principle was used by Charles Darwin and others to defend the evolutionary postulate that all species develop from earlier species through gradual and minute changes rather than through the sudden emergence of new forms. Modern evolutionary biology has terminology suggesting both continuous change, such as genetic drift, and discontinuous variation, such as mutation. However, as the basic structure of DNA is discrete, nature is now widely understood to make jumps at the biological level, if only on a very small scale.
RB is concerned with one of the most interesting "evolutionary puzzles", namely language being a property solely unique for the human race marking "the obvious discontinuity between the human species and all other organisms: language is evidently unique to the human lineage" (p. 65).

The integration of language pathologies, genetics, brain imaging, molecular embryology, and sequential gene expression by means of DNA/RNA/proteomics is a remarkable open possibility of new agenda called genetic foundations of language faculty. The article "A Geneticist's Dream, a Linguist's Nightmare: The Case of FOXP2" by Massimo Piattelli-Palmarini and Juan Uriagereka (MPP & JU) (pp. 100-125) opens a new, so to say "molecular window", into the research of language but it is by no means the only one that has been studied in the past few years but at the same time it is evident that the findings by genetics, linguists and other disciplines should work hand in hand instead of against each other. In the present article a certain (well founded and well argued) skepticism but also a good portion of healthy optimism with respect of the findings of molecular and human genetics cannot be denied: "As it turns out, the most detailed genetic, developmental, and brain-activation analyses to date lead to conclusions that in many instances clash with those derived from over

fifty years of linguistic inquiry in generative grammar. Though we have no reason to question the accuracy of the genetic, brain-imaging, and develop- mental studies as such, when it comes to mapping a very specific point mutation of the FOXP2 gene onto the differential activation of brain areas (with respect to normal subjects), we do question the interpretations and conclusions that we find in the technical literature. We will show here that, for each one of the brain regions cited in the literature as pertinent to what the gene is really up to in linguistic behavior, other neurological publications suggest different functions. We stress that remarkable genetic and biochemical analyses are being mapped onto imperfect knowledge of the brain, rather arbitrary evolutionary reconstructions and, alas, a very naive linguistics. Linguists should of course welcome a prospective integration of different domains of inquiry. But in the present case, if these distinguished geneticists, neurologists, and embryologists are right, then a lot of linguistic theory and data are deeply wrong and vice versa. Our aim is to show that this need not be the case, and that both fields may in the end be correct, albeit under a significantly different interpretation of the facts.“ (MPP & JU, 100-101). Let us briefly recall some basic facts on what the subject is about: in the last decade of the twentieth century, an entire English family was brought to the attention of neurologists and logopedists at the Institute of Child Health in London working with Jane A. Hurst and her team. Over three generations, about one half of the family members showed a remarkable language deficit, which presented the textbook characteristics of an inheritable condition. Psycholinguist Myrna Gopnik studied this deficit and was able to track the family (ever since known as KE) also in Montreal (Gopnik 1990). Although we will not describe the condition in great detail here (we refer the reader to the review in Vargha-Khadem et al. 2005), we would like to underline the following facts: The members of the KE family did not suffer in other physical, oral-facial, auditive or hearing problems while they failed to complex jaw movements. By accident, a mutation was discovered on a section of chromosome 7 of the FOXP2 gene. Since then, many teams, among others also the team of Antony Monaco and Simon Fisher (cf. Monaco et al. 2001) worked on the discovery of speech disorders and language impairments that can be traced back to a genetic defect. The observed speech disorders are caused by the mutation of certain genes that are broadly known as FOXP2 gene. These horns various types of verbal dyspraxia, problems in articulation, which have been proven by word repetition tests, further cases of orofacial dyspraxia, in which the control of facial muscles is impaired, but otherwise no impairment of motor function observed. In general, however, find a certain correlation between relative low IQ, and the impairment of grammar (especially syntax). We can say that individuals affected with developmental

disorders of speech and language have substantial difficulty acquiring expressive and/or receptive language in the absence of any profound sensory or neurological impairment and despite adequate intelligence and opportunity. The article by MPP and JU investigates the limits of genetics and linguistics if taken as two separate, isolated disciplines. Instead, what one learns from the observations and additional remarks of this highly motivating and interesting chapter is that very often linguists and genetics do not work together as it should be. The discovery of the KE family in Great Britain shows opportunities but also limits as the present article by MPP and JU tried to demonstrate. New methods, such as the New generation sequencing as applied in the project by Kosta et al. (in progress)[1], enlarge and enforce the interdisciplinary research and show new paths how to access the link between genes, cortex and language faculty.

The next, brief chapter, "Biolnguistic Investigations: Genetics and Dynamics", by Lyle Jenkins, deals with the issue of asymmetry in the biological world. The author claims, following Di Sciullo (2005, this volume) that asymmetry is a fact in biological systems (brain lateralization, for example), and a plausible biolinguistic line of inquiry is to find out whether it has arisen from symmetry or some other independent source. This is of special interest since the physical world seems to be ruled by very deep principles of symmetry, from string theory to relativity. According to Jenkins, asymmetry would arise as a result of self-organization of biological units, which is a very interesting thesis specially because of its apparent incompatibility with deeper physical claims, but, once again, just as string theory came to fill the gap between the symmetric Universe that relativity needed and quantum leaps at the Planck scale, there could be a deeper solution for this. For example, considerations of "wiring economy", without it being clear whether there is just one optimal choice, this is, if we have "the best of all possible brains", and if so, given which interface conditions. The formal model Jenkins has in mind is Turing's, which could arguably be said not to be the best choice, since it has not been updated to deal with recent discoveries in both mathematics and biology, like chaos theory. However, a good review on Turing-based models (including non-linear mathematics) is provided and so the objection is partially dismissed, even though the models are not described at length. These are the questions that, proceeding to a mode DNA-oriented biological inquiry (this is, how "leaps" can be identified and studied within the genome, so

[1] Full title of the project: "Differentiated linguistic and genetic explorations of language impairments and disorders within a biolinguistic approach on language faculty and language design (UG)".

called *whole genome association studies*, WGA), Jenkins proposes and that generate great expectations in the field.

The following chapter 7, presented by Tecumseh Fitch, is devoted to the evolutionary aspect of homology (pp. 135-166). This chapter concludes the first part of the book. Fitch is taking a very interesting stance toward the evolutionary aspect of the genetic and evolutionary trait between humans and primates stating that "there is no reason why the search for homologs to human cognitive traits should stop with primates, given that most aspects of human brain structure are shared with mammals in general, and indeed that the most salient organizational principles of the nervous system characterize all vertebrates (Striedter 2004). We are mammals, and vertebrates, just as surely as we are primates. Furthermore, as recognized at least since Darwin, the closest animal analog to certain human characteristics is found not in primates or other mammals, but in birds (e.g. habitual bipedalism, or vocal learning; Darwin 1871). Understanding both the biological basis for, and constraints on, such traits and their evolutionary origins in terms of adaptive function, we need to cast the net wider than primates or mammals, and perhaps in some cases beyond our own vertebrate phylum" (Fitch, 135).
In the following Fitch is analyzing a very interesting "trait" of deep homology: "Deep homology exists when the develop- mental mechanisms responsible for generating a trait are shared by common descent, even though the phenotypic trait itself was not present in the common ancestor (Shubin, Tabin, and Carroll 1997; Hall 2003). For example, the wings of flies and those of birds are not homologous in any traditional sense (the common ancestor of birds and flies did not have wings). Nonetheless, the genes and developmental processes that generate wings turn out to be shared between these species. Thus, in cases of deep homology, the developmental mechanisms generating a trait are homologous, but the trait itself is not." (p. 136). Fitch shows that even though the architects of the neo-Darwian synthesis (e.g. Mayr 1970) assumed the deep homology to be very slim to hinge upon, the recent development of molecular genetics can help in broadening and deepening our understanding of human language faculty and genes. The hypothesis put forward is called "Pervasive Deep Homology Hypothesis" (PDH). Despite its uniqueness at the phenotypic level, human language (FLB) is generated by mechanisms that are broadly shared, at the genotypic level, with other animals. More specifically, both regulatory genes (Hox, Pax, Fox, etc.) and housekeeping genes (genes building enzymes, neurotransmitters and receptors, cell structures, etc.) involved in language will have clear orthologs in other species. The critical changes in the human genome that underlie the multiple mechanisms of the FLB will be either small tweaks to these orthologous

genes at the sequence level (as in FoxP2), or possibly larger changes in paralogous genes, following recent gene duplication events. Finally, the changes in phenotype and function caused by such genotypic changes will have direct parallels in other extant species (though not necessarily in cognitive domains having anything to do with communication), constituting deep homology as already documented for the evolution of eyes, limbs and vocal learning.
As already intimated above, this hypothesis would have seemed absolutely crazy two decades ago, before the discovery of Hox genes, and may strike some as highly implausible even today. This hypothesis is quite strong (in the sense of being readily falsifiable): I have phrased it this way purposefully. But strong and provocative hypotheses play an important role in science, and I think the examples discussed in this chapter make this particular hypothesis not only plausible but, at least for most aspects of the FLB, very likely true. There may well exist aspects of language that are truly unique to humans, and to language, at the phenotypic level (members of the subset of the FLB that my colleagues and I have dubbed the FLN; Hauser et al. 2002), but their specific genetic basis might nonetheless turn out to be shared. That is, traits that are truly unique to human language may nonetheless result from more basic aspects of cell morphology and/or function, or principles of neural development, that are shared, perhaps broadly, with other species. Thus, I will go so far as to suggest that the PDH hypothesis should be treated as the default in biolinguistics, assumed until proven otherwise.

The *second part of the volume* is devoted to the ***range of variation***. Here, both contributions to the evolutionary aspect of language variation and general theoretical contributions and diachronic findings are highlighted by some very interesting ideas. Due to lack of space, we cannot deal here with all seven contributions, but we will focus on the following articles: 8 The Three Factors in Design Evolution and Variation by Lyle Jenkins, furthermore the contribution no. 9 Three Factors in Language Variation by Charles Yang, the article no. 10 Approaching parameters from Below by Cedric Boeckx and the Article no. 13 A Biolinguistic Approach to variation by Anna Maria Di Sciullo. Contributions No. 11 (Bio) Linguistic Variation: Have / be in the Present Perfect by M. Rita Manzini and Leonardo M. Savoia and contribution No. 12 The Biolinguistic Program and Historical Reconstruction by Giuseppe Longobardi Cristina Guardiano are left for discussion.

In his contribution entitled “The Three Design Factors in Evolution and Variation” (pp. 169-

179), Lyle Jenkins takes up the core questions of the biolinguistic program developed in Chomsky (2005):

(1) What is knowledge of language and how is it put to use?

(2) How does language develop in the child?

(3) How does language evolve in the species?

But as we know, the biolinguistic perspective is also concerned with the interplay between what Chomsky has termed the "three factors" in language design: (4) genetic endowment, (5) environment, and (6) principles not specific to the faculty of language (Chomsky 2005). It has been suggested that an example of (6) might be computational efficiency. Lyle Jenkins in his article suggests another example, namely, viz., principles of symmetry, which are non-domain specific and non-species specific, but which can contribute to the variation seen in biological systems, including language. These kinds of questions about design are sometimes called the why questions of biolinguistics; that is, why language is as it is.

Chapter 9, by Charles Yang is concerned with "Three Factors in Language Variation", but as specialist in computing and language acquisition he adresses above all the problem how a child acquires a language. Yang is concerned with the problem of variation and selection in showing that parameter setting in early child grammar (such as the one for V2 effect in German or pro-drop vs. top-drop in Italian and Chinese) has to consider predictions about how a child learns on the basis of sufficient input. Thus, parameters must provide remedy for both the formal and the empirical problem in child language. The hope, in our view, lies in paying attention to the factors of experience (1b) and the process of learning (1c), which have not been addressed with sufficient clarity in the generative approach to acquisition. The variational learning model (Yang 2002) seems to be such a promissing "attempt to provide quantitative connections between the linguistic data and the child's grammatical development through the use of parameters. To capture the gradualness of syntactic acquisition, we introduce a probabilistic component to parameter learning."

Chapter 10, by Cedric Boeckx, addresses the problem of Principles and Parameters and their plausibility. The author's aim seems to be to rescue the essence behind the P&P "approach" and implement it in a Minimalist way, since its GB embodiment has "outlived its usefulness", in terms of the degree of knowledge we can gain from it. It is not clear, from the beginning, the difference between "approach", "theory" and "program", an essential difference if the P&P "approach" is to be maintained as such. It seems that the objections towards P&P are

directed to its GB version, but the difference between the GB-version and the Minimalist version are not clear to us, since the idea of optionality being part of UG is still there. If there is any further claim, then we are in presence of a theory, not an "approach". This opposition is spelled out in section 10.2: apparently, the GB version requires a richly articulated UG, whereas the tendency in recent Minimalism (Chomsky 2007), is to "approach UG from below", impoverishing it while still accounting for the properties of natural languages. During GB times, and even in early Minimalism, several models of Parametric variation arose, like Yang's (2002) and Thornton & Tesan's (2007), all maintaining the same underlying claim: there is a set of invariant properties encoded in *principles*, which constitute the core of the initial state of the Faculty of Language FL. The author points out theoretical and empirical complications of those models that assume a hierarchy between parameters (hierarchy that can be traced to papers as recent as, for example, Uriagereka 2007), a hierarchy that is obviously incompatible with Minimalist desiderata since the ruling principles of the structured set of parameters has to be stipulated as it does not follow from virtual conceptual necessity. As far as Minimalism is concerned, languages are assumed to be underlyingly the same (no news here), all variation being recoverable from "easily detectable properties of utterances" (Chomsky 2001). Boeckx is right in questioning the entity of those properties, since there is no definition or criterion to recognize them. He proposes what he calls "Strong Uniformity Thesis":

Strong Uniformity Thesis
Principles of narrow syntax are not subject to parametrization; nor are they affected by lexical parameters.
Under the SUT, points of variation (i.e. parameters) would be confined to the margins of narrow syntax, especially the morpho-phonological component (PF)

As in the GB tradition, parameters are restricted to the lexicon (e.g., Wexler & Manzini, 1987), this is, to the pieces to be inserted in syntactic terminal nodes under a separationist framework. Boeckx deprives syntax of any possibility of variation, which is a desirable state of affairs (specially within a free-Merge framework), but with the concomitant undesirable price of stipulatively pushing the burden to the PF interface, which is in most accounts (even in Chomsky's) parasitic and overlooking the semantic component completely. Boeckx sees three sources for his SUT: Borer's 1984 conjecture that variation is restricted to the lexicon, Chomsky's (2004) unrestricted Merge proposal (which is, however, in contradictory terms

with a strongly agree-driven system) and the linkage between narrow syntax and the semantic component, which is thought to be a uniform mapping. This third conjecture is highly controversial, as the limits between semantics and syntax are not well-drawn and, as Hinzen (this volume) points out, it is very likely that it is syntax (i.e., Merge) what structures thought, and, even more, licenses the possibility of computation.

Section 10.4 is devoted to the analysis of parameter schemata, and the way in which different proposals go inside the Minimalist architecture. The parameters that are discussed are parasitic on the notion of Agree and Feature Strength, which makes the argumentation weaker than if the proposed parameters followed from independent reasons, optimally, VCN. However, within the chosen path, the argumentation flows with ease, and, should one assume a highly articulated feature structure (which, by the way, does not go very well with Boeckx's previous Free Merge proposals), the conclusions follow from the premises without (more) added stipulations. Variation would amount, by and large, to the question whether a feature bundle *f* is expressed synthetically or analytically given certain PF possibilities.

The conclusion expresses a very clear idea: Plato's problem has to be rethought from the ground up. It is not clear that this chapter fulfills this condition, since it assumes most part of the Chomskyan machinery, but the desideratum is there and is very welcomed as a programmatic suggestion.

Manzini & Savoia tackle the problem of variation within biolinguistics in chapter 11. Their point of departure is the *lexical parametrization hypothesis* that Wexler & Manzini proposed in 1987, and goes along the Chomskyan lines in so far as all variation is restricted to the LEX. The hypothesis, within a biolinguistic framework, is supported by the fact that no genetic variation appears in FLN, and LEX would belong to FLB. Lexical variation would be affected by external (i.e., geographical, political, social) factors but, crucially, not biological ones. The authors also make mention to separationist frameworks like Distributed Morphology, although the argumentation is weaker by moments, since in a DM model variation would affect mainly the B-List, but by no means the A-List. A good point, however, is the fact that they problematize the fixed distinction between lexical and functional categories, as obscuring underlying universalities. Based on independent evidence, they claim that there is no separate functional LEX, universal and divorced from the substantive or lexical LEX. Moreover, they ascribe to a representational system, in which notions like transitivity and voice can be directly said to be LF primitives, instead of arising as result of syntactic componentiality. In this sense, the system is not very economic, since the array of basic elements is enriched with

objects whose primitive, atomic character is far from clear. Sections 11.2 and subsequent present the empirical data in which the analysis is based.

In chapter 13, Anna Maria Di Sciullo analyzes inter-linguistic variation from a biolinguistic point of view. Variation, from this point of view, would result from the interaction between an innate device and factors of concrete experience, variation being constrained by factors external to FL (contrarily to the GB model). Her focus is put on the D domain, affixes and clitics. The first part of the article, 13.1.1, introduces the concept of Parameter as it was understood in GB and actualizes the idea with Minimalist assumptions. There can be felt the strong presence of the concept of Asymmetry of Merge, which the author has already worked on (e.g., Di Sciullo & Isac, 2008) and presupposes a feature characterization for each element that enters into a Merge relation with another and a checking taking place in this relation, quite in the line of Pesetsky & Torrego, 2007. The argument can be easily followed and, should one accept her premises, the conclusions derive straightforwardly, without wiping anything under the rug. And this argumentative virtue is actually fundamental, since the leap from linguistic parameters to biological parameters (13.1.2), although natural from a theoretical point of view (going beyond explanatory adequacy, asking for *justifications* of why things are the way they are), is not uncontroversial. Once again, the concept of Asymmetry proves essential to the argumentation. If that property can be found in all biological (and physical, considering the potential scope of Montell's quote that "*asymmetry in force will cause dynamics*") domains, then the Biolinguistic enterprise would have stronger basis. Biological variation is claimed to manifest in language by means of the feature system that triggers Merge, an asymmetric operation since there is feature checking in play. The justification of features of the form [val-F] from a biological point of view, however, is not fully accomplished, but presupposed. This is not a drawback if the reader accepts the basic tenets of mainstream Minimalism, but could prove problematic for readers with a strong biological background, and not familiar with the "small letter" of the Minimalist contract. Feature valuation is at the very heart of the system, and asymmetry follows from the fact that there is a local relation between two instances of a feature, one valued and one unvalued. That system is implemented at all levels of morphosyntax. Should this approach be correct, however, the system of variation exposed by Di Sciullo would be very elegant and parsimonious, although its biological plausibility (i.e., how do we understand *biologically* the nature of an [u-V] or [u-Dem] feature? What are their neurological basis? Is there any difference that can be seen in an fMRI, for example?) would still need further clarification. The author claims (p.c.) "*Features, categories, rules, sets, derivations, etc. is part of the*

vocabulary of formal linguistics. They are not part of the vocabulary of biology. After all nobody had its features, […] *removed by a clinical intervention. Difficult to use the microscope or some other biological method to really isolate them yet. But it is possible to formulate hypothesis on how these systems work, what are their basic parts how they combine how they vary. Biolinguistics, from a linguistic view point can be thought of as using the vocabulary of linguistics to model a part of the biological system, we do not know the full functioning yet. The biological nature of features and categories and merge, etc... is yet to be discovered. There is no breakthrough that I know of allowing to choose on biological grounds for "categories" over "features" or "features" over "categories as bonafide biolinguistic elements.*". Many of these inconveniences could be avoided if connectionist networks like those programmed by Jeffrey Elman in the 80s and 90s were used: such networks could encode categories (like N and V) as neurological activation thresholds based on distributional evidence. There is, then powerful evidence in favor of "categories", but not a single note on "features", which are, as the author mentions, part of the formal vocabulary of chomskyan linguistics. Using linguistic vocabulary for biological systems is not justified in the article, but just assumed: the enterprise needs the development of a new vocabulary, as other chapter in this volume show (e.g., Chapter 6).

This is one of the reasons why we would like more development of the notion of *derivational complexity* (13.1.3), since the proliferation of features leads to highly articulated trees, in the line of cartographic approaches. The economy of derivation and representation is not obvious at this point.

Part 13.2 is devoted to *diachronic* variation. Once again, the whole argument is based upon the notion of asymmetry, now in dynamic relation with symmetry, which gives us a picture like the following (fig. 18):

Anti-symmetry > asymmetry

The pattern obeys the dependence on, for example, left-right relations (e.g. left-Spec vs. right-Spec), following the proposal of Kayne (1994) of LCA. In an anti-symmetric stage, there are two options for linearization, whereas in an asymmetric stage, only one option is possible. The fluctuation is exemplified from L-acquisition data, formation of compounds and linearization of constituents within the DP. A possible conflict here is the heavy dependence on the tree-like 2-D representations, in contrast to what has been posited recently in cognitive science and even within linguistics, of 3-D representations of symbolic structures (either linguistic or not). As a consequence, this point could arise some debate on the nature and

ontology of symbolic mental representations, which would be very much welcomed within the Biolinguistic enterprise.

The most important part in Anna Maria di Sciullo's paper concerns the assumption that there is an asymmetry in the operation Merge and breaking of the symmetry by feature-valuation: "*there is also asymmetry and breaking of symmetry in evolutionary developmental biology, and in language that the variation may be due to symmetry braking related to a change in feature values.*" (p.c.). In addition, di Sciullo assumes that there is behavioral evidence and ERP evidence that "*derivational complexity is measurable on the basis of neurological experiments*" (p.c.). However, the concept of "derivational complexity" is left undefined (is it just the number of Merge applications? How could the interfaces read the number of applications of Merge if not by using labels, which violate the inclusiveness condition?), and recent articles in which experimental evidence is shown (cf. di Sciullo 2012; Di Sciullo, A.M. & N. Tomioka 2008; Tsapkini, D., G. Jarema, & Di Sciullo, A.M. 2004**)** do not lead unambiguously to any definition (apart from methodological considerations involving abstracting derivational complexity from behavioral evidence, which includes the assumption that the mind works like a Turing machine, for example). There is, then, a certain "leap of faith" one has to take in order to follow the argumentation without problems.

The conclusion, clear and straightforward, is that *asymmetry arose with language*, therefore being central in the functioning of FL. Symmetry, the author claims, could have been brought by experience. This last claim, attractive and challenging, asks for further development, as it could allow us to gain deeper insight on both the inner functioning of mental faculties and the relation between the architectural properties of those faculties and the external phenomenological world.

In chapter 14, Richard S. Kayne, the "father" of Antisymmetry, analyzes the consequences of adopting the concept in a biolinguistic framework, specially with respect to the distinction between grammatical categories like N and V.

Section 14.1 starts with a discussion on recursion: is it available in non-humans? If so, to what extent? The possibilities Kayne proposes are the following (p. 329-330):

1. *It might be that in non-human species recursion is available with spell-out to Phonological Form (PF), but there is no compositional interpretation available, that is no mapping to Logical Form (LF) (and correspondingly no notion of idiom).*

2. *It might be that external merge is available, but no internal merge.*

3. *Counterparts of language in non-human species might allow branching of arbitrary degree, rather than just binary branching.*

4. *Recursion might be available, but without any notion of phase, in which case, if Kayne (2006b) is on the right track, non-human species might lack the possibility of having pronounceable elements be unpronounced in certain contexts.*

5. *Counterparts of human language in non-human species might more generally have no silent elements at all, including in particular elements that are invariably silent. (For example, there might be no contentful pauses in birdsong.)*

6. *It might be that parametric differences are absent within any given nonhuman species (assuming a sufficiently clear way to individuate species). For example, there might be no parametric differences in birdsong (within a given species) comparable to the parametric differences present in human language. (Related to this is the question why parametric variation exists in humans (and why only certain kinds); for some discussion, see Baker (2001: ch. 7).)*

Of whatever nature the relation human / non-human capacities is, the working hypothesis is that anti-symmetry is a human-specific characteristic. Let us bear in mind that the concept of anti-symmetric relation as introduced in Kayne (1994) was applied to phrase structure, more specifically, to the concept of c-command with linearization purposes. A general definition of anti-symmetry would be as follows:

A relation between A and B is anti-symmetric iff it holds for A -> B but not for B -> A

This rules out the possibility of having "mirror" trees, and gives major importance to the side of the tree in which an element appears, in the line of what Di Sciullo (this volume) has posited for Merge. The tree diagram is, then, more than a diagram, it becomes more a substantively correct representation of mind content. This is a claim that underlies Kayne's work, and one that might undermine its biological plausibility, as it is highly unlikely that humans actually have bi-dimensional trees in their minds (more so, but without necessarily accepting the *Quantum Human Computer Hypothesis* of Salmani-Nodoushan, 2008 and Krivochen, 2011).

Tree-like phrase structure and anti-symmetry gives as a result a non-ambiguous system of projection of heads, following Chomsky's (2005) algorithm and Kayne's own LCA (1994). This framework requires assumptions that resemble Adger's (2011) *Self-Merge*, so that there is no relation of mutual c-command between terminals. The general impression is that the arguments are very well developed, but each deductive step requires an extra assumption, which goes against strictly minimalist desiderata.

Furthermore, the distinction between Vs and Ns is taken to rest on the unvalued features that the former have and the latter lack, allowing Vs to expand their projection. Taking Di Sciullo's Asymmetric Merge system, this follows quite naturally, should one accept the rest of premises. However, there is a controversial claim which affects the syntax-semantics interface, namely, (10):

(10) An element can "denote" only if it enters the derivation with no unvalued features.

It is not clear why some unvalued feature (whose nature is not clarified) should matter for a post-syntactic process, namely, referent assignment, which is, widely accepted, pragmatic and not syntactic. So, Ns denote because they have no unvalued features, and they have unvalued features because they are Ns. The argument gets somehow messy here, and the definition of categories rests, to our opinion, on thin ice. The crucial difference between categories is whether they behave "more like" Ns or Vs in different environments, thus deriving the whole system of syntactic categories, and the cases in which, for example, a (phonological) V appears in a nominal environment. This, of course, raises the question to what extent is there a V there, and not merely a phonological signature (in Hale & Keyser's 2002 terms).

The next section (14.11) deals with derived nominals, and the structure in which case is assigned to their arguments via [P]. The argument appeals to "remnant movement", which is a theoretical tool in very much use nowadays, even though a clear, fully-fledged derivation for deverbal N is not provided. However, restrictions as regards generation and distribution of derived nominals are very clearly explained, and a fair amount of (English) data is given to support the argument. The focus is never lost, and the concept of Anti-symmetry is all over the chapter, giving it coherence and unity. A logical order in the argumentation from word-level to sentence-level towards the last sections (14.13-15) clarifies the scenario and enhances the argumentation. The final conclusion, the fact that the concept of anti-symmetry underlies categorial distinctions in language is not innocent and makes quite strong predictions, but precisely because of that it is of scientific interest and worth pursuing.

Howard Lasnik deals with a very conflictive topic for all generative linguists in Chapter 15: the nature and functioning of the generative device. From finite-state Markovian systems, proved inadequate for natural language description in Chomsky (1957). From there on, the assumption has been that natural languages are constituent-sensitive grammars, with a recursive function that allows infinite embedding in every level: phrase structure grammars. Discrete infinitude, in Humboldt's terms, was given at the beginning by the *transformational* component, which left the *base* component (i.e., LEX + PSR of the form [Σ, F], rewriting rules) with *limited* generative possibilities. This transformational approach to grammar can be tracked back to GB, in which two operations, *Satisfy* and *Move* α, recycled EST's *PSR* and *Transformations* respectively. The importance of the transformational component grew exponentially in the EST period, including so-called "generalized transformations", strictly binary operations upon phrase markers. This is connected with the recent concept of Merge (Chomsky 2008), and binarity is now attributed to computational efficiency. However, it is difficult to see how a system (*any* system) is efficient *per se* without a detailed description of the conditions it must fulfill, namely, *interface conditions* in modern terms. There have been recent attempts to derive binarity from interface legibility conditions, but these are well outside the scope of Lasnik's article. The point, which is of extreme interest, is the following, quoting Lasnik (p. 360):

(…) *This solution merely replaces one untenable situation with another: in place of an infinite number of phrase structure rules, one for each number of coordinated items, we have an infinite number of generalized transformations.*(…)

The curious fact is that this "untenable" solution seems to be quite in the line of recent generative research on Merge, that has revived the concept of GT from the earliest stages of Chomsky's thought (e.g., *The Logical Structure of Linguistic Theory*, 1955).

Lasnik's argumentation is clean, clear, free of baroque elements, and very convincing. The only weak spot we find is that the aim of the article is not clear, beyond a detailed exposition of the formal history of the conceptions about syntactic generation. This does not amount to saying that the relation between Markovian and non-Markovian operations and Transformations (now, in terms of Move α, section 15.2) is of no interest, but we find it hard to see the consequences of adopting one or the other conception for the theory of grammar, even the more formal and mathematical-oriented ones. There is also no clear conclusion,

which enhances the feeling of "walking in the fog" that the reader may have, even though, once again, this is a very well-presented and clean fog. The lack of a conclusion does not favor the unity of the article, and this is something the reader will certainly notice.

In chapter 16, Richard Larson examines the syntax-semantics interface His initial thesis is a highly attractive one: given the fact that the computational system projects NUM into *fully interpretable* pairs (π, λ) to be read by the performance systems (i.e., containing no superfluous symbols), the properties of linguistic representations must be searched for in the interfaces. More specifically, Larson's scope narrows to the representation of intensionality in syntax and how that could help clarifying the ontology of linguistic representations. This narrowed focus is perhaps more problematic, since the domain of intensionality is not clear even in semantic theories, and even less so in the domain of syntax. However, it is a natural development of Minimalist thinking that, if a property appears *at* the interface (and not as a product of interface computations), it must be licensed by elements in the syntax. The controversial assumption here is that, in effect, intensionality is somehow encoded in the syntactic representation. Moreover, Larson's claim gets even stronger when he identifies the clause (the phasal clause, more specifically) as the *locus* of the expression of intensionality, focusing on *transitive sentences*, which are said to have a causative *phase head v**. Transitivity opens the door to introduce opaque contexts and revisit what is expressed Leibniz's famous claim "*Eadem sunt, quae sibi mutuo substitui possunt, salva veritate".* The problem here is that precisely the introduction of truth values within a syntactic theory is not a natural deductive step at all. What is more, neither referential (e.g., Mill, Kripke) nor intensional (e.g., Russell, Searle) theories of reference (which are very much interested in truth) go well with a theory of syntax, because their connection is with something outside Language (i.e., a description or an object). However, the step from DPs to factive Vs is a wise one, and very effective indeed. The strong thesis is as follows (p. 368):

Sententialist Hypothesis:
Semantic intensionality ↔ Clausal complementation

Larson is cautious when examining apparent counterexamples, with either ECM or pure transitives, and his arguments are clear and convincing. The argumentation gets stronger with the consideration of cross-linguistic evidence (section 16.2).

A separate mention deserves the analysis of psych-verbs in English, a widely studied topic (e.g., Belleti & Rizzi 1988). According to Larson, there is a bi-clausal structure underlying sentences like (i):

(i) John needs [DP]
This structure would be:
(ii) John needs [PRO ~~to have~~ [DP]]

More generally, **all** ψ-V would license the bi-clausal interpretation (at LF? Are there covert elements **in the syntax**?). It is not clear whether this bi-clausal structure is licensed by a specific element in the syntactic structure, namely, a functional/procedural feature (Escandell & Leonetti, 2000, 2004) or it is an interpretative reconstruction. If that is so, on what basis can we interpret it? These questions remain unfortunately unanswered, but in general do not undermine the argument, which is easy to follow and quite straightforward, accepting the initial premises. The analysis of believe in terms of selecting an existential clause (EC) is particularly interesting, specially because it takes EC to be locative in nature (à la Jackendoff, 1987; Mateu 2000). Even though the boundary / interface between syntax and semantics is not clearly drawn, the argument is specially appealing to those linguists that follow the claim that semantics is actually syntax, mirroring the relation between syntax and morphology proposed by Distributed Morphology.
There is a strong argument in favor of computational views of the semantic component, since it is sensible to constituency. This means that it has some algorithm to recognize them, and solve ambiguities. However, some of the examples (e.g., 57) are quite conflictive, as it is not clear to what extent C-I (or any semantic component the reader has in mind) can parse within the limits of a coined element, if there is not a theory of the syntax-all-the-way-down, as proposed in some versions of XSM and Radical Minimalism. The analysis of examples (59), moreover, explicitly requires mention to a non-defined notion of context, which determines truth value. This section is dangerously built on thin linguistic basis, and rely more on the existence of a strong theory of reference and truth that can be incorporated into a full theory of language. Given the fact that such theory, to our knowledge, is not yet available in full form, the argumentation weakens, specially when the concept of phase is put into play. There is a (re) definition of the notion of phase in the terms the author has introduced so far:
Phases constitute the point where CHL computes potential "propositions" (understood as ILFs) for the ψ faculty, a part of λ.

Of course, positing a faculty ψ within the semantic component is not only theoretically risky, but also not very minimalist, as there is either a great deal of overlapping between ψ and λ's

functions or λ is superfluous altogether. In either case, a revision of the architecture of the system is in order.

In chapter 17, Alessandra Giorgin deals with the representation of indexicality in the syntax, and its relevance for semantic interpretation. Her focus is put on the functional category T, which is apparently crucial for effects at both interfaces: it triggers Agr with the subject in PF and provides a temporal anchor for eventive interpretation in LF. Despite its dependence on the traditional Agreement system (Chomsky 1999, Pesetsky & Torrego, 2000, 2007) and a potential problem with Vocabulary Insertion and whether it needs to be "triggered" by an element in the syntax or it just applies, the proposal is very appealing on the LF side, attempting to provide a principled, third-factor explanation to long-standing phenomena. The analysis of *consecution temporum* is accurate, even though some minor mistakes appear (for example, Latin participles in absolute constructions are not always interpreted as Present time, but in relation to the nearest finite form, be it present, past or future). An interesting point made by this author is that the T features in a clause determine co-occurring elements, like "adjuncts". Apparently, any tensed sentence must be interpreted doubly-folded: with respect to the time of the utterance and with respect to the time expressed in the sentence itself, that is*, utterance T and main event T*. However, when the author says:

"In English, the temporal location of the speaker is relevant for the temporal location of events embedded in complement clauses as well. Consider for instance the following sentence:

(4) John said that Mary is pregnant

For this sentence to be felicitous, the embedded state must hold at the time John spoke and at the time of the utterance."

It is not clear at all what she means by "felitious", a term coined by Austin and intimately related to the first expressions of Speech Act Theory. Felicity has nothing to do with truth, and here neither seems to be clearly relevant. There is also a lack of accurate distinction between Time and Aspect in English, oversimplifying the system, especially in cases of imperfective continuous progressive aspect in subordinate clauses, like the following example:

1. John said that Mary was eating a sandwich.

The lack of distinction between Time and Aspect and the semantic contribution of each at this point, results in an inaccurate characterization of the properties of T, which weakens the argument. In any case, the indexical / deictic value of T must be distinguished from non-deictic Aspect, which expresses the perspective of a subject with respect to an event *already located in the Time continuum* as a point-in-time or as unfolding. The interaction between T and Modality, on the other hand, is made clearly, distinguishing shades of Mod and the effects the interaction with T has in the LF interface level. No classification is provided, however, and, as a consequence, the reader must re-construct the possibilities in his head, which, for us, is not the neatest way of presenting a topic as complicated as Modality and Time.
The interaction between Modality and Time is also functional for the argumentation about C-deletion in Italian subordinate noun clauses with subjunctive mood (part of the argument is valid for Spanish as well) and its contrast with English C-deletion with indicative subordinate noun clauses, which is neat and well-developed. In this section, the distinction with Aspect is drawn a bit more clearly, which makes the argument easier to follow, in contrast to the first part of the chapter. Her provisional conclusion is that both English and Italian obey a generalization about the doubly-folded interpretation of Time, as we have already said. In the domain of Mod there are some risky assumptions, like the absence of subjunctive mood in English (Vocabulary Item or the irrealis meaning altogether?), but nothing is fatal for the argument, should one reinterpret some claims under his own assumptions. The addition of Rumanian and Chinese data makes the argument stronger, regarding the indexicality of T.

Wolfram Hinzen, in Chapter 18, is concerned with the place of semantics in an internalist theory of language. Most accurately, he claims that 20th century semantics have been primarily externalist, which makes it hardly compatible or plausible to be integrated within the biolinguistic program. Biolinguistic's mentalism does not follow the materialistic tradition of recent philosophy of mind (or even, as we have seen above, theories of reference). A virtue of this article is precisely the fact that it problematizes the issue, in clear terms. Even if the traditional problems (e.g., a formal definition of "reference") are still there, Hinzen is not only aware of them but also presents them as the subject of further inquiry. This is a methodological advantage of this article, together with the clarity of exposition and the impeccable logic of the argumentation. Moreover, the reader is presented with an overview of

the philosophy underlying very recent work within the Minimalist Program (Chomsky 2001, 2005, 2008), exposing the strong points as well as the flaws. "*An explanatory approach to human semantics is what we need and lack*" (p. 419) summarizes the first part of the article in a concise and accurate way.

There is a brief case against referential theories of reference, which is primarily based on the fact that language does not pick up a physical object from the world, but, in any case, a Russellian-like description of the object, or a conceptualization of it (in the same vein as Jackendoff's *projected world*). The gap between philosophical semantics and linguistic semantics is clearly exposed and it is clear for the reader that identifying them is, at best, a fallacy.

Of great interest is the discussion about the creative capacity that underlies language, even more so in connection to the arithmetical capacity. Although no unified theory is proposed, the mere comparison is a good signal, since there are currently many important works within the mathematical linguistic field (e.g., Stabler, 1997 et. seq.). Linguistic knowledge, just as mathematical knowledge, is said to be *systematic*, and that is, apparently, a third-factor characteristic. This means that research aiming at understanding the *compositional* nature of complex structures is closer to *virtual conceptual necessity*. However, the author warns, the Minimalist architecture and the role of the semantic component may very well block progress on this front. If the computational system is optimally designed to satisfy the needs of the interface systems, this could license a richly articulated syntactic component, depending on the way in which interfaces are characterized. A very promising provisional conclusion is that *syntax is underspecified*, it formats thought rather than (linguistically) express it. Such a conception, to our mind, is a great leap forward, both methodologically and substantively. Moreover, which we find particularly interesting, the author discusses critically neo-Davidsonian approaches to semantics, which are very much used by Generativists. The virtue of this chapter, then, is not so much to provide answers, which it does not, but to raise very interesting questions concerning the very foundations of the generative-biolinguistic enterprise, and this constitutes a great scientific advance.

Chapter 19, by Cecchetto & Papagno, presents the issue of the role Short Time Memory (STM) plays in language comprehension. STM models are intimately related to the concept of Working Memory (even though not all authors accept them as synonyms), and the amount of information a subject can keep active after having been presented with it. STM can in fact be defined quantitatively as this very amount of information, which, in biological terms, is not

quite a desirable definition since there is no architectural principle involved. However, the authors present theoretical and empirical evidence and the standard model for Working Memory (Baddeley & Hitch 1974, more clearly presented in Baddeley 2007) is made fully explicit. Within the model, the *phonological loop*, which maintains active verbally coded information, is the center of the inquiry. Moreover, the authors present and critically discuss an alternative model, in which sentence comprehension would make use of a different processor, overlapping with Baddeley & Hitch's *central executive* (reminiscent of Fodor's 1983 *central module*). The argumentation is at all points clear, and the inclusion of references to studies of impaired subjects is a good point. The authors, however, assume a model of syntax which they do not fully explicit, namely, the establishment of long-distance dependencies (LDD). No formal / biological definition of "dependency" is provided, nor is a definition of "long distance". The argumentation that is based on the concept of LDD could very well fall apart if a revision of the concept were to take place, as current research suggests. In any case, the consideration of linguistically impaired patients is a point in favor of the article, which describes briefly the conditions in which the experiments were performed with each patient, as well as comments on the results. Methodologically, then, the article has little or nothing to reproach, and the empirical evidence is very clearly presented, in an objective way, as far as "objectivity" can go, at least. Substantially, however, some more definitions could have been provided to strengthen the arguments, moreover, it is not clear what the authors understand as "*appropriate level of syntactic complexity*" (p. 454), for example, which is crucial since their case is based on the concept of complexity and dependency establishment. All in all, the article is a good presentation of conceptions about sentence comprehension and the role of Working Memory, without higher pretentions.

The last chapter is a very appropriate closure for the book. Dealing primarily with Merge and its most primitive version, Berwick is inclined to analyze syntax without semantics, as it would appear in the natural world. By applying Halle's (1997) phonetic system to birdsongs, in which there are no formal features to trigger Movement, he wants to gain further insight in the nature of External Merge from an evolutionary perspective. The assumptions from which he departs are not innocent, in fact, but they are the very same that constitute the backbone of the whole book: there is a specific Faculty of Language, it interfaces with (at least) two other faculties, namely, C-I and S-M, the basic structure-building operation is Merge and, furthermore, it applies in two variants: external and internal. Internal Merge is, in turn, driven by the morphological necessity to get rid of uninterpretable features. The reader is tacitly assumed to accept all these assumptions, which is not a little task indeed, but if he does, the

article flows very straightforwardly. Founded on the FLN-FLB distinction make by Hauser et. al. (2002), the focus is set on FLN, the recursive capacity, its phylogenetic origin from a biological point of view. In this respect, the article formalizes the biological notions in order to present them crystal clear, which is a methodological point in favor of the chapter. Moreover, the strictly biological argument can be very well followed even if the aforementioned assumptions are put aside, and this is another advantage.

DNA encodings are very clearly exposed, and so are operations we could theoretically apply to nucleotides. The article in some points draws a bit too much on biology and the linguistic part of the problem is lost (e.g., 472 ff.), even if the mention of FOXP2 seems to assure the preeminence of language: as the exact role of FOXP2 in the development of FLN is not yet clear, a delicate balance in the exposition should be maintained. This is not the case in some sections (20.1.2, for example, despite its title), in which the genetic enterprise is explained at length, but its connection to the linguistic inquiry is left for the reader to make.

Section II draws more heavily on language, specially on the S-M system and particularly, syllable structure. Even though the externalization problem is discussed with clarity, it is not at all obvious what the connection between Sections I and II is, as there seems to be a gap which has to be inferentially filled. Moreover, the notion of Merge, crucial for the second part of the argumentation, is not formalized, so that the whole chapter is on thin ice should the reader conceive Merge in an alternative way and tries to apply his own conception to the notions spelled out in the chapter: since no specific definition of Merge is provided, such problems could very well arise. In p. 491, the mention to Edge Features and undeletable features seem to point to a Pesetsky & Torrego-like (2000, 2007) notion of Merge, but in a work like this, the concept should have been spelled out with utmost clarity.

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