Quod Homines tot Sententiae — There Are as Many Opinions as There Are Men

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by Lluís Barceló-Coblijn

"But as my conclusions have lately been much misrepresented, and it has been stated that I attribute the modification of species exclusively to natural selection, I may be permitted to remark that in the first edition of this work, and subsequently, I placed in a most conspicuous position — namely, at the close of the Introduction — the following words: "I am convinced that natural selection has been the main but not the exclusive means of modification."
This has been of no avail.
Great is the power of steady misrepresentation; but the history of science shows that fortunately this power does not long endure."
(Darwin 1870, final chapter of the sixth edition of *On the Origin of Species*)

Richard K. Larson, Viviane Déprez, and Hiroko Yamakido (Larson et al. 2010) have at last published one of the most eagerly awaited books on the evolution of human language, in which fourteen lectures have been collected from the *First Morris Symposium on Language and Communication* (held at Stony Brook University 14–15, 2005). The time elapsed between the conference and the publication of the volume is one of the reasons that make the book so interesting and long-awaited. The editors have chosen as their starting point the perhaps most controversial paper on language evolution of the last decade (Hauser *et al*. 2002), which could be secured as the volume's first chapter; very useful indeed, as it is cited and commented by most of the other contributors.

The Roman playwright Terence (Publius Terentius Afer, 195/185–159 BC) once said, "there are as many opinions as there are men".[1] And as soon as one reads the editors' introduction, one begins to feel that the variety of the arguments and points of view therein will be more than "several". Such feeling is indeed confirmed: The reader has in her hands fourteen different voices expressing different theories and presenting original arguments in order to support each of them — an attractive compendium to get an idea of the situation of current research on evolution of language.

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[1] The sentence of the title is thus his and it comes from the play *Phormio* (161 BC).

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The first chapter, thus, is a reprint of Hauser *et al*. (2002, henceforth HFC) and will not be commented in depth in this review, since there already exist plenty of writing about it, including the intense debate consisting of Pinker & Jackendoff (2005), Fitch *et al*. (2005), and Jackendoff & Pinker (2005). In a nutshell, HCF provides an important framework for the study of language evolution. More specifically, HCF propose that "recursion and the mappings to interfaces" is a unique property of human cognition, constituting the only relevant aspect of the "faculty of language in the narrow sense" (FLN) — which, at the same time, is the core element of the "faculty of language in the broad sense" (FLB). This paper has provoked (and still does) many reactions in many different fields of study. It puts forward a provocative hypothesis about the human uniqueness — an issue that worry many scientists —, this time focused on the recursive capacity of human beings to produce limitless hierarchically structured sentences. According to their view, recursion is precisely the special element of human cognition and the element that non-human animals lack. What makes HCF special is that it represents an attempt to integrate in a single field of research — *biolinguistics* — empirical and theoretical issues that concern the biological study of language, the study of cognition in general, and its evolution.

The discussions that HCF has caused among linguists are well known, above all, due to the three different definitions of FLN the reader can find in the text. Whether it means "recursion only" or rather "recursion plus mappings" is something has given rise to a lot of opinions. Both Fitch but above all Hauser deal with this topic in their respective chapters and reveal that the original text "had to be cut to about half its original length" (p. 75), as an excuse for such central ambiguity.

Let me provide a sketch of the four-part structure of the volume:

(1) *Language architecture* (Chomsky, Jackendoff, Fitch, and Hauser);
(2) *Language and interface systems* (Gärdenfors & Osvath, Corballis, and Sperber & Origgi);
(3) *Biological and neurological foundations* (Dor & Jablonka, Piattelli-Palmarini, Lieberman, and Stromswold);
(4) *Anthropological context* (Tattersall, Bickerton, and Bingham).

The editors' purpose is to offer a storyline that provides some order within the chapters which deal with theoretical linguistics, genetics, biology, pragmatics, and so on, yet the borders of the frameworks of each contribution are not always clear, making evident the interdisciplinarity collected here. All of the contributors are reputed scientists, so it is clear from the beginning that this is not an introductory book, but a publication for advanced readers on these matters. In my view, this is both a weak spot (due to the possibility of losing some interesting details) and a virtue (because it forces the reader to take a look outside the traditional topics in linguistics and evolution).

After the editors' introduction and HCF, **Noam Chomsky**'s contribution is first. The text has been available on the internet until very recently for some time already, but the published version is more complete and carefully written.

Chomsky does not disappoint at all, in the sense that one can perceive that characteristic flavor of his style. This also means that a single reading is not enough to grasp it completely. In the beginning, Chomsky brings his point across when he makes clear that he is not in favor of an *adaptationist* view of language evolution. After a historical introduction about the birth of the term "biolinguistics", as in several of his papers, Chomsky invites the reader to consider whether language is the result of adding up "interfaces + recursion". This is important because one can easily follow the concept of language Chomsky has in mind. Unlike many texts about language evolution, Chomsky considers *language* human language only. What's more, he always talks about the linguistic system of *H. sapiens*. Hence, before this hominid there is no language but other kinds of communication systems. Even if the reader does not agree with that, it is of great appreciation that one has not to wait and make continuous suppositions until one finally understands what the author means by *language*. We will see throughout the book that this is unfortunately not a general rule.

Thus, according to Chomsky, it is worth considering the hypothesis that language is a computational system able to improve the cognitive capacity of human mind by means of the emergence of *unbound Merge* within the sensorimotor and the conceptual-intentional interfaces. In Chomsky's view, unbound Merge is a relatively new feature, evolved in modern humans only. Still in the minimalist framework, this mechanism of merging two elements into a new one again and again, is able to account for the structure language seems to show. The times when Universal Grammar was sophisticated and specified are gone. Now it contains minimal specifications to get the same results. The language does not matter because the underlying elements — Baker's (2002) *atoms* — are the same in Lakota, in Catalan, or in Basque. Here is where the reader can perfectly smell the Evo-Devo flavors: Inspired by Jacob's idea about genetics, Chomsky argues that it would be useful to adopt the basic concepts of evolutionary genetics in order to obtain a new picture of the events that affected language throughout its history. Thus, if minimal changes in control gene expression yield completely different biological forms, the same could be applied to language. In other words, linguistic variation would be just the result of minimal changes, being the underlying mechanisms the same in all languages and shared by all *H. sapiens*.

Another important idea, this time borrowed from Alan Turing, is the view of organisms as "living systems" that undergo the general laws of physics and chemistry, so that the possible forms are far from endless. According to this view, the superficial variety of organisms/languages is regulated by a *developmental genetic toolkit*.

This is a clear effort to incorporate some of the most remarkable ideas and theories from evolutionary biology to the studies about the evolution of the human faculty of language. According to Chomsky, there are (at least) three basic factors in language design: genetic endowment, external data, and principles not specific to language (Chomsky 2005). In this respect, I personally like Ott's (2007: 4) addition of a fourth factor that "concerns the embedding of the Language Faculty within the mind — that is, the way it interfaces with other components". It makes clear the biological frame in which language ontogenically develops, that is to say, *H. sapiens'* brain/mind and not any other.

All this is framed in an *internist* theory of language, a position one is almost forced to assume if one has — even if only generally — a formal conception of the mechanisms that structure language. The *Cartesian* stand Chomsky has taken traditionally is obviously defended here: In the beginning, "it was a language of thought". A student of evolutionary studies or comparative psychology would have immediately asked: If so, what about the rest of hominids? Could *H. heidelbergensis*, *H. neanderthalensis*, *H. erectus*, and so on, make use of that language of thought? The answer is "no", according to the final part of Chomsky's essay. At least *not* as *H. sapiens* does. Chomsky defends the idea that the conceptual system of our ancestors was different from that of non-human animals. But the inclusion of the rest of the members of the sub-tribe *Hominina* is almost never taken into account in the theories about language evolution. Chomsky adopts a skeptical view about a secondary and independent language of thought (p. 55), but, if our (above-mentioned) ancestors had a different, non-animal conceptual system, could they have had that kind of mental language? Bickerton is partly right when he notes (pp. 199–200, see below) that Chomsky and HCF almost never take into account the rest of the members of the genus *Homo* in their hypothesis (with the exception of some commentaries by Fitch and Hauser related to speech — but not language — and Neanderthals). That the rest of *Hominina* could vocalize (on their way) is almost certain, since all other primates can, and nothing on the fossil record indicates the opposite. So, in which place, as regards cognition, should we put those hominids? Another thing that is not completely clear is that this initial period for the language of thought could be misunderstood as a period of silence. In short, it is not clear at all whether there really was a moment zero for that silent language of thought only, or whether it was parallel to the vocal and/or gesture communication system those hominids could make use of.

Later on, Chomsky speculates and gives an example about a theoretic hominid called *Prometheus*[2] (p. 59) who, as the first member of his community endowed with unbound Merge, would have taken advantage of all its potential. We all, full-fledged modern humans, would be his descendents. Like *Prometheus*, we can make use of "duality of semantics, operator-variable constructions, *unpronounced* elements with substantial consequences for interpretation and thought" (p. 59; emphasis added). Here is maybe where the prose becomes a little bit messy. It is clear (it should be clear) that this is a metaphor; *Prometheus* was alone in his "internal linguistic" condition, so there was, at the beginning, no place for "unpronounced elements". What's more, it *was* a language of *thought*. Chomsky himself often cites Ian Tattersal, a paleoanthropologist who, among other interesting reflections, has argued that "the arrival of new behavioral or technological innovations has *not* tended to coincide with the appearance of new kinds of hominid. This actually makes considerable sense, for the only place in which a novelty can appear is *within* species" (Tattersall 2004: 22). In other words, it takes several generations, *within a species* for a novelty to be "discovered" and "exploited". It would not be necessary to state this (the fact that *Prometheus* did not

[2] Curiously, in Ancient Greek *Prometheus* means "forethought"; his mythological brother *Epimetheus* means "hindsight", literally "afterthought", but in the manner of a fool looking behind, while running forward.

exploit his potential language of thought), if we were not aware that sometimes we find comments on Chomsky's words because they have been taken literally. His example clearly does not help much to clarify his view. A radical reading of this passage could come to the conclusion that *Prometheus* produced unintelligible utterances to his own parents. This kind of literal reading can be found in this book in Bickerton's chapter (p. 202).

Next, we find **Ray Jackendoff**'s essay, which is, along with Lieberman's (see below), one of the most transparent in the presentation of the hypothesis he puts forward. Clearly, the author has a different theory of language and, hence, a different view of the evolution of this cognitive faculty. Jackendoff argues that a good strategy in order to explore the features of language and its evolution is *reverse engineering*. He first classifies the elements that compound language in four different departments: (1) things necessary for language, but that did not require genetic changes (e.g., lungs); (2) innovations in the human lineage useful for language or its acquisition that serves other general purposes (e.g., theory of mind); (3) aspects of language that are unique to humans, that are exclusively for language or its acquisition that required a change of the pre-existing primate structures (e.g., vocal tract; in this regard, the author agrees with Lieberman in that the vocal tract evolved *for* language); and (4) something altogether new and unprecedented in the primate lineage. The last one would be the right place for FLN, according to HCF (pp. 64–65).

Jackendoff underlines the fact that we need to have "analyses of other capacities to compare them to language". The problem is that there are no such analyses, just a few or largely abandoned ones (e.g., on music and on visual perception). In Jackendoff's opinion, other strategies like the comparative method advocated in HCF are insufficient. Departments (3) and (4) could be null, that is, "nothing *special* needed for evolution of language", though it is not his bet.

According to Jackendoff (p. 67), there are two kinds of theoretical architectures for language, syntactocentric (Chomsky's proposal) and parallel (Jackendoff 2002). The difference lies in the way both conceive the lexicon formation. For the former one, each item is an association of phonological, syntactic and semantic features, all of them embedded into a syntactic structure. So, syntax makes possible the connection of thought with vocalization. In this proposal, recursion is inserted between the interfaces. For the latter proposal, there are "independent principles of combinatoriality in phonology, syntax and semantics, each restricted to its proprietary structure" (p. 67).

The first and biggest problem Jackendoff sees in HCF's proposal is that "the whole generative syntactic system and the mappings to phonetic and logical form have to spring into existence more or less out of the blue" (p. 69). For HCF, recursion would be in department (4), whereas for Culicover & Jackendoff (2005), recursion is an element also of visual cognition, that is, belonging to department (3).

Jackendoff's proposal is original in the sense that the semantic/conceptual structure is the product of a combinatorial capacity, but at the same time independent of syntax. And here is where we find the vaguest part of his hypothesis: "[T]hought was highly structured in our ancestors [i.e. at least, the rest of the

members of the genus *Homo*]" but "they couldn't express it" (p. 71). This kind of silent thought is not only the result of a combinatorial capacity, but its units are also liable to further non-syntactical combinations. According to the author, this is a preadaptation and its product, combinatorial thoughts, useful to be "shared". This is possible because the parallel architecture allows us to establish links between the phonological and semantic interfaces, without intervention of syntax. Hence, our ancestors would have a proto-lexicon (more or less à la Bickerton). As we will see in the review of Bickerton's chapter, the proto-language itself — in this case, the proto-lexicon — lends to the production of multiple vocalizations. Finally, syntax, "the capstone innovation", would have appeared in successive and gradual stages. The reader is referred to Jackendoff (2002) in order to learn more about the even more gradual stages the author proposes therein.

I think it is easy to grasp the great difference between Chomsky's view and Jackendoff's. Notwithstanding the final stage, the emergence of syntax is not described therein, which leaves the reader with a feeling of incompleteness. Back to the possibility of a kind of thought that is combinatorial, useful for sharing, but that couldn't be expressed at the beginning, the whole thing leads us to the next question, what did make possible to share this kind of inexpressible thoughts? Its usefulness for sharing?

The possibility is widely accepted that the vocal tract was prior to the modern capacity for language. However, most linguists are reluctant to concede a sophisticated vocal system to other hominids. Jackendoff concedes vocalization to hominids, in order to explain the emergence of linear order as a precursor of language (p. 71). But surely vocalization goes further back in time. As well as Chomsky, Jackendoff presupposes a surreptitious stage of silence where thought is already propositional but cannot be expressed. Looking at the rest of the primates, this seems an anomalous possibility. In any case, this is an interesting chapter that invites the reader to think about this plausible architecture of language.

In the next chapter, **W. Tecumseh Fitch** talks about something that is of great necessity in the debate originated by HCF: the meaning of recursion. Indeed, it was missing in HCF, since it was not published in a linguistics journal but in a journal for general science. The point is that although it could come as a surprise, recursion has three (really) different meanings, depending on the field of study — computer science, linguistics, or meta-mathematics. It is a fact that differences concerning the meaning of recursion arise immediately when linguists talk to mathematicians. They simply do not talk about the same thing. More or less the same can happen when one of the interlocutors comes from computer science (CS), though the differences may not be so strident. According to Fitch, recursive functions typically take "their own past output as their next input". On the one side, when defining recursion, as a term used in CS, we see that it "is one which calls itself" (the keyword is the verb *call*; p. 76); on the other side, in linguistics, recursion "has the property of self-embedding, that is, in which the same phrase type appears on both sides of a phrase structure rewrite rule" (p. 79). Clearly, here the keyword is *embedding*. This difference is crucial to understand why recursion is *different* in both fields, and in fact it is so crucial that a recent paper con-

cerning the nature of recursion focuses precisely on this (Arsenijević & Hinzen 2010): It seems that recursion in linguistics necessarily implies embedding of elements. Finally, in meta-mathematics, there is a long tradition in the study of recursive functions, something that in some cases could surprise non-mathematicians, since, as Fitch noted, there are some iterative functions, and even non-recursive functions, which are included in the set of recursive functions. Fitch notes that mathematicians are more concerned with computability and not with whether a function recalls itself or implies embedding. These seem to be simply very different things and in fact a new label, *computability theory*, replaces the old terminology.

Another factor related to these definitions is whether or not there are tree representations behind the structures or outputs resulting from this operation. In the CS case, Fitch argues the answer is "no", since nowhere in the software or the code is there any implicit tree diagram. This might be something people just draw as an aid. Hence, the tree is not in the code. However, this is not true for linguistic theory, where the tree diagram is important and explicit. This remark helps Fitch to bridge another famous debate on whether tamarins and starlings have recursion (or lack it), arisen by virtue of the results published in Fitch & Hauser (2004) in which they put to the test cotton-top tamarins in order to see whether or not they are capable to process different kinds of grammars — a Finite State Grammar (FSG) and a Phrase State Grammar (PSG). This issue is brought up again by Hauser in this volume, too (p. 97).

The most extended interpretation (mine included, I confess) was that it was a test for recursion. What's more, it could have even been interpreted somehow as a kind of experimental proof of the *pumping lemma,* so many different readings were possible, depending on the reader's background. Anyway, immediately a great debate arose (e.g., Kochanski 2004) and other scholars put to the test humans (Perruchet & Rey 2005, who found that humans are not that good at learning a PSG) and starlings (Gentner *et al.* 2006, concluded that starlings can process a context-free grammar; but van Heijningen *et al*. 2009 disagree). Contrary to these interpretations, Fitch argues that Fitch & Hauser (2004) was not a test for recursion, that the word 'recursion' was even not mentioned in the paper. Fitch shows quite convincingly that both A^nB^n (PSG) and $(AB)^n$ (FSG) grammars "can be represented recursively". Nonetheless, it is also true that the picture of the grammars in Fitch & Hauser (2004: 378) clearly shows a classical diagram of a center-embedded grammar, so that the risk of misinterpretation was more than high.

What I find particularly interesting and of great value in Fitch's contribution is his aim to put some order within the terminology of the field, and at the same time his effort to build bridges to other fields of science. Abstract concepts like *recursion* are sometimes the seed of sterile debates simply because different people have different conceptions of the same term. I'm sure that researchers who work every day in interdisciplinary labs will appreciate this kind of work.

Next, in the fourth chapter, we find **Marc Hauser**'s contribution. The text takes a personal tone and, right from the beginning, the author advises the reader that these are his opinions (and not HCF's). First of all, Hauser defends his work done

on animal communication, in response to Bickerton's (2007) "puzzling point" minimizing the work Hauser, Fitch, and Chomsky have done in this particular field (pp. 92–93). Next, the author rejects the interpretations of HCF as a paper that "flats out animal communication", recalling that he is still working in animal communication (p. 93). And so there are three more sections defending that: (i) the sensory-motor system as a homologue or analogue with other animals' system is a hypothesis; (ii) HCF's hypothesis is *not* recursion only; (iii) he is not a closet minimalist. By the way, an interesting last comment is that there exists a last chapter of this saga on the web: The reader can find another paper — this time the authors' order is "Chomsky, Hauser and Fitch (2005)" (p. 95) — where a response to the second part of Pinker & Jackendoff (2005), which focus on the Minimalist Program is provided. I'm sure that the reader interested in such debate will appreciate this *last* release — it is five years old, yet written at the time these communications were made.

Finally, Hauser deals, once again, with the distinction between FLN and FLB, what it is useful for, and why it should be taken into account. The author defends the usefulness of this strategy of putting the elements into one set or into another. The status of each element, says the author, has not to be permanent: As experimental research offers further empirical evidence, an element of FLN could be moved into FLB. Let us remind the reader that Fitch *et al*. (2005: 181) noted that FLN "could possibly be empty if empirical findings showed that none of the mechanisms involved are uniquely human or unique to language". The last part is a reflection about the above-mentioned experiment with cotton-top tamarins (Fitch & Hauser 2004). Hauser recognizes that such a grammar was not the best choice, since many other mechanisms "could underlie this competence" (e.g., the mechanism of counting argued for by van Heijningen *et al*. 2006).

Although here we will find neither a new theory nor a new hypothesis, the reader will find some answers to those questions that arise in reading the cited papers. It may not be the most spectacular essay of the volume, but these new pieces of information about Hauser's intentions and posterior reflections provide the reader with a human perspective that is missing so often in the scientific literature.

In the second part of the book, **Peter Gärdenfors & Mathias Osvath** make a contribution concerned with the evolutionary stage of the hominid mind, when there was yet no language (neither oral nor mental). The authors talk about a time prior to the emergence of symbolic thinking. They agree that *H. sapiens* is the only animal whose use of symbolic language has been proven. Thus, Gärdenfors & Osvath aim to deepen our knowledge of the forces behind language evolution. Their hypothesis focuses on the Oldowan culture (for that matter, see also my comments below on Bickerton's contribution) for the first stages of which, as they clearly state, there is no recognized author. This constitutes a problem because at that time there were many species of hominids. The authors follow Plummer (2004) on that matter, but the reader has to be aware that not only *H. habilis* and *H. erectus* could have made that lithic industry, but also *Parathropus*[3] according to

[3] It comprises at least three species: *P. aethiopicus*, *P. boisei* and *P. robustus*. This genus is be-

an examination of the hand of parathropines (Susman 1988). This is important because Gärdenfors & Osvath's hypothesis turns on "the Oldowan culture", unaware that this could affect both genera *Homo* and *Paranthropus* with very different results.[4] Thus far, the Oldowan culture becomes the ecological niche for some of these hominids, which could act as one of the driving forces towards symbolic thinking. Nevertheless note that, for Gärdenfors & Osvath's hypothesis to be viable, one has to assume that the same *force* has very different consequences on genetically very close co-existing species (a point, thus, indirectly in favor of evo-devo theses).

Their hypothesis is as follows: *Prospective cognition* precedes symbolic thought and is based in two kinds of thoughts, *cued* mental representations (CMR) and *detached* mental representations (DMR). The former refer to present objects, whereas the latter refer to non-present objects or events. DMR could be, according to the authors, one of the novelties of the frontal lobes, since these parts of the brain have been linked to activities like planning and fantasizing. Again, arguments such as these have to be taken very carefully. The current role of the frontal lobes could differ from their role in those days.[5] DMR are related to Hockett's *displacement*, though slightly different (p. 105), and imply the existence of an *inner world* (the collection of detached mental representations). DMR seem to be the basis of the ability "to envision various actions" which, according to the authors, is a requirement for *planning* (p. 106).

Gärdenfors & Osvath note in passing that even chimpanzees show the ability for planning when preparing tools for fishing termites. This would be a case of *immediate planning*, whereas *prospective planning* must have a detached representation of *future needs*. Although the authors do mention *H. habilis*, they bet indeed for *H. erectus* as the hominin showing such mental abilities (always following Plummer 2004). According to the authors, this hominin would have been able to carry lithic tools, to divide the labor within the members of the group according to their aptitudes, and to hunt and gather Although Gärdenfors & Osvath do not mention this, it's highly likely that *H. erectus* hunted micro-fauna. In any case, their view is that prospective cognition was a necessary evolutionary novelty.

This is the part of the paper I find more well-grounded and fitting best with paleoanthropological data. However, the linking to *H. sapiens*'s language is still remote. The way in which Gärdenfors & Osvath build the bridge is by appealing to the notion of *cooperation* as the element favored by language (p. 111): Symbolic language favors cooperation about future goals. I have no problems accepting that *prospective cognition* could have been an important element in the evolution of modern human cognition, but to resort to abilities like "manipulation of attention" (proposed by Tomasello, 1999: 131) or "sharing visions" does not

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lieved to be a parallel line to the genus *Homo,* which reached an evolutionary cul-de-sac and died out. Other scholars believe they all three are should be included within the australopithecines.

[4] Additionally, recent findings support the suspicions that the genus *Australopithecus* already made and use tools for scavenging 3.39 million years ago (McPherron *et al*. 2010).

[5] In this line of argumentation, it has been argued that *H. floresiensis* could have had some kind of sophisticated mental abilities (Falk *et al*. 2005). See below, on Bickerton's essay, for similar observations.

explain completely how it is possible that language has the syntactic, phonological, or semantic structure it has. Note that in this context, *symbolic communication* is still not "virtually equivalent to language", as Tattersall says. Thus, the gap from *H. erectus*'s way of communication to current modern language, understood as our cognitive faculty, is too broad to be covered simply by arguments on the usefulness of sharing symbols through communication.

The seventh chapter is **Michael Corballis**' paper. Corballis tries to reconcile Chomsky's and Bickerton's theories (as we will see, Bickerton explicitly says these are simply incompatible). As many authors in the book, Corballis takes as his starting point the extant hominins of 2 million years ago (mya), explicitly *H. rudolfensis* and *H. habilis*,[6] and the lithic culture of that time, that is, the Oldowan culture. Another trait he considers important is bipedalism, something that characterizes the genus *Homo*. Let me update a bit on this point: Recent work on that matter claims that bipedalism is not an innovation of the genus *Homo*, since a previous ancestor, the *Ardipithecus* (4.4 mya), was already biped (see especially Lovejoy (2009) and Lovejoy *et al*. (2009a, b). Additionally, *knuckle walking* is different in gorillas and in chimpanzees, a fact that suggests that the typical locomotion of these two great apes are (independently appeared) derived traits of these species rather than an ancestral trait, thus suggesting a common bipedal locomotion in our distant ancestors.

Corballis thinks that both Chomsky's and Bickerton's theories can be reconciled "if it is supposed that language itself evolved gradually, but it was based in the first instance on manual gestures, with gradually increasing vocal involvement" (p. 115). I do not see clearly how this can fix the problem, since the point is that both Chomsky and Bickerton basically agree about the fact that vocalization and its physical apparatus were already part of our ancestors, before modern language was a reality. So, it is not a problem of the modality of the output, but a different vision of the way the computational mechanism underlying syntax evolved.

It is said that the classics do not fail; and, Corballis, as other authors in this book, resorts to some classic paleoanthropologic scenarios in which (i) savannah replaces dense forests; (ii) there is an increase of brain size, "driven by selection for such cognitive abilities"; and (iii) a protolanguage (in Bickerton's sense) increases its sophistication until it reaches the current state (p. 116).

As in any other adaptationist hypothesis, this process is gradual and takes place always through natural selection. Corballis argues that this is "reasonable", but he does not explain why. In fact, the author appeals to a famous Chomsky quote: "It would be a serious error to suppose that *all* properties, or interesting structures that evolved, can be "explained" in terms of natural selection" (Chomsky 1975: 59, p. 117 of the present volume; emphasis added). This and other commentaries have been interpreted as suggesting that Chomsky is against, or that he rejects, natural selection as a driving force in evolution (e.g., Johansson 2005: 161). The emphasized word *all* in that sentence, in that context, clearly points out that some properties can be explained by natural selection, while

[6] There is controversy about whether or not both hominids belong to the same species.

others cannot. In any case, Chomsky is not original in this way of thinking, as we can see in the introduction of this review, since Darwin himself would have agreed.

Corballis assumes with Bickerton and Chomsky that modern language (in the author's sense, but, a necessarily *orally* externalized language) appeared "with or even after *H. sapiens*" (p. 116). Such "after" is quite interesting, since one may wonder how, under normal conditions, it is possible that any human of any land can acquire any human language if this capacity did not arise from the beginning in the same African population group. According to Corballis, the answer is straightforward: "[I]t was not language itself that emerge with *H. sapiens*, but rather the capacity for autonomous speech" (pp. 115–116). This would be possible since, according to Corballis' hypothesis, syntax would have appeared in a gradual process while, the expression channel for the output was manual, rather than vocal.

One has to acknowledge the continuous effort Corballis makes in trying to integrate his ideas and theories to current paleoanthropological data, and this can be easily detected throughout his work. It is a difficult task, since the field continuously evolves as new findings are published in many fields of study. However, sometimes the author takes as evidence for his hypothesis some data — for example, the *hypoglossal canal* — which Corballis knows to be controversial (p. 117). It is even more surprising when the explanatory power of this physical trait has been put into question by several scholars specialized in speech evolution (e.g., Lieberman 1999, Fitch 2000). For Corballis, it is reasonable to assume that modern speech mechanisms were "incomplete" in Neandertals and the common ancestor they have with modern humans (p. 117). The author argues that a piece of evidence in this direction is the human *FOXP2* gene, which he views as a novelty of the species *H. sapiens*.[7]

Corballis' own gestural hypothesis is largely grounded in the discovery of mirror neurons, which fire in both hand and mouth movements, and in the recognition of these movements in conspecifics. Such neurons have been detected in the F5 area of monkeys' brains, but still not in humans — though there is a lot of indirect data suggesting their presence in our brains. The author notes that grasping movements even "affect the kinematics of speech itself" (p. 120), which is taken in support for the motor theory of speech perception (Liberman *et al*. 1967). According to this theory, speech sounds "are perceived in terms of how they are produced". It goes without saying that this theory is quite controversial, though it has, in my view, some good points.

Up to here, the evolutionary theoretical background is quite robust, in general lines. Further, Corballis speculates about a possible scenario, always taking into account the *mirror system* as the basis for the further development of language. Thus, according to him, communication was basically gestural. In Corballis' hypothesis, bipedalism is a crucial element for the freedom of hands. The author further speculates that, as the technology of tools develops, language and tool-making are in conflict, due to the fact that both activities require the use of

[7] A fact refuted in the last publication on this matter (Burbano *et al*. 2010). Some commentaries around this gene are made below, on Lieberman's contribution.

hands. Such an adaptationist story, however, is forced to exaggerate the role of an activity like tool-making and the time those hominids (all members of the crowd) should invest during millions of years, in order to make of a cultural activity a driving force, to such an extent that it finally acts on the genome and its subsequent development. Another weak point is that, even if this was really so, there is no reason why no other hominid followed such path. When *H. sapiens* left Africa, an encounter with *H. neanderthalensis* at the region of Kebara took place. Both cohabited the region during thousands of years; they are believed to have been in contact, that they possibly had trading relationships, and now we know that they were able of eventually interbreeding (Green *et al*. 2010). But, even before we knew this last incredible piece of genetic data, the archaeological and fossil information already suggested that the two hominids were not that different.[8] Instead, Corballis contends that "the final conversion to autonomous speech may have been an invention (Corballis 2002) or, as suggested above, it may have resulted from the *FOXP2* mutation (Corballis 2004)" (p. 123). Again, we find reduction to a single factor and overlooking other species within the same context.

The next chapter is written by **Dan Sperber & Gloria Origgi**, and it covers an aspect of language which is quite interesting: pragmatics. The authors show that contextual factors play an important role in the way we interpret an utterance, and how it is possible that even sharing the same code does not guarantees that we all process that utterance likewise. The reader not familiar with Sperber's *Relevance Theory* (e.g. Sperber & Wilson 1986) should know that it largely builds on the Gricean philosophical theory of language. Sperber & Origgi confront two models: the code model, based on the fact that sentences are sound and meaning pairs, and the inferential model, which states that the inferential information we get from the context is relevant for our final representation of the utterance.

Although, according to the authors, both models agree that languages are codes with a recursive grammar (p. 125), the inference model includes explicitly what the authors call *naïve psychology*, which includes the ability to attribute mental states to others. This is because humans seem to "spontaneously interpret one another's behavior […] as belief-guided fulfillment" (p. 126). Sperber & Origgi point out the importance of the continuous inferences we make in our communication acts, how they are intervened by the context, how communication can fail if the communicator cannot fulfill her intention "by making it manifest to the hearer" (p. 126) — "or to the beholder", in the case of sign languages, we could add.

The authors acknowledge that the manipulation of mental states can be useful, but they observe that this mechanism is "cumbersome"; instead, overt communication, where both actors (communicator and addressee; it is interesting the use of *communicator* instead of the classic *emissor*, maybe because one can be *emissor* without voluntarily being *communicator*[9]) "are intent on comprehension"

[8] Remember that both hominins share 99.5% of the genome.

[9] But see Seyfarth & Cheney (2003: 147) for a different notion of communication, where even unintended acts are taken as active parts of communication (more in tune with Claude Shannon's mathematical theory of communication): "Although the frog has no goal of

and hence, the transmission of information becomes successful at low cost. An interesting aspect of the authors' proposal is that in their model, "a fragmentary coding is sufficient", contrary to the code model which has to encode the information unambiguously.

The authors defend the idea that humans have an inferential model and animals do not. Animal codes would be closer to the code model, since both communicator and receiver must share the code. Any difference would lead to potential errors. Recall, for example, the alarm calls in vervet monkeys. According to Sperber & Origgi, this is an example of a genetically transmitted code. It is their opinion that such codes are counter-adaptive (p. 127). I do not agree with that, since this statement is made from a strong anthropocentric point of view. Alarm call systems cannot be counter-adaptive, since so many different extant species have this kind of communication system. That our system looks much better to us, to our human logic, is a different issue. If extant species do have an alarm call system, it is because it has been beneficial.

Anyway, Sperber & Origgi observe an important difference between genetic systems and inferential systems: The former does not easily allow the incorporation of new elements, whereas the latter "does not require that the communicator and audience have the same semantic representation of the utterance" (p. 128). What's more, an "ad hoc meaning is contextually constructed" (p. 128). I think this is an important observation in order to differentiate some well-known animal communication systems from the human communication system. This fact increases the sophistication of the system; however, as the authors observe, it does not "protect" the users from potential misunderstandings (p. 129).

Finally, Sperber & Origgi propose an imaginary situation in which the communicator has a more sophisticated system than the receiver, and it seems that communication does not fail. Here, the communicator could represent the first generation endowed with a syntactic device (and this reminds us of Chomsky's *Prometheus*), a device which allows the holder to go beyond the coding possibilities of the hearer. This does not represent a problem for them to communicate, while the contrary would not be true. Next, the authors affirm that the holder of such a new device and "her co-mutants [i.e. subsequent generations] communicate more effectively than other members of their community" (p. 130). Again, in my view, the authors confound the fact of having devices more complex syntactically and semantically, with the fact of being better at communication. Complexity does not always mean better results, especially in the light of ecology. The communicative systems of our ancestor and related species of hominids worked effectively enough for their communicative purpose, and the proof is that those hominins could occupy an ecological niche for thousands of years. In my opinion, their systems were not that bad, at least not for their communicative necessities. They were different, possibly qualitatively different, but good enough for the recipient, their minds. Apart from anything else, as Tattersall (2004) notes, the

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communicating to the bat, communication occurs nonetheless, as bats take advantage of a lucky accident and extract useful information from a signal that evolved for entirely different reasons".

place where a novelty can appear is *within* species and usually it has to pass a very long period of time until the species discover its potential, so that, when they can exploit it, a considerable group of members of the community — if not a majority of them — already share such novelty.

In any case, Sperber & Origgi's contribution shows the power of inferential systems when working along with a linguistic system. It also informs us that this is an important aspect that should be covered by both theoretical and empirical biolinguistic research. I think the reader interested in communication and its evolution will appreciate this line of work.

The next chapter is **Daniel Dor & Eva Jablonka**'s contribution. The authors present an original hypothesis, quite different from what's proposed in the rest of the book. It has a well-grounded background in genetics, and it is framed in the light of evo-devo, though they pay more attention, in fact, to the development of the phenotype. Dor & Jablonka use a special notion of language: According to them, language is a collective *invention*, which "culturally evolved before its speakers were specifically prepared for it on the genetic level" (p. 136). In their particular notion, language is something that, in its last stage, "was already out there, as an object for learning" (p. 146).

Dor & Jablonka argue that this has been possible because "the social world evolved to the point that collective inventions became possible" (p. 136). Although the authors do not make explicit what would be required to properly speak of "invention", they insist that there are inventors of language, and that not everybody can become an inventor — the truth is that the level of abstraction — required to grasp the notion which is behind the "invention" — is sometimes not so clear. Thus, the key in their hypothesis is the genetic and neuronal *plasticity* of the human condition. This term refers to the "ability of a single genotype to generate, in response to different environmental circumstances, variable forms of morphology, physiology, and/or behavior" (p. 137). It seems that in every species there are individuals who have more plasticity than others, and this factor gives them the possibility to adapt to changes in the environment. The authors give the example of Kanzi, the bonobo, who grew up within special conditions so that pre-existing components of his developmental systems were reorganized (p. 137). The authors note that this is a complex process in which other mechanisms and elements play particular roles:; for example, the *attractors* are "stabilizing end-states towards which the system seems to "strive"" (p. 137). Further, *canalization* consists in the adjustment of developmental pathways by natural selection and, as the authors note, the opposite to *plasticity* to some extent.

In this contribution, Dor & Jablonka offer a new scenario for the evolution of language. The truth is that it is highly speculative; the authors make a lot of assumptions, almost without citing where their conclusions come from. Thus, we have to assume that their thesis is based on the notions just mentioned, *plasticity* in particular. It is the authors' opinion that the members of the community are inventors. An important point is that they always talk about humans, and never about other hominins, so that we do not exactly know whether the term "human" is mentioned abstractly or whether the entire language evolution took place in modern humans only, as it would be if we read the text literally. In any case, Dor

& Jablonka start from the point that humans try to solve problems and therefore invent new words. Interestingly, some "problems emerged as systemic consequences of the development of language" (p. 140). On the other hand, "the community gradually sophisticated its world-view adding new linguistic categories". *How* is something not explained. It seems that those categories were invented by an inventor and learned by the hearers. Following this process, "language developed into a system of rules" (p. 141). Again, we have to suppose the system was invented thanks to plasticity. A rule system leads to a major stability, and this, at the same time, led to an increase of plasticity.

Suddenly, the authors change the topic, and they talk about the evolution of languages, the emergence of slang and jargon in linguistic communities and how this helps to social secrecy, something that makes hazy the concept of *language* they have been talking about. When the authors refer to linguistic changes caused by phenotypic variations, they choose examples from phonology or speech, never from syntax. This is possible because it seems that the rules, in their scenario, simply were invented and then suffered a process of "social negotiation and struggle" (p. 142).

The reader will find that the notion of language is quite different, it has to be *learnable*, but not everybody can learn it, since there is variation in plasticity. However, people who could not learn the more complex system could perhaps learn part of it, at least to reach some level of comprehension (p. 145).

Reading this chapter, neither the temporal frame their hypothesis covers nor the species to which it applies are ever clear enough: Sometimes it seems that humans are *H. sapiens*, but sometimes this is doubtful. Moreover, I missed some bibliography to ground their many assumptions and speculations in the second part of the paper.

In the tenth chapter, the reader will find **Massimo Piattelli-Palmarini**, a veteran in this field. In this contribution, he summarizes the history of science (especially biology) in relation to linguistics, to show how linguistics has changed the way languages have traditionally been considered and observed: from collections of treasures to natural objects. After each part of his discourse, Piattelli-Palmarini draws a conclusion in the form of a lesson. So, when he talks about the first steps taken by the people behind *string theory*, the lesson is that linguists have to encourage empirical research and pursuit of new ideas, even when some of these ideas could have at first sight a "dimly" conceptual content; or that we should not put any limit to the level of abstraction, if such is required. Piattelli-Palmarini revises the parallels between linguistics and other scientific fields. Thus, the reader will be reminded that at the beginning there were languages and philology, until the notions of I-language and E-language appeared (still controversial, by the way). The former paid attention to the tacit knowledge of language and this changed the study language evolution, since the object of study was now a cognitive capacity rather than a prescriptive, to some extent artificial, grammar. The author guides the reader through the ages in which some linguists decided to pay attention to other disciplines in order to get new ideas, which could help them explain the structures underlying natural languages. It was then, tells us Piattelli-Palmarini, that syntax took a central position: first the generative gram-

mars, and then the minimalist program. The author then focuses on *edge features* (Chomsky 2008) and the operations they carry out, stressing the importance of this concept as well as the notion of *phase* ("self-contained derivational domains, characteristically nested one into the other, that are simultaneously sent to the two [sensori-motor and conceptual-intentional] interfaces"; p. 151).

Piattelli-Palmarini agrees about that recursion is an essential element of human cognition, and especially of language. He terms "the age of specificity", when generative grammar, the modularity of mind, visual cognition, and the Chomsky hierarchy were established. He also observes that "[p]ossibly the right formal characterization still eludes us, or possibly there cannot be any such purely formal characterization, because of inherent bio-evolutionary contingencies" (p. 153), or in other words, *principles not specific to language* (also known as the third factor). Piattelli-Palmarini does not believe either in gradualist or functional explanations of language evolution (or organisms, for that matters); as a biologist, he rather contemplates "the biological picture" as quite complex, "multi-faceted", and therefore he believes that biolinguistics must incorporate new ideas and models (p. 157).

In conclusion, Piattelli-Palmarini has summarized the essentials of the last forty years of research on language evolution. The notions and conceptions are quite clear, and so are the goals: the understanding of the biological principles and structures underlying the cognitive faculty of language. If this supposes to change the whole traditional paradigm, so be it.

Philip Lieberman is also one of the veteran authors in language evolution. Quite impressively, Lieberman defends the same hypothesis after forty years, concerning the possibilities of the Neandertal vocal tract (Lieberman & Crelin 1971). In Lieberman (2002) we find an original hypothesis, built onto the knowledge accumulated in these years. Lieberman's approach focuses on *basal ganglia,* subcortical structures that, far from being old or static during the time, have evolved in a particular way in humans. In passing, Lieberman proposes a new term, *reiteration*, which subsumes the properties of *recursion* à la HCF, but it "is expressed outside the domain of language when we change the direction of a thought process as well as in seemingly unrelated activities such as dancing" (pp. 163–164). Besides the interest of the idea — no doubt important in the debate concerning the limits of the range of action of *recursion* in human cognition —, the question is whether or not such a new theoretic term is indeed necessary, given the close resemblance with *recursion*: both entail nested hierarchical structures (p. 164). One difference, mentioned above, is that iteration works outside language. A second difference, according Lieberman, is that "iteration instead generates the sentences and semi-sentences that can be observed in real life by inserting relative clauses, […] and other elements without the torturous and often arbitrary operations of traditional generative theories" (p. 164). The operations which entail *reiteration*, following Lieberman, are thus linked with the reiterative function of the basal ganglia. This fits smoothly with his theory of language evolution, strongly based on sensory-motor control. Like in his last contributions, Lieberman argues in favor of leaving behind the classical Broca–Wernicke model, since it is inaccurate and does not fit with current neuropsychological data, which show that aphasias

always present subcortical damage (not just cortical), often in the basal ganglia.

An additional argument in favor of his approach, argues Lieberman, are the new pieces of information available on the FOXP2 gene (p. 171). Lieberman is clearly interested in the function of *FOXP2*, since it has a strong relationship with the control of orofacial muscles and, so it seems, with a decrease of the affected person's IQ. Until very recently, it was believed that *FOXP2* (i.e. the human version of the gene) was a recent innovation in the modern human genome, but according to Krause *et al*. (2007) it was part of the Neandertal genome too — and if so, it was also part of the ancestor's genome of both *H. sapiens* and *H. neanderthalensis* — while a second independent analysis cast a shadow of doubt, since it obtained different results, and the conclusions were that Krause and his collaborators' analysis were contaminated (Coop *et al*. 2008), a new analysis of the gene with a new methodology show new results that are in favor of the presence of the *derived* version of the gene in both Neandertals and modern humans (Burbano *et al.* 2010). In other words, language evolution theories based on motor control arguments like Lieberman's or Corballis' (see above) should take this important factor into account when inferring any relation between control of speech and language. In this respect, Lieberman can overcome this potential theoretical problem — still unknown at the time of the conference — saying that a modern superior vocal tract and the modern speech producing anatomy is present in *H. sapiens* only (p. 173).

A final argument Lieberman provides is the problem of *choking*, which affects every modern human being. According to the author, this problem must have a trade-off, otherwise — as in any adaptationist theory — "there would have been no reason for retaining the mutations that resulted in a human S[uperior] V[ocal] T[ract], unless the neural mechanisms that confer the reiterative properties of speech were in place" (p. 174). But there are many things in the biological evolution of organisms that will always scape from our (human) "logic" way of reasoning if we always think in terms of trade-offs. For example, the presence of the totally useless appendix, whose inflammation will affect the 7% of the world population, according to Brunicardi *et al*. (2004); or wisdom teeth, absent in a low percentage of fortunate people only, provoke more troubles than trade-offs, and they are still there.

In any case, Lieberman is one of the few theoreticians of language evolution who offers a hypothesis which takes into account not only purely theoretical linguistic arguments, but also data from neuropsychology, paleoanthropology, and evolutionary studies like genetics. One may or may not agree with his adaptationist view of language evolution, but one must admit that Lieberman has built a very strong, well-grounded hypothesis, which fits very well with current empirical data from many scientific fields.

The twelfth chapter is devoted to language acquisition and genetics. **Karin Stromswold** presents the results of the work she has been doing on language acquisition and genetics. In short, she compares the heritability factor (h^2) of language in a population group of twins. Assuming that an organism is the result of the phenotypic expression of its genes in an environment and that this process is partially mediated by both perinatal and post-natal environments, the compa-

rison of homozygote vs. heterozygote twins is useful to determine the influence of the environment. Following this procedure, researchers can focus on the heritable genetic factors only. It is a fact that all typically developing humans acquire the basic morphosyntax of their language, "but perhaps some adults fail to master rare linguistic constructions" — like some examples we find in the technical linguistic literature.

Traditional texts on evolution theory talk about the fitness of some traits as the driving force in organic evolution. When Stromswold and colleagues look at the possible relationship between greater linguistic precociousness or proficiency and reproductive success, they find several interesting results: Consistent with a genetic "stoppage",[10] Stromswold's study shows that firstborns with more siblings are less likely to be language-impaired than latter-born children or children with fewer siblings. In other words, language proficiency could have been seen as something qualitatively important when mating took place. However, Stromswold recalls that previous studies (Alwin 1991) found even more interesting results, and contrary to the reproduction success prediction, "children's vocabulary, verbal SAT and IQ scores are inversely correlated with the number of siblings and spacing of siblings" (p. 179). And finally, it is known that women with more education have fewer children, and later than other women. In my opinion, the lesson here is that a theory of language evolution cannot be built on fitness arguments only. These are for sure important and probably have played a role in it. But it would be all but accurate to exaggerate its role in a process of high complexity as organic evolution.

This is a difficult area of research, since participants are not easy to find. In addition, Stromswold and collaborators had to determine which aspects or components of language should be the targets, since "one cannot merely determine the heritability for overall language" (p. 177). The selected targets were syntax, phonology, and lexicon. Although the author does not even mention it, at first sight, this procedure has a ring of *modularism* à la Fodor (1983), which could surprise scholars who don't feel so comfortable with this hypothesis. Notwithstanding, I think it could also be considered as an indirect test for a strong *modularist* view of language. Stromswold and collaborators found significant genetic overlap for these components, indicating that, *possibly*, "some of the same neural circuitry is necessary" for the smooth running of two or more of those components. Stromswold and colleagues wonder whether those components co-evolved or are partially parasitic on others. However, as Stromswold warns, "it could just be happenstance" (p. 178). Therefore, Stromswold's team has carried out a *Perinatal Environment and Genetic Interaction* study. They employed an enormous amount of linguistic and non-linguistic data, information that covers extensive periods of the twins' lives. The results suggest that there is a high genetic overlap for language and oral motor skills as well as fine motor skills (p. 185). Their interpretation is that this "could reflect shared neural circuitry for tasks that require complex motor control". But overlaps do not end here: linguistic and social abilities also overlap. Again, a plausible explanation about shared neural circu-

[10] A conscious family planification due to evident genetic impairments. Thus, families with such impairments would have had less descendents.

itry is offered. Next overlap is even more remarkable and surely will catch the reader's attention: Stromswold and collaborators found a large genetic overlap for phonology and syntax scores, greater than for lexical and either syntax or articulation scores.

Thus far, the whole evolutionary picture has become even more difficult to draw. The enormous task carried out by Stromswold and colleagues is more than welcome to a scientific discipline, *biolinguistics*, which is in need of this kind of research in order to revise the general theory along with empirical data. Otherwise, it would be doomed to an endless dialectical spiral of arguments — something useful by itself, for sure, but only to some degree. Stromswold invites us to rethink HCF's hypothesis about FLN. Two options suggest themselves in order to explain this large overlap for phonology and syntax: Either HCF is wrong or another element should be included into FLN, an element that participates in both phonology and syntax. It is not the first time we find new applicants for the selective group of FLN; for instance, there are well-grounded reasons to consider the inclusion of *Duality of Patterning* into this set (Rosselló 2006), a feature that precisely shows this dual character. I'm pretty sure that this new perspective of language evolution will be appreciated by readers interested in human evolution, whatever their training, for its potential in constraining linguistic proposals.

Ian Tattersall is a reputed paleoanthropologist, who makes a brief but interesting contribution, summing up what is known about the evolution of the sub-tribe *Hominina* until our days. In anthropology, symbolic thinking is one of the most important concepts they use to refer to modern behavior. Chomsky cites this expression as if it was Tattersall's, though it's not. Maybe what it is original from him is making it equivalent to modern language. Anyway, the phrase is sometimes used as certainty of having language, something really controversial (see also my above remarks on Gärdenfors & Osvalth). The hypothesis for human evolution presented by the author is the so-called *Out-of-Africa*, the opposite of the *multiregional* theory. The former tells us that all modern humans are descendents of a small group of African early *H. sapiens*. The latter theory maintains that *H. sapiens* is the result of a continuous interbreeding of different species of the genus *Homo*. More precisely, according to this theory, there was just one species, hence the possibility for interbreeding. Although there are still some scholars defending it, genetics clearly favors the "Out-of-Africa" view. Tattersall also assumes splitting off in two different species, *H. neanderthalensis* and *H. sapiens*. That would mean no interbreeding at all between these two hominins. As I have already noted, it has been found that this was indeed possible (Green *et al*. 2010) but just occasionally (the two species never merged into one), a fact also known as *introgressive hybridization* or simply *introgression*. As already suggested, this fact is relevant for accounts of language evolution that focus on a particular capacity of *H. sapiens*, that do not also pay attention to the presence of that capacity in *H. neanderthalensis*.

In order to expose the emergence of modern cognition, Tattersall embraces the possibility of *co-option* or *exaptation*. Several elements already extant in the mind/brain of our ancestors would have been co-opted, reused for new or additional tasks. Such a mechanism makes 'the work' easier for the emergence of new

evolutionary functions. A second mechanism the author takes into account is the so-called *byproduct*, also known as *spandrel* (a term coined by Gould & Lewontin 1979), which is a biological novelty that depends on structural constraints, not on a functional role. The confusion of both mechanism is not uncommon, as if they were the same mechanism.

Thus, Tattersall applies these evolutionary mechanisms to language. In his scenario, speech emerged well before symbolic thinking (as indexed by archaeological record). Language proper and modern cognition would have evolved in *H. sapiens* only. Tattersall reconstructs the evolutionary path of the most representative members of the genus *Homo* through the type of industry each of them is associated with. The association of a determined industry to a species is something difficult and risky because most of the time tools are found in the absence of fossil remains, so that the attribution to a particular species can be a tricky matter. Besides, the author talks about the refinement of the tools and the growth of the brains,[11] arguments which are no more decisive: Recent research on such matters has shown that humans are not special at all; hence, regarding the nerve cell average of their brains, modern humans are equal to other apes and monkeys within the mammalian *order* of primates (Azevedo *et al*. 2009). But primates do stand out when compared with other non-primate species. Thus, relating large brains with intelligence is risky. It seems that the type of interconnection of the different parts of the brain plays a more relevant role — indeed this feature has been put forward to speculate about the possibility that *H. floresiensis* had a kind of modern or at least sophisticated cognition (Falk *et al*. 2005; see also above on Gärderfors & Osvath's chapter). In general, I really think that contributions like Tattersall's should be taken into account in biolinguistics, since they talk about the evolution of the organisms within which, the ancient communication system/language was embodied. The problem is that the picture of the evolution of primate species continuously changes, as new fossils, archaeological items, and genetic data are gathered, forcing researchers to keep an eye on developments.

Next author is **Derek Bickerton**, who does not beat around the bush: His theory and Chomsky's are incompatible (in spite of Corballis' allegations to the contrary, see above). As we have seen when discussing Chomsky's contribution and HCF, Bickerton reproaches them for saying very little about the paleoanthropological context where language emerged. What's more, he argues that the most crucial questions one could ask Chomsky, Fitch, and Hauser (and advocates) are: (i) how, where, when, and why took place the integration of the elements of [$_{FLB}$ FLB [$_{FLN}$ FLN]]; and (ii) why other species close to humans, having some of those elements, had never developed any communication system like ours.

Although the *"how, where, and when"* are logical and legitimate questions, to answer *why* a change took place wherever is nearly impossible to answer. For instance, the process of mutation obeys several factors such as migration, genetic drift, and, in the case of humans, cultural factors that could also play a role. Bickerton's question seems to be grounded on the basis that natural selection is

[11] The average *Australopithecus'* brain was 450 cc; whereas *H. habilis'* (2.2 mya) was 660 cc, earlier *H. erectus'* (i.e. *H. ergaster's*) was 850 cc, and later *H. erectus* achieved 1,100 cc.

the driving force in evolution. It is true, but it is not the only driving force, as Darwin repeatedly said (see the introduction of this review). The second question also has a deterministic flavor. The evolutionary way followed by different species can be imagined in a three- (if not four-) dimensional space, where species, regarding some traits could be very close, but their routes to achieve that close position where completely different. On the contrary, two species can share several traits and reach different new abilities or features, even if they cohabited the same ecological niche.

More than a half of the text is a criticism of Chomsky's hypothesis, and the rest is devoted to present his own hypothesis. As we have seen, Chomsky's hypothesis has a relative high level of abstraction, which sometimes makes it difficult (to some extent, simply not possible) to adapt to current paleoanthropological and neurobiological empirical data. Maybe, this is a common feature of all testable hypotheses in their first stages. Before introducing his criticism, Bickerton sums up quite well both Chomsky's and HCF's stand regarding the role of recursion in FLN. However, in my view the author fails when he says that "[i]n other words, this 'quite different', fully developed human conceptual system formed a necessary prerequisite for the *emergence* of recursion" (p. 201, emphasis added). Bickerton himself mentions, both at the beginning (p. 199) and at the end of his chapter (p. 210), that according to HCF, recursion could have been used in other domains. Therefore, the conceptual system must not be *per se* a *prerequisite for the emergence* of recursion. The nature of this *emergence* is quite obscure, an aspect never clearly explained by HCF. The particular characteristics of this mechanism has led many to think that, if real, the evolutionary biological explanation could be articulated around the concept of *exaptation* (Tattersall 2004) or even a *spandrel* (Barceló-Coblijn, in press). The conceptual system may be an element that intervened in its emergence — one among others (see Arsenijević & Hinzen's original proposal 2010 for recursion as an epiphenomenon of the interaction of linguistic interfaces) — or it may be not. This point notwithstanding, his revision of the concatenation mechanism regulating anaphoras and sentences (pp. 203–204) invites reflection; though, intuitively speaking, we possibly may find an explanation not so far from that which seems to work for sentences like *Mary saw the man walking to the bus station* (with three possible interpretations), that is, "computationally plausible principles of generation and minimal search" (see Chomsky's contribution, pp. 46–48).

It also deserves mentioning that the vision the author has of language and humans still drags along a strong anthropocentrism, bestowing language and humans the power of "effective command and control over all other species" (p. 200). From a biological point of view, this is, at least, an exaggeration, alas, quite common in linguistics across the board. According to this point of view, Bickerton proposes an adaptationist and gradualist scenario. In his opinion, this proposal is "more consistent with, and can be more readily integrated into, biologically and paleontologically based accounts of the overall process of human evolution" (p. 206). The author, as well as Gärdenfors & Osvath in the sixth chapter, singles out one of the traits of language, *displacement* (see Hockett 1958; though Premack 2004: 303, argues that chimpanzees can also make use of displacement), and considers its essential role in animal communication (like in bees and ants).

He proposes that it could be the propelling force, metaphorically speaking, for ancient hominids to make use of *recruitment*, that is to say, the ability of gathering individuals to reach one target. This is really quite interesting in fact because we see how basic communicative properties are shared by evolutionary very distant organisms. The channel is different, but the use of *displacement* is quite the same. The problems arise when the author tries to integrate his proposal "into, biologically and paleontologically based accounts of the overall process of human evolution". According to Bickerton, the process of language evolution would have begun more or less 2 mya. On the one hand, the author does not designate any species: At that time, we can find *H. habilis*, early *H. ergaster* (1.8 mya) or even the *Parathropus*, as mentioned above. Bickerton cites the Oldowan tools, the first made 2.7 mya, which have been associated to *H. habilis* under statigraphic arguments only[12] (Tattersall points out too the uncertain authorship of such tools, p. 195). Other *H. habilis* remains have indeed been found together with such Industry. Up to here, although Bickerton has moved the discussion at least 2 mya back into the past, it is still not clear which hominid could uniquely satisfy his contention (given the more than probable cohabitation of several species of hominids and great apes).

Another argument that the author borrows to make possible the integration of his hypothesis with paleoanthropological studies relates to the evidence of consumption of carcasses of mega-fauna by hominids of that time, as "the richest source of food", required to sustain greater demands of energy. However, according to the evidence, this activity was occasional and fortuitous. To see in the carcasses the "richest source of food" is to overlook the many resources those hominids had, as recollection, micro-fauna hunting and other daily available rich protein sources (e.g., termites and other insects). As in other proposals about language evolution, too much weight has been put onto a single argument or force (in this case, *recruitment*) by Bickerton.

In the final part of his contribution, Bickerton compresses a lot of information and arguments that have recently been put into question, like the well-known argument over brain growth (also used by Chomsky and other authors in this volume) — which should be reconsidered in light of recent findings about the number of neurons in primate brains (see Tattersall's discussion, above).

Finally, the reader will find **Paul Bingham**'s essay. Bingham is a molecular and evolutionary biologist, and he has contributed a new perspective as well as a new theory of language evolution. Like Bickerton, this author strongly trusts, right from the start, in natural selection as the explanatory mechanism for language (p. 211). The starting point is the classic adaptationist stand: "[V]arious constraints impose adaptive trade-offs, resulting in elite execution of one task at the expense of merely serviceable (or negligible) capacity for another" (p. 211). In other words, in order to develop language, modern humans have lost something along the way. As any other adaptationist theory, it focuses on one element that is

[12] See footnotes 3 and 4. These tools were found tens of kilometers away of the next hominid, and this was not a member of the genus *Homo*, but the *Australopithecus garhi*. The controversy still remains and has been revived by recent evidence of tool-use for eating with a datation of 3.39 mya (McPherron *et al*. 2010).

raised to the category of propelling force of the process. This time, it is the "conflicts of interest" that gets center-stage, on the grounds that even "exchange of information is apparently directly determined by conflicts of interest" (p. 212). The author continues: "Design information builds organisms. Organisms replicate this design information by replication" (pp. 212–213). This process generates competition and alternative forms are lost along the way. However, we do not know whether or not some of those lost forms could have been slightly better to some extent and their loss due to factors other than direct competition like, for instance, by chance. Moreover, Bingham assumes that organisms use two strategies to assist the mechanism of replication: personal reproduction and assistance of close kind. Additionally, there is the reflection of some basic ideas from Hamilton (1964a, b) as well as Dawkins' (1990) theory of the 'selfish gene'. In Bingham's view, DNA, though unconscious, is intervened by "Natural Selection so that it builds organisms that tend to behave exactly as they would *if* they were controlled by genetic design that *did* have such conscious interests" (p. 213). Hence, it seems we have the basics of the adaptationist recipe.

Bingham's approach is more connected with the study of communication in a broad sense than with the study of the faculty of language *H. sapiens* developed. His view is that non-human animals "arguably parse highly dynamic, hierarchically nested combinatorial information sets of stupendous complexity" (p. 215), as in combinatorial movements. This is enough for the author to think that they really have all the requirements for language, "but in a more modest scale", since "no other factor than the solution of conflicts of interest problem nor new capability […] needs precede evolution of symbolic communication" (p. 215). According to this theory, there are two kinds of actors: *cooperators*, which are non-kin individuals that cooperate until exceeding the costs of cooperation, and *free-riders*, who fail to pay the initial cost of cooperation and hence cooperation does not evolve. Bingham then develops a theory in which these two factors interact in such a way that they have to solve their own conflicts of interest, which inevitably affect all of them, as it happens in any social network.

The key, for Bingham, resides in the mastery of "elite projection of conspecific threat remotely", which would have appeared within our ancestors. This kind of remote threat produces an "enormous reduction in costs" (pp. 218–219). Bingham's particular effort of integration of his theory with anthropological data focuses on the ability of elite throwing objects that only *H. sapiens* seem to have developed. One could immediately argue that, if this were true, there appeared a new ability (contrary to what the author said at the beginning of the paper). But this would not be completely true, since other apes and monkeys can throw objects somehow. The critical point is degree of mastery. Humans have an expertise on these matters, whereas non-human animals roughly throw whatever they can throw. This improvement would have required, by 2.3 mya (i.e. the period of convergence of several *Hominina*; see footnotes 3, 4 and 12), the redefinition of shoulder, pelvis, and the foot — a process completed roughly by 1.8 mya, when *H. erectus* appeared.

In favor of his theory counts the fact that subsequent experimental work has indeed paid attention to Bingham's hypothesis (among others), and it seems that there are reasons to believe that rhesus monkeys do understand the threat

that implies the throwing action by humans, and therefore these researchers have concluded that "the capacity to throw did not co-evolve with psychological mechanisms that accompany throwing; rather, this capacity may have built upon pre-existing perceptual processes" (Wood *et al*. 2008: 360).

In any case, the jump from this kind of ability to language seems to me excessive, to say the least, to be accounted by only one factor (*conflict of interest*). The simplest fact in biology seems to obey more than one factor. Reducing so much the explanatory elements gives us no clue about the emergence of psychological and neuropsychological capabilities that really differ among primates, or even among mammals and other *orders*. Even more so when the concept of language is so diffuse and confused with speech, as in Bingham (p. 221). If there is anything that finally has been differentiated in linguistics, it would be the core concepts of *speech* and *language*. What is surprising is that, once we have seen such fuzzy use of these concepts, Bingham accuses linguists of ignoring the very famous *H. erectus* endocast in which Broadfield *et al*. (2001) argued to have finally detected an incipient modern form of the Broca's cap (p. 222). Besides the contentious current status of the classic Broca–Wernicke model (see above, on Lieberman's chapter), I'm sure that scientists like Falk (2007) or Lieberman, who have dealt with this kind of empirical paleo-data and have enormously contributed to the understanding of language evolution, would have a say on this. Although I concede that not every linguist knows about it, maybe the issue is, once again, the concept of a 'linguist' different scientists may have in mind.

As I said at the beginning of this review, there are as many theories on language evolution (in a very brief period of time) as there are people. In this volume alone, there is almost an original hypothesis for each author (certainly, Hauser, Chomsky, and Fitch agree on the basics, but some differences can also be detected). Contrary to appearances, this proliferation of hypotheses could turn out to foster a new, synthetic approach more in tune with current paleoanthropology, anthropological genetics, and evolutionary biology.

I really think that language evolution theory has to leave behind this obsession in finding the element that makes humans special (the key factor, in Lieberman's words). The combination of elements that make us humans human is that key factor. This is neither popular nor spectacular, but it is more in tune with current evolutionary studies. If scholars more or less agree that language is a complex object, then let's think about it in complex ways, taking into account as many variables as possible in order to enrich the general picture. Neither the scientist who proposed natural selection as an evolutionary force nor modern studies on evolution and development ever stated that this is the only mechanism of evolution. The integration of Evo-Devo ideas into the biolinguistic field is warmly welcome, but they have to be integrated within the general evolutionary theory of species (hominids and primates, in particular) — along with a well-grounded linguistic theory, as Bickerton and, above all, Lieberman have tried. Much the same can be said as regards neurobiological theories of language evolution. It is true that there is still much to discover about ourselves and our hominid past, but this should not prevent us from aiming this synthesis — quite the opposite.

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