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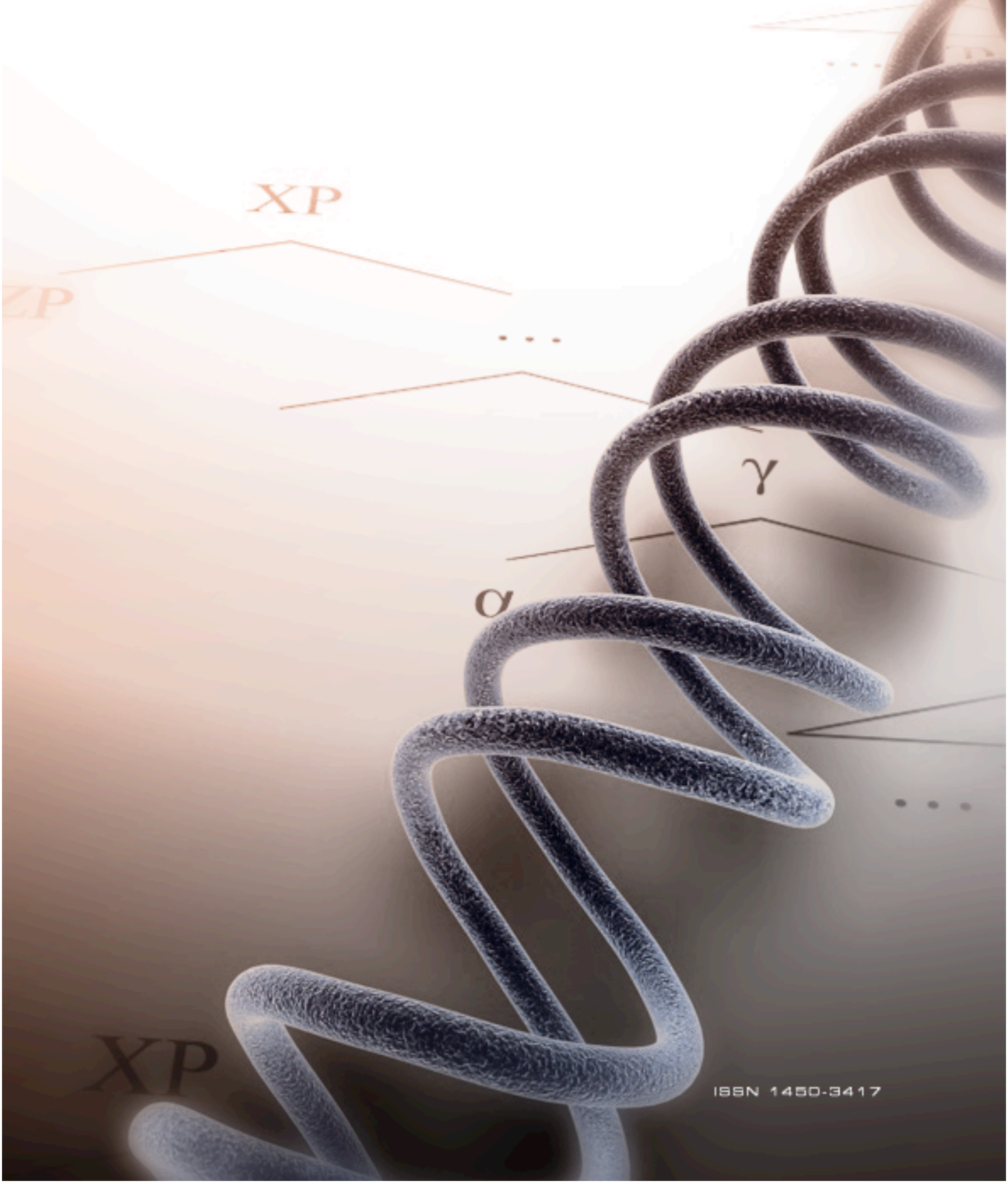

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★ EDITORIAL ★

Upcoming Changes in *Biolinguistics*

Kleanthes K. Grohmann & Cedric Boeckx

This issue marks the completion of the 5th volume of *Biolinguistics*. In these five years, we have managed to produce a traditional journal-like publication — free of charge for readers and contributors alike, and open to all. The "traditional journal-like publication" is a journal that comes in volumes, one per year, with each volume further divided into regularly appearing issues. In the case of *Biolinguistics*, our four issues per volume correspond to the four seasons and have been scheduled to appear at around the same time each year.[1] (As an exception, since *Biolinguistics* first appeared in late 2007, the first publication was a single-issue volume.) Throughout volumes 2 to 5, we have been fortunate enough to receive contributions on interesting topics which we could thematically group into special issues, and for space and time reasons these counted as double issues, a practice we expect to continue in the future. Aiming for state-of-the-art research and the dissemination of cutting-edge ideas, within a variety of topics and even (sub-)fields in biolinguistics, we also try to ensure quality publication through a proper peer-review process for submitted articles and briefs.

However, this is about where the similarities to a "traditional journal-like publication" stop. We don't have a publisher behind us, we don't even have a financial support system that would allow us to contract professional editorial staff, so in sum, we don't have much in terms of professional infrastructure that would allow us to emulate a "traditional journal-like publication" further. We've tried our best over the first few years but have reached our limit, most pressingly in terms of time. Leaving aside the review process itself, each issue costs us around 100 hours of working time formatting, proof-reading, and editing the material. Our resources do not allow this to continue, so we will be implementing some changes from volume 6 onwards, starting in 2012:

(1) We urge all authors to follow the *Biolinguistics* style sheet to a dot, as we will simply be unable to spend all these extra hours formatting their contributions for them; this concerns in particular references for which we have a style sheet but which most often are not formatted or even listed properly, and which subsequently take up a fair amount of our copy-editing time. In the past we have routinely added references that the authors mentioned in their texts but failed to list in the reference sections, or corrected them. We simply won't be able to do this anymore.

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[1] This contrasts from other open-access journals that publish as articles are accepted and finalized for publication, where each article constitutes a separate issue, for example.

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(2) We urge all authors to ensure that their contributions to *Biolinguistics* are fully proof-read by native speakers and conform to general, high-quality academic writing in English concerning style, grammar, punctuation, etc. (especially as specified in the style sheet) — and in turn will not spend all these extra hours proof-reading and copy-editing contributions for authors.

Failure to comply with either (1) or (2) may constitute grounds to reject a fully reviewed, revised, and even otherwise accepted piece; that is, acceptance for publication in *Biolinguistics* is contingent on complying with the editorial policies of the journal. This will be stated clearly in due time on the journal's website at http://biolinguistics.eu/index.php/biolinguistics (or simply biolinguistics.eu).

We are aware that these policies may lead to some visual changes in *Biolinguistics*, namely, that the homogenous style and layout of the journal will not be as consistent as it has been up to now. But we believe that this is a small price to pay for a free-for-all journal without major professional infrastructure and financial support. It also reflects the collective spirit of the community a little better, which means that in order for *Biolinguistics* to be successful as a free, open-access journal, everyone needs to contribute.

On this note, we would like to remind our readers that the entire project can only be successful if the growing community of biolinguists chips in, that is, authors vis-à-vis submitters and peer-reviewers. It is an obvious fact that we depend on the submissions of articles and briefs to the journal if we want to reach our goal of publishing relevant cutting-edge and trend-setting research on a regular basis. But we also need to be able to rely on more reviewers who accept our requests for review positively and promptly, and then deliver in a timely fashion. As an online journal, we have the advantage of being able to publish very soon after acceptance, but it requires some effort on all parties involved.

Continuing this 'community spirit' theme, we would appreciate if potential reviewers would not approach *Biolinguistics* the way they may deal with more traditional journals in linguistics, but make it a priority to deliver on time reviews, following the practice in other fields.

On the positive side, we are delighted to announce that, thanks to the hard work of Charles Reiss and his team, especially Melinda Heijl, we will very soon be able to offer a LaTeX style sheet for *Biolinguistics* as well! It will be a close enough mirror of the rather intricate Word-formatted style sheet, which will also look a little different from volume 6 onwards.

Lastly, we're happy to announce our newly forged cooperation with EBSCO Publishing to disseminate the journal further. We are also participating in DOAJ, the Directory of Open Access Journals (http://www.doaj.org/doaj?func=openurl&issn=14503417&genre=journal), and we recently applied for inclusion in Thomson Reuters products (among others, for the ominous 'impact factor'). Basically, we will continue to look for ways of improving the journal, and we welcome any suggestions and help in this domain.

It's time to turn the page, and very soon the calendar. Happy reading and a great start into 2012 — and thanks for all your support!

A Biolinguistic Approach to the Vocalizations of *H. Neanderthalensis* and the Genus *Homo*

Lluís Barceló-Coblijn

This paper revisits the old question about the possibilities and aptitudes *H. neanderthalensis* had for vocalization. New evidence will be discussed that moves the discussion beyond traditional interest in the presence and interpretation of the fossil record and its comparison with the closest species to *H. sapiens*, like chimpanzees and gorillas. An interdisciplinary perspective on the analysis coupled with information gathered from neuropsychology, genetics, and comparative psychology will prove useful for obtaining a new vision in biolinguistics, so that neurocognitive activity becomes important thanks, above all, to the comparison with other species. Finally, it will be argued that it is still plausible to accept the hypothesis on the formation of a vocal capacity prior to the cognitive faculty of language, and independent of it, so that Neandertals were probably able of vocalizing voluntarily, with communicative intentions and in a sophisticated way.

Keywords: air sacs; *Homo (H. neanderthalensis)*; hyoid bone; vocal tract; von Economo neurons

1. Introduction

The issue about language and the communicative possibilities of Neandertals is not new at all. Indeed, it has been revisited from time to time. Intuition has traditionally told us that, at the moment when both hominins — *H. sapiens* and *H. neanderthalensis* — met, they had to be able to establish some kind of communicative interaction. This kind of thought has led people to take for granted that Neandertals possessed a cognitive faculty of language like modern humans do. Although this is possible, I argue that, contrary to that first impression and to

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I am grateful to the members of the *Group of Human Evolution and Cognition (EVOCOG)* for comments and support. I specially thank Antoni Gomila, Jeroni Tutusaus, and Manuel Vega Reynés for helpful comments and the audience of the *VSSoL International Conference on Language, Nature, and Cognition* (2010). I thank Txuss Martín, Cedric Boeckx, and the audience from the Universitat Autònoma de Barcelona, who attended a talk on this matter, for an interesting discussion and comments. I would like to express my gratitude to Michael Corballis and a second, anonymous reviewer for their careful comments which have improved the final version of the text. The remaining errors are mine only. This project was supported by the BES-2008-003607 grant and the project FFI2010-20759 from the Ministerio de Ciencia e Innovación (Spain).

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what could apparently be most likely ("what common sense tells us"), it is not so obvious that *H. neanderthalensis* could communicate as we do. Moreover, I will put forward the idea that vocalization is a capacity *almost* independent from the capacity we humans have to create linguistic constructions that can be conflated to semantic macro-concepts — though it could have co-evolved along with this latter capacity. Our approach takes into account pieces of evidence from several areas of science in an attempt to contribute with a much wider vision than the one we can get exclusively from the hyoid bone. I will apply a quite classic methodology in traditional philology: If one has not got any written proof of a lexical form that could have existed, while at the same time being certain of its existence on the basis of other indirect evidence (e.g., linguistic comparison or reconstruction), then a symbol (*) is applied to the alleged word until a document is found that can prove it. Instead, if one thinks that there is not enough evidence, or that it is not conclusive, a symbol (*X*) will be applied. In the case of a positive conclusion, the symbol will be (√). Let's see some samples not directly related to vocalization:

Trait	**H. neanderthalensis**	**H. sapiens**
Bipedalism	√	√
Chin	X	√
Hyperboreal adaptation[1]	*	X

Table 1: Examples of traits and their evaluation, where (√) means 'positive/present', (X) means 'negative/absent' and (*) means 'probable though not confirmed'.

Thus, we can see how 'bipedalism' is clearly inferable from the osseous morphology. Hence both species have a confirmation symbol for this trait. Instead, 'chin' is confirmed in *H. sapiens*, but discarded in the *H. neanderthalensis* case. Finally, 'hyperboreal adaptation' is clearly absent in modern humans, but not fully rejectable in the case of Neandertals. So I apply an asterisk because there are signs that this is a trait of this species, though more proofs and research are needed in order to finally confirm it.

Next, I will review some traits I consider particularly relevant regarding human vocalization, and I will try to extrapolate them to the Neandertal case. The idea is to draw some conclusions that help me offer a more complete and enriched picture of the issue. Traditionally (and understandably), attention has specially been focused on the analysis of the fossil record, that could reliably indicate a linguistic capacity in this species, so close to *H. Sapiens* from an evolutionary point of view. As we will see, the issue has become more complex than previously thought. Thanks to interdisciplinary work, even some apparently

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[1] This trait refers to an adaptation to cold climates. Thus it is argued that, leaving aside the possibility of using fur covers, Neandertals were somehow adapted to cold regions, as sapiens are to tropical regions. The idea of Neandertal resistance to cold weather has recently been reinforced by the finding of a late Mousterian archaeological site at Byzovaya (Russia) near to the Arctic Circle (Slimak *et al.* 2011). The Mousterian industry has traditionally been linked to Neandertals.

insurmountable obstacles, like the fact that brain does not fossilize, can be overcome in a minimal (but substantial) way. Data from different scientific fields come together in such a way that different areas become linked, so that the issue can be studied from different, but confluent perspectives. In what follows I will also use the comparative method. Comparison between species provides us with clues about what is inferable from other species through homologies and convergences. However, I am aware of the limitations of this method and of the necessary caution in order to deal with the presence or absence of a particular trait in an extinct species.

The list of the traits that will be object of analysis in the present work is as follows:

Trait	***Brief Definition***
Larynx	It is an organ for phonation, an air conduct and a sphinteric mechanism which goes from the base of the tongue to the traquea.
Vocal Tract	Animal cavity where the sound, produced in its source (the larynx), is filtered.
von Economo Neurons	Neurons characterized by its spindle shape and only present in the anterior cingular cortex, in the disgranular prefrontal cortex and in the frontal insula.
Lack of air sacs	Absence of laryngeal air sacs which were connected to the hyoid bone.
Mirror Neurons	Neurons that fire when the animal performs or observes an action by a conespecific.
FOXP2	Gene and protein related to specific language impairments.
Region 2-4 kHz	Region of the sound wave curve that contains relevant acoustic information for humans.

Table 2: Traits that have been related to speech and vocalizations

As we can see, traits in Table 2 go well beyond the traditional way to set out the problem of vocalizations in extinct hominids. So, I have bet for interdisciplinary work, importing information from genetics, neuropsychology, and acoustic models. This implies that the physiology of the vocal apparatus loses part of its leading role, favoring an approach that takes into account the role of some brain regions and, more concretely, of some neurons related to particular aspects of human cognition, language and communication. Thus, what matters for vocalization is not just anatomy, but also the neural areas involved in its control.

This work is structured as follows: In the first part I will review the literature dealing with larynx reconstructions and the great (still enduring) debate around this cavity of the vocal apparatus. So I will consider both the discussions about the lowering of larynx as a trait characteristic of modern human beings and the discussions about the role of the hyoid bone in the larynx, as well as the possible autapomorphic character of the morphology of this bone in modern humans, as opposed to the rest of great apes. I will also discuss the hyoid bone's orifice where the so-called air sacs are connected, which is lacking in humans but present in all other great apes.

Finally, I contend that vocalization is to a large extent independent of the emergence (in *H. sapiens*) of a capacity until now undetected in other species — I mean a capacity to create meaningful linguistic structures, and to endlessly code thoughts and reflections. Thus, I think there is a clear difference between speech and language, as shown in modern linguistics. Moreover, as a result of my analysis, I will also defend that the critic point in the Neandertal vocalization issue — once theoretically separated from the above mentioned linguistic capacity — is whether or not this hominin had the capabilities for motor control of orofacial and lingual muscular structures and, in the last analysis, the same cognitive capabilities related to linguistic sound processing (i.e. the phonetic and phonological aspects).

1.1. Some Basic Notions from Evolutionary Studies

In order to avoid ambiguities, I will use some traditional concepts from cladistics which are useful for describing the characteristics of particular species, particularly specific changes that attract the attention of scientists. Thus, in cladistics (see Kitching *et al*. 1998), a derived change is called *apomorphy*. Whereas adding to it the prefix *sin-* indicates that the character is shared with other species and their common ancestor, the prefix *auta-* indicates that the character is an exclusive novelty of a particular species. Instead, a *homoplasy* is a new character shared by two or more non-related species. Thus, (i) an example of an autapomorphy is the loss of hair in humans, but not in other primates, (ii) an example of a sinapo-morphy is the loss of hair in cetaceans, but not in other mammals, and (iii) an example of a homoplasy is the fact that both bats and birds have wings.

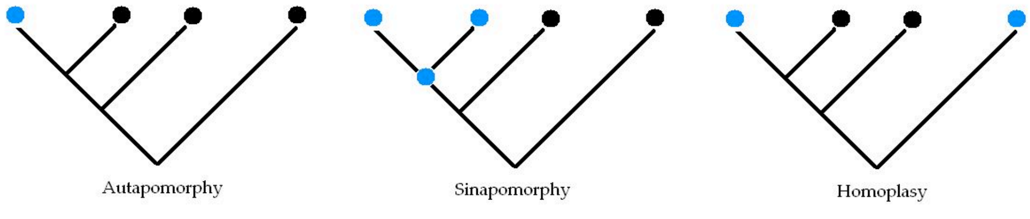

Figure 1: Cladistic concepts

In section 2 I will deal with the perceptive capacity of modern humans which, as it seems, could have co-evolved along with the rest of the vocal apparatus. The reason is that, according to an evolutionary perspective, in order to produce distinctive vocal sounds, we have first to be able of perceiving them adequately, because such feedback is required to imitate them. Thus, I will explore the cognitive abilities related to both production and perception. In this way we will see how they — according to experimental work in this field on the one hand, and to the comparative method on the other hand — could have been very similar in the hominid ancestors of *H. sapiens*, like *H. heidelbergensis*, and, by extension, *H. neanderthalensis*. In doing so we can judge better whether it is a case of autapomorphy, sinapomorphy, or homoplasy.

This methodology is also applied in the third section, where I review some aspects of the neuronal apparatus sustaining part of the human cognition related to speech. I take an integrative view according to which the brain has neither evolved *de novo*, nor layer after layer, as if the only new and mutable part were just the cortex. The approach I put forward agrees with a vision of the brain according to which this organ has undergone different selective pressures and hazardous episodes (e.g., mutations) that made possible not only the expansion of old areas (e.g., prefrontal cortex), but also the slow and gradual modification of already extant structures (e.g., current basal ganglia). Therefore, I am cautious when inferring the presence of certain neurons: on the one hand mirror neurons, and on the other hand *von Economo* neurons. The former have been detected in several representatives of the primate world and it is argued that they could also be part of human brains; the latter constitute a kind of neurons which, from an evolutionary point of view, is very new in primates. In fact, they are present in only one family of hominoids: the *Hominidae* (see Figure 2), which covers the extant species of great apes. These neurons are particularly numerous in the tribe *Hominini* (both genera *Homo* and *Pan*).

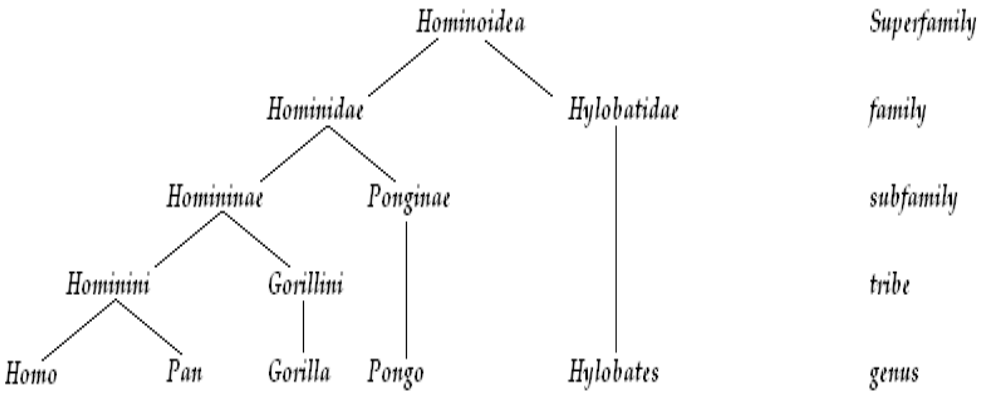

Figure 2: Classification of Hominoidea

The classification of hominids is a burning issue in the current debate of the evolution of human beings. There are many ancestors, and there are several possibilities as to which one of them is our direct precursor. The following classification I is just one among others, and should be taken only as a reference frame, rather than as a firm proposal. However, I have included a mark referring to a variant proposed by Wood & Collard (1999), according to which the current genus *Homo* would have begun with the *H. erectus*. I take into account this approach about the genus *Homo* because it reorganizes the vision one could have about it and, in consequence, it has implications for the conception one could have about a particular fact related to speech.

Finally, the comparative method is also applied at a molecular level. Thus, in the fourth section I will review succinctly the most important aspects of the gene *FOXP2* (and of its protein) as regards the role it plays in vocalization and in the control of orofacial muscles, and other genes related to sound perception as well. This gene is interesting because it is related to some language impairments, but also because the dating of its evolution is quite recent. As a matter of fact, it could be circumscribed to the genus *Homo* or even only to a subset of it.

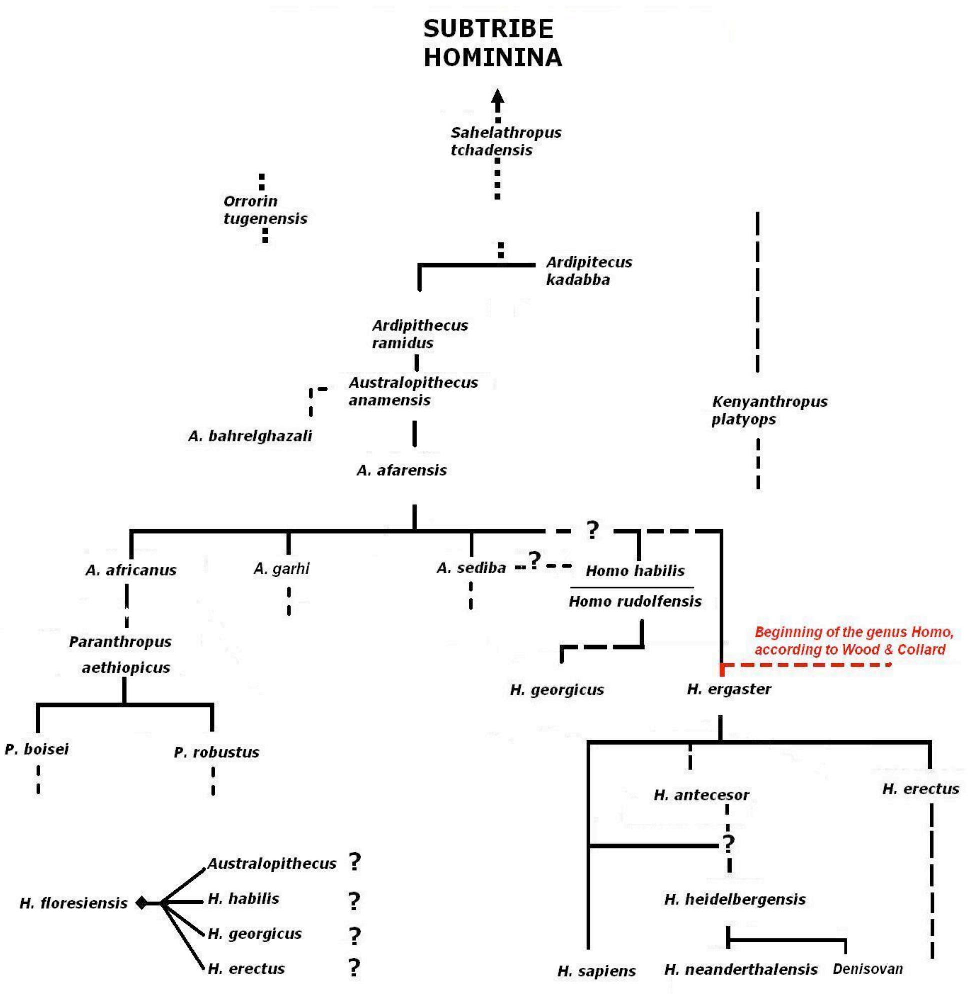

Figure 3: A possible classification of the subtribe Hominina

2. On Vocal Apparatus Reconstructions: The Archaeological Method

The issue of linguistic capacity in species other than *H. sapiens* has been a topic for debate for a long time. Nevertheless, the lack of data sometimes forced researchers to get into a high level of speculation, so much so that in 1866 the *Société de Linguistique* from Paris banned this topic as unsuitable for scientific research. The discovering of Neandertal remains and its acknowledgement as a species turned out the status of the modern human. This 'cousin' was seen somehow as a kind of 'rival'. The modern human had been considered by many people as the final milestone of evolution. Since then, there has appeared a need to look for the factors that make us humans. One proposed factor was language. This apparently unique human capacity became center-stage in a great debate that still endures nowadays. Language attracted attention in the evolutionary debate, due to its potential explanations of facts still on discussion, like the extinction of Neandertals.[2] Leaving aside the enormous responsibility suddenly acquired by speech concerning the probability of a species survival, Philip Lieberman said something that posterior studies have confirmed — although it is by and large not so radical in their conclusions: In some cases, a language or any other social factor like religion could be an important, if not essential, factor in the genomic evolution of a population.[3]

2.1. *The Role of Fossil Record in Models and Reconstructions*

Lieberman & Crelin (1971) carried out a study that heated the Neandertal issue, and so began a still enduring debate. The authors reconstructed the larynx of the specimen found in *La Chapelle aux Saints* in order to verify whether or not this Neandertal had, from a morphological point of view, the basics required to produce the vowels that are typical in modern human phonetics. This model was then compared with the larynx of both modern human newborns and chimpanzees. Their conclusion was that the Neandertal laryngeal structure was more similar to that of a human newborn than to that of an adult human. Possible and impossible sounds for Neandertals are reproduced in Table 3. It is important to keep in mind that, among the vowels they could not produce, there are three vowels which are considered universal in modern human phonetics (Maddieson 1984). Furthermore, these are the most stable vowels, so that they are considered the minimal repertoire a language can have: [u], [i], [a].[4] The authors argue that modern humans can produce them due to their high larynx position. How is it possible? According to current phonetics, the human vocal system is based on two tubes, two cavities: The first one is horizontal, from the lips to the velum, and a second one is vertical, from the velum to the glottis (see Figure 4).

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[2] For example, Lieberman (1992) has argued that the kind of 'speech' held by Neandertals was the genetic factor that provoked their extinction.

[3] Examples of this are the results of several works on Basque population (Alonso *et al.* 2005, Alzualde *et al.* 2006), Catalan population from Mallorca descending from Jewish groups (Picornell *et al.* 2005), or the Catalan population from Ibiza, whose mitocondrial DNA could directly descend from Phoenician people (Tomàs *et al.* 2006, Zalloua *et al.* 2008). These social factors (same language, religion, or even genre) do not work on the genome directly, but they have an influence on mating selection and, hence, indirectly on the future genetic recombinations.

[4] These vowels are the most extreme in the vowel space: [i] is the highest and most anterior, [u] is the highest and most posterior, and [a] is the lowest.

H. neanderthalensis	**Consonants**	**Vocals**
Impossible	[m], [n], [ɱ], [ɲ], [ɳ], [ŋ], [g], [k]	[u], [i], [a], [ɔ]
Possible	[b], [d], [s], [z], [v], [f]	[ɪ], [e], [U], [æ]

Table 3: The Neandertal vocal sounds from Lieberman & Crelin's (1971) model

While in adult humans these two cavities have similar length, newborns' larynx is higher, so that they are not able to emit the same range of sounds. The same happens in chimpanzees. On the one hand, they have a high larynx when compared with humans'. On the other hand, the horizontal cavity is longer than the vertical one. The most provocative conclusion, according to Lieberman & Crelin, is that the drop of the larynx has not other adaptive advantage than benefiting oral communication, while the price is the danger of choking, too high if no compensation makes it bearable.

Later on, Lieberman (1973) speculated on an evolutionary process divided in three stages: (1) increase of confidence in vocal communication for activities like hunting; (2) emergence of a vocal tract which is able to produce acoustic signals which are both distinctive and resistant to articulatory errors; (3) evolution of neural mechanisms that made use of the pre-adapted[5] properties of the supra-laryngeal vocal tract for a faster codification of communication. This idea has been redesigned and enhanced through time: It would have been a gradual process that flowed into the current human capacity for voluntarily articulating sounds, which would have been acquired, since *H. erectus*[6] (Lieberman 1998).

Thus, according to Lieberman & Crelin, the 'key factor' of speech evolution would be the specific place where the hyoid bone was placed — a contention criticized by Falk (1975): It was based on a too high reconstruction of the hyoid bone position. Falk pointed out that Lieberman & Crelin did not take into account the requirements of 'deglutition', nor the age of the subject in relation to the distance of the hyoid bone below the mandible, nor the sexual dimorphism in the development of the thyroid cartilage. Moreover, Falk remarks that, in chimpanzees, the corpus of the hyoid bone "appeared as continuation of the laryngeal air sacs" (Falk 1974: 125).[7]

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[5] The author makes use of the concept 'pre-adaptation', also used by Darwin (1871): "[N]atural selection channeled development in particular directions of previous modifications selected for some other role". This use of the term 'pre-adaptation' is not entirely adequate and differs from Darwin's notion, since it should be an *exaptation*. The most important is the idea that follows: In organic evolution, there is a continuous exploitation of already existent structures.

[6] Partially in support of this idea, MacLarnon & Hewitt´s (1999) conclusions point out that an enhancement of breath control is produced at some point between 1.6 millions of years and 100.000 years ago. Thus, probably at the time of *H. erectus*.

[7] This is an important observation, since Falk is one of the first in noting the relevance of the laryngeal air sacs in the debate of the evolution of the vocal apparatus. Falk argues that the vertical movement of the hyoid bone "compresses the orifice of the laryngeal air sacs" (p.

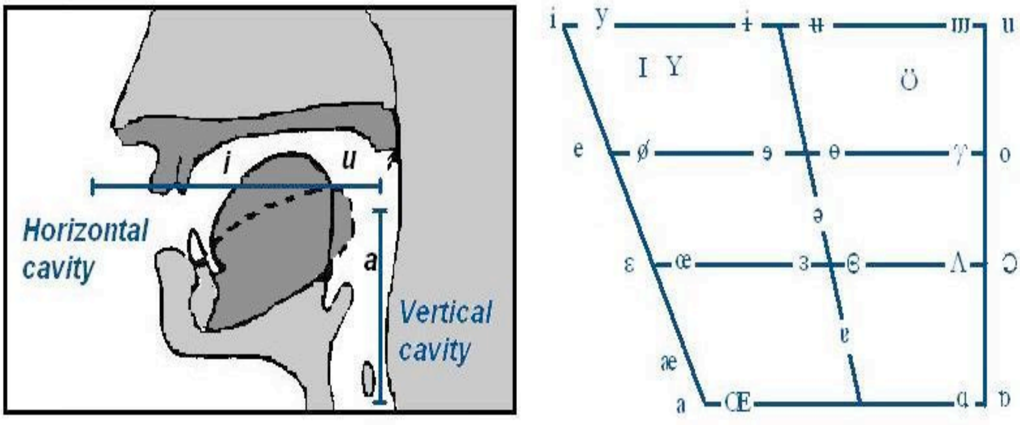

Figure 4: Vowels [i], [u], [a], in the human vocal tract (left side) and the repertoire of vowels in human languages (right side)

Thus, the discussion focused on the hyoid bone, in fact quite problematic: It is the only bone of our body which is not in direct touch with another bone, that is to say, it 'floats' within tissues. To the uncertain position of this bone, a further problem was added up: They had no sample of a hyoid bone of *H. neanderthalensis*. The debate did not change its course when a Middle Paleolithic hyoid bone — belonging in all probability to a Neandertal individual — was finally found at Kebara (Israel). Its morphology was similar or even identical to that of *H. sapiens* (Arensburg *et al*. 1989, Bar-Yosef *et al*. 1992). Paleoanthropologists were inclined to conclude that this finding supported a lower larynx and hence, that speech was already present in *H. neanderthalensis*. It is worth noting that the conclusions ventured from the observation of morphological remains were to some extent hazardous. This great debate about the linguistic capabilities of Neandertals has often been focused in such a way that sometimes it is not easy to discern whether the object of analysis is language or speech. It has been specially Lieberman who has been centered in doing research into Neandertal speech, and not language[8] (as a cognitive faculty). Lieberman (1993) has rejected these conclusions about the hyoid bone from Kebara, arguing that the shape of the hyoid bone is irrelevant: A pig's hyoid bone could pass for a human's; hence, it does not follow that the shape is linked to a supralaryngeal air duct, since this must not to be linked to the hyoid bone. I partially disagree with Lieberman in this respect: The shape is important because, as we will see next, it helps us decide whether the air sacs were connected to the bone or not, and this fact differentiates us from the rest of great apes (gibbons, gorillas, chimpanzees and bonobos; see below section 4.3.).

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125) and hence, this fact also participates in the mechanics when the air sacs are emptied and filled up. Chimps lack the mechanisms that prevents humans during deglutition, author adds (above all, because they do not need it at all). Section 2.3.1. of this work is devoted to laryngeal air sacs, whose absence in human beings has been claimed as one of the apomorphies of the genus *Homo* (see Figure 3).

[8] Though Lieberman has developed his own hypothesis particularly based in speech and its evolution paved the way to the emergence of modern language. On this matter, see below section 4.3.

Figure 5: Models of H. sapiens (male on the left; female in the middle) and H. neanderthalensis (male on right)

Gradually, new morphological elements were introduced which could shed some light on the debate and undo the knot that had tied it down: the basicranium angle and the hypoglossal canal. Lieberman (1993) has pointed out the relevance of basicranial flexion, a trait that was related to the supralaryngeal vocal tract, in which larynx is placed down in the neck. An often unnoticed detail is that this does not mean that Neandertals had no capacity for language or speech, but only that these would simply be different from ours (Lieberman & Crelin 1971: 221). This idea was strengthened by arguing that while one half of the human tongue remains in the oral cavity, the second half remains into the pharynx. Daniel Lieberman & McCarthy (1999) ousted the use of the basicranium angle as the 'key factor', when they compared the ontogeny of this feature in chimps and humans, and proved that there is no direct relation between the vertical growth and the vocal tract.[9]

In a letter to *Science*, Lieberman (1999) — against Wolpoff (1998) — disagreed with the physical anthropologists's statements which base the presence of language in Neandertals on their having the hypoglossal canal[10] (through which the tongue nerves pass). Lieberman (1999) argued that, the fact that chimps cannot talk tells us nothing about the hypoglossal canal[11] (smaller in the genus *Pan* than in the genus *Homo*).

Thus it seems that — for the moment — the methodology pretending to report an articulated vocal capability based only on fossil morphological traits, has been exhausted. As we have seen, the traditional reconstructions do not let see either (1) whether or not Neandertals had the required superior vocal space, or (2) whether or not the different components (muscles, bones and cartilages) are located properly in the place they had in living individuals, or (3) whether or not Neandertals were able to manage all this bio-mechanic apparatus in order to produce voluntary vocalizations. As far as cognition is concerned, Krantz (1988) had already speculated that Neandertals had to have some neural circuit which made possible the vocalization exhibited by *H. sapiens*, but he did not add any

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[9] This has been admitted by Philip Lieberman (2002: 555).

[10] In fact, Wolpoff echoed the work made by Kay *et al.* (1998) on the hypoglossal canal.

[11] That's the reason why, in this work, I have not included this trait as a feature directly related to the capability of vocalization.

detail.[12] The great problem is how to measure it in Neandertals, when the best we can get is an endocast of the brain, where one can vaguely detect the brain's circumvolutions, verify the lateralization and not much more as regards language.[13]

2.2. *New Approaches for Vocalization: The Role of Larynx*

In recent years a great technical development has taken place in the field of computational models. This has allowed facing the issue of vocalizations in non-humans from perspectives and techniques which differ from the aforementioned.[14] Moreover, the tendency to consider the evolution of modern language lightly practically disappears, so that statements about linguistic capacities based only on the presence of the hyoid bone are not frequent any more (though they are often hinted, especially in paleoanthropology).

This notwithstanding, proposals focused on finding the 'key factor' of modern speech are still persistent. Such tendency to avoid the creation of a multi-factorial model which could integrate different proposals brings, time and time again, to a theoretical cul-de-sac. Now the discussion has moved to cavities rather than to bones. Making use of new technologies, Honda & Tiede (1998) proposed a new analysis centered in orofacial morphological factors which correlate with the individual variation of the larynx position, measured in a resting condition. Using orofacial geometry, the authors made a statistical analysis that predicts the height of the larynx. Orofacial geometrical data based on magnetic resonance imaging came from previous works (Honda & Tiede 1998, Tiede, 1998). The analyses of correlation showed that the height of the larynx can be predicted either from the aspect ratio of the oral cavity or from the antero-posterior dimension of the facial cranium. A concept taken into account in Honda & Tiede (1998) is that of *maximal vowel space*.[15] Boë *et al.* (2002) used this parameter to estimate the vowel space in Neandertals. They concluded *H. neanderthalensis* had a vowel space similar to ours and, hence, could have been able to emit the same range of vowels, though they warned that this does not mean that Neandertals were able to talk. What they affirm is that Neandertals had no morphological limitations in this respect. These results have been criticized by Lieberman (2007), who consi-

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[12] Lieberman (1992), of course, would disagree with that.

[13] This is true regarding the particular cito-architectonic structure that could be involved in Neandertal vocalization. However, there is another kind of information we can get from studies of endocasts and skull shape. For instance, Gunz *et al.* (2010) have proved that both Neandertal and modern human infants had the same brain development in a first stage, but that the latter differ in a second stage, entering a 'globuralization phase' (argued as a derived *H. sapiens* trait). Thus, it seems that both species had the same brain volume at birth and that "most endocranial shape differences develop postnatally". The authors speculate "that a shift away from the ancestral pattern of brain development occurring in early *Homo sapiens* [that is, previous to the Out-of-Africa] underlies brain reorganization and that the associated cognitive differences made this growth pattern a target for positive selection in modern humans." (Gunz *et al.* 2010: R-922)

[14] See de Boer & Fitch (2010) for a historical and critic summary of the computational models of vocal tracts.

[15] Boë *et al.* (2002: 473): "[N]-dimensional space within which are situated the n first formants (resonances) of all possible vocalic sounds that can be realized by a given vocal tract".

ders that larynx cannot be considered the 'key factor' in any case;[16] on the contrary, the author proposes that the descent and shape of the tongue is what makes possible to *H. sapiens* the emission of so many different vocalic and consonantal sounds (Lieberman 2007: 556).

Indeed, many authors have paid attention to the descent of larynx. However, sometimes two different debates have been mixed up, when they are essentially different: Having a *vowel space* like ours does not imply to have a low larynx like ours too. The most important difference between Boë and colleagues, on one side, and Liberman's arguments, on the other, is that the former pays special attention to the relevance of articulatory control, whereas the latter focuses on anatomy. However, the methodology used by Boë *et al.* (2002) has been recently criticized by de Boer & Fitch (2010), due to the fact that the model of Boë and his collaborators is based on a previous model — Maeda's (1990). De Boer & Fitch argue that this model is suitable only for modern human speech. The goal of Maeda's model was to determine four points: the jaw's position and the position, shape, and tip of the tongue. De Boer and Fitch also note that most animals do not have an L-shaped vocal tract — something essential, as we have seen — exactly as described in the classical *source-filter* theory. De Boer & Fitch add that, since Maeda's model cannot report the irregularities of the tongue, its application to non-humans can lead to wrong inferences regarding the motor abilities of infants or of non-humans, as in the case of Boë and colleagues. Note in passing that the same criticism, hence, would be applicable to Lieberman & Crelin's (1971) model. Research based in computational models should be enriched with data from theoretical and field studies, so that the object of study can be outlined with more certainty. Introducing the data from the comparative method is a way to do that, as we will see next.

2.2.1. *Data from the Comparative Method: The Larynx Exaptation*

Nowadays there is a 'new' trend which recovers the concept of *exaptation* and applies it to the different traits involved in speech evolution. We have seen that the descent of larynx cannot be left aside when one analyzes the human vocal capacity. The fact that a mammal has a low or a high larynx has immediate acoustic consequences. Nonetheless, it is not the 'key factor' that made vocalizing possible. On the contrary, it is one change among others — possibly an apomorphy of the genus *Homo* — which has been added to the rest.

The descent of larynx has been explained by Lieberman by means of a functionalist theory: The descent of the larynx was beneficial to speech. I disagree with this view, since there are other, more convincing, arguments, concerning a posterior exaptation of this fact. This kind of argumentation has been put forward by Fitch (2002) using the comparative methodology. Fitch argues that other extant non-human species can either lower the larynx momentarily or even maintain it in such a position permanently. Within those that have been proven to exhibit a permanent low larynx, we find two kinds of deers, the Red deer (*Cervus elaphus*) and the Fallow deer (*Dama dama*); the males present a permanent

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[16] This opinion is also shared by Boer & Fitch (2010).

low larynx. This seems to be also true for koalas (*Phascolarctos cinereus*). *Canidae* — for example, dogs — can lower the larynx for a moment. In other words, the exhibition of a low larynx is a trait more common in mammals than previously thought. Hence, it should not be so difficult to include Neandertals among them, as Fitch himself has noted.[17] The author coincides with Lieberman about the gradual view of such a process, but differs from him regarding the function they could have given to it and thinks that the first hominids could have lowered the larynx momentarily (Fitch 2002: 37). Fitch's arguments are based on two facts: (1) This trait is evident in other species, whose males can experiment a second descent of the larynx; (2) given that formants can be used to estimate the size of the emitter, the laryngeal descent could have been favored by the ability to recognize whether the opponent is big or small, or even to make believe the adversary that one is bigger than really is.[18]

Thus, while Lieberman & Crelin concede the descended larynx an adaptive function enhancing an articulatory ability — a progressive enhancement of vocalization in genus *Homo* —, Fitch, even admitting gradual change of the laryngeal morphology, rejects that this has been due to linguistic factors (concretely, speech factors. Instead, Fitch believes that the quotidian use of the linguistic vocalization is an exaptation of this trait.

This debate has recently been tackled by means of new models: De Boer (2007) created different computational models and then compared the results with real data.[19] The results pointed out that an optimal larynx is that whose values are approximately equivalent to the horizontal dimension of the vocal tract (that is to say, as Lieberman has repeatedly said, that values of the horizontal and vertical dimensions are 1:1). The feminine larynx is the one that is closer to the 'optimal larynx' — a fact that seems to agree with Lieberman *et al.*'s (1969) thesis that the vocal tract would have evolved in order to enable as many distinctive articulations as possible. Regarding the second descent of larynx in *H. sapiens* males, de Boer accepts the theory about the emitter's size exaggeration (Fitch, 2000) — as the lowest formants point out-, and so the concomitant deeper voice. But he points out that, although this moves males away from the 'optimal larynx', in no way supposes a communicative impediment. According to de Boer, the results suggest that, if communication is important for surviving, then the larynx which is placed in the best communicative position will be selected.

I would like to stress that two different issues have been confused during the debate: (1) to have an independent articulatory space — as that proposed by Boë *et al*. (2002) — is independent of the function a low larynx could have (de Boer 2007); and (2) to have a particular physical trait does not always imply a particular function. Functions can vary in time. The original function of an organ could have nothing to do with the current function. Fitch (2002), as well as Hauser & Fitch (2003), have noted that the physical part gives us only one aspect

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[17] "If dogs can achieve a substantially lowered larynx, without any changes in basicranial angle or hyoid morphology, it seems likely that neanderthals, other fossil hominids or chimpanzees could as well" (Fitch 2002: 34).

[18] This theory is not completely new: Ohala (1984) pointed out that in a lot of human cultures and other non-human species, the F0 is used to seem aggressive and for threatening.

[19] The first, a very simple one, gives the author the values of the 'optimal larynx'; the second and third, more realistic, take into account the differences between males and females.

of the trait under analysis — voluntary vocalization with communicative intentions — and hence, the authors affirm that such a change, along with other important changes in the mechanisms of neuronal control, would have smoothed the path for the vocal capacity in *H. sapiens*. In this regard I agree with them and, as I will argue next, this clearly points in two directions: the neuronal aspect (see sections 4.1 and 4.2) and the genes that favor the neuronal control mechanisms in vocal tasks, like *FOXP2* (see section 4.3).

2.2.2. Some Conclusions about Computational Models

The debate about the capabilities for vocalization in Neandertal has been focused until now on morphology, since this was the only secure source of data at first available. Attempts to infer a capacity for oral production counted on little evidences whose argumentation was not difficult to refute. The basicranium angle or the hypoglossal canal alone will never give us more than a trace of how this hominin could have been (traces and signs that must be *added up*, and not refused, into a global, integrative evolutionary theory of speech). The vocal space in Neandertals seems to be consistent enough in order to produce vocalic sounds, although Boë *et al.*'s (2002) model is more suitable for *H. sapiens*. Obviously, some species-specific differences are expected, which could move *H. neanderthalensis* away — at least qualitatively — from *H. sapiens*. Not for nothing are we talking about two species. Lieberman's defense against the idea of conceding Neadertals a modern human-like speech, however fierce, does not discard the possibilities for a positive conclusion about the Neandertal vowel space, as the newly available resources and technology seem to point out (Boë *et al*. 2002, 2004, Fitch 2002). Hence, it is conceivable a Neandertals superior horizontal vocal tract very similar to that of modern humans, in which the potential vowels would have been very similar — if not exactly the same- to that we produce nowadays. De Boer & Fitch (2010), on the one hand, are pessimistic and think we will never know for sure how the Neandertal vocal tract was. Nonetheless, they admit that, even though mammals cannot produce vowels like humans do, it is also true that they could produce enough phonetic distinctions in order to build a basic oral language.[20] In effect, this has been proven in other cetacean species (Zoidis *et al*. 2008) or even primates — be these monkeys (Ouattara *et al*. 2009a, b) or great apes like orangutans (Spillmann *et al*. 2010). Thus, voluntary vocalization in general is not an exclusive matter of humans and the *H. Neanderthalensis* should be included, according to these criteria, in this set of species. These conclusions are reflected in the following table:

Trait	***H. neanderthalensis***	***H. sapiens***
Low larynx	*	√
Vocal tract	*	√

Table 4: Probably Neandertals had a similar laryxn and vocal tract

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[20] Of course, the authors refer to the vocalization capability, not to the computational, syntactic aspect that builds human language, an issue they do not go into.

Obviously, this does not rule out strictly Lieberman's hypothesis: Neandertals would not have 'exactly' the same vocal tract as humans and hence their vocalizations could perfectly differ from ours. To which extent, it is still not possible to verify. At the same time, Lieberman's hypothesis does not rule out communication. It is possible that, even in a situation so constrained as the one this author assigns to Neandertals, 'smooth' communication could be achieved.

In the next sections I will try to enrich with new pieces of data those aspects that computational models cannot deal with, due to their inherent limitations.[21] Regarding this question, let me quote de Boer & Fitch's (2010) words about the explanatory scope of computational models of vocal tracts:

> A real vocal tract cannot make all signals that can be produced by a system consisting of two cylindrical tubes, nor can a system consisting of two cylindrical tubes produce all signals that can be produced by a real vocal tract. (de Boer & Fitch 2010)

Much the same could be said about other linguistic phenomena like syntax or semantics, in the sense that the models that describe the modern human cannot be good for describing the Neandertal. As a consequence, a strict application of such a view would automatically invalidate any kind of inference, even those from the comparative method, making almost impossible to deepen the hypothesis on what has been said about the extinct hominids or to offer alternative ones. However, I will show in the next sections that it is indeed possible to obtain information from other scientific areas, and that such information can be related to *H. neanderthalensis* and its potential vocal capability. These sources, in the line pointed by Fitch (2003), are neuropsychology and genetics. Thanks to the comparative method we will see data from other animals that can help us to draw a picture of the Neandertal phonating capability, richer than that offered by works only based on reconstruction or on simulation. I would like to stress that these works do not have to be excluded. On the contrary, they have rather to be complemented by these new data.

Next and finally ending this part on the vocal tract, I will briefly review the historical evolution of an apomorphy of the genus *Homo*, which distinguishes it from the rest of the *Hominidae* (see Figure 3): the hyoid bone in relation to the lack of laryngeal air sacs. The fact that these two traits are evolutionary related to each other has had important consequences on the phonating ability of the whole genus *Homo*.

2.3. *The Hyoid Bone in the Genus Homo and the Missing of Air Sacs*

As I have advanced (in section 2.1.), we cannot figure out the evolution of voluntary vocalization only from the hyoid bone. However, it is a good pointer of the phylogenetical path followed by the vocal tract: The hyoid bone has implications in the final result of phonation and, this being, the difference in

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[21] As de Boer & Fitch (2010) notice: "The first lesson is that one cannot always apply models and theoretical frameworks that have been developed for modern human language to the evolution of languages and expect valid results" (p. 15 of the pre-publication manuscript).

morphology has to produce (minimal) changes. Falk (1975) said that the critical flaw in Lieberman & Crelin's (1971) results was that the authors had wrongly placed this bone. Thus, the concomitant result was an unproductive vocal tract in comparison with that of *H. sapiens*. Interestingly, Falk also pointed out the direct relation of this floating bone with other primates' air sacs of the tribe *Hominini*, like chimpanzees (*Pan troglodytes*) and bonobos (*Pan paniscus*): the hyoid bone compresses the orifice of the air sacs. Thus, one may ask some reasonable questions: in which moment has changed the hyoid bone? Is this a change of the whole genus *Homo*, or just of the species *H. sapiens*? Can it be extendable even to the whole subtribe *Hominina* described in Figure 3?

We find the answer in the fossil record. As said, I assume Wood & Collard's (1999) proposal according to which the genus *Homo* has its birth at the African *H. erectus* clade, also called *H. ergaster*.[22] I'll show that the hyoid bone is another trait in favor of this classification.

Additional to the Neandertal hyoid bone from Kebara, two more hyoid bones were found in the Iberian peninsula (Martínez *et al*. 2008). They date from about 530.000 years and belong to the *H. Heidelbergensis*, species, considered a precursor of both Neandertals and modern humans (Mounier *et al*. 2011), though this last issue is more controversial.[23] Anyway, if a distribution as the one showed in Figure 2 is accepted, the common ancestor of Neandertals and modern humans already had a modern or *derived* hyoid bone.

Figure 6: Different hyoid bone models from different primates. © Bone Clones www.boneclones.com, with permission

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[22] These authors thus transfer *H. habilis* and *H. rudolfensis* to the genus *Australopitecus* (see Figure 3).

[23] *Homo heidelbergensis* has also been called *archaic Homo sapiens*. Other authors consider *H. heidelbergensis* just an archaic state of *H. neanderthalensis* and hence, these two species could be merged into the same species (see Cela-Conde & Ayala 2001: 336–342). If this is finally confirmed, only two species and not three would have confirmed a derived hyoid bone.

However, the most important fossil remain that seems to mark the end of an archaic period of the hyoid bone in the subtribe *Hominina* is the one found at Dikika, Ethiopia (Alemseged *et al.* 2006), which belongs to an *Australopithecus afarensis*. The most remarkable characteristic is that the orifice in the corpus clearly reflects the presence of laryngeal air sacs, a trait absent in *H. heidelbergensis*, *H. neanderthalensis* and *H. sapiens*. A morphological analysis of the *Australopithecus* hyoid bone places it in the range of *Gorilla gorilla* and very close to *Pan*.[24] This floating bone has evolved in many different ways in different species (Figure 6). But its effect on modern acoustics begins with the disappearance of air sacs.

Laryngeal air sacs have been almost unnoticed by scholars. It is Fitch, above all, who has demanded more attention for this apomorphy of the genus *Homo* (Fitch 2000, Hauser & Fitch 2003): Among great apes, only *H. sapiens* lacks them. Air sacs have an important effect on vocalizations, producing a sound with its own formant. Campbell's monkeys use this sound as a particular call apparently linked to a particular meaning (Ouattara *et al.* 2009a, b).

One could question whether or not these sacs appeared previously to the emergence of our species. Again, I apply the *principle of parsimony*: There are plenty of mammals exhibiting laryngeal air sacs, like primates, but also cetaceans (Reidenberg & Laitman 2008, Hewitt *et al.* 2002) which suggests that air sacs are a quite common ancient trait. Within great apes such novelty can be considered an autapomorphy of *H. sapiens*.[25]

Apart from that, in relation to the rest of the extinct hominids of the genus *Homo* that eventually cohabited with *H. sapiens*, the lack of air sacs is a trait shared with *H. neanderthalensis*.[26]

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[24] If in the future it is shown that the species currently classified as *H. habilis* had a primitive hyoid bone like that from Dikika, it could be more forcefully argued (1) thar such species is a member of the genus *Australopithecus (i.e. Australopithecus habilis); (2)* that the derived hyoid bone is an apomorphy of the genus Homo and (3) that all remaining hominids of this lineage (from *H. erectus till H. sapiens*) shared such morphology, much modern and closer to that shown by *H. sapiens.* Much the same could be said, of *H. floresiensis*: a hominid which presents several morphological features that had been lost at the node of *H. ergaster.* Its inclusion into the genus *Homo* is due to the fact — as in the *H. habilis'* case — that some tools were present in the near of the remains (some of them belonging to the Mode I and some other to the Mode IV), relating them to that species. Its morphology, however, moves it away from the rest of the members of the genus *Homo.*

[25] Note that there exists a kind of laryngeal air sac in *H. sapiens*, but it has a pathological origin: The laryngeal ventricle of Morgani — a little and elliptical cavity located within the false and true vocal folds — can suffer an abnormal saccular dilatation which could expand outside or inside the larynx. This has been called laryngocele. The increment of air pressure could be its trigger. The most known functional factors are cough, singing or blowing into an instrument (Giovanniello *et al.* 1970). It is a disease known among trumpet players and glass makers.

[26] And probably with another new species: Recently Krause *et al.* (2010) have published the results of an analysis of DNA of an unknown specimen dubbed the 'Denisovian hominid'. It is for sure a hominid, and lived 40k ago in the middle of Siberia, at the Denisova cave. Precisely 100 km far away from that place, the Teshik-Tash cave (Uzbekistan), at more or less the same time, was inhabited by Neandertals — as Krause *et al.*'s (2007) DNA analysis has confirmed.

A first datation suggested that the common ancestor of Denisovians on the one side, and Neandertals and modern humans on the other side, dated from 1 mya, and hence it could well be a descendant of an Asiatic *H. erectus*. Nonetheless, two recent works have shown

Experimental studies using computational models have also dealt with this characteristic physical trait and have found that air sacs affect vocalic formants elevating them and thus loosing vocalic space (de Boer 2008, Riede *et al.* 2008). Although it is necessary to make more research in this field, especially on the functions of air sacs, some conclusions can be drawn. First, the variety of species having this organ suggests us its ancient character. Its absence is a derived trait in extant modern humans, shared with Neandertals. Though it should be confirmed, the evidence points to the possibility that *H. erectus* already lacked air sacs and also had a derived hyoid bone.

Second, we cannot discard that in some cases it could be a case of new and convergent novelty or *homoplasy* (see Figure 1).

Third, experiments have stressed the instability effect produced by air sacs and this supports the benefit of maintaining a stable sound source in oral communication, above all in short distances. While air sacs are useful in long distances, as shown by the behavior of Campbell's monkeys, who create a security perimeter around their living area (Ouattara *et al.* 2009a, b), or by the long vocalizations of orangutans[27] — semi-solitary great apes — whose calls seem to be a distortion source in short distances. This could be the context that would be favored by the *sudden* disappearance, probably neutral in a first moment, of the air sacs in the genus *Homo*: close communication within con-specifics.

Lastly, the effect caused by laryngeal air sacs does not indicate that the path covered by the genus *Homo* was always the 'improvement path' of sound production. Air sacs seem to increment the range of vocalizations: Some cercopitecs are known to make use of this organ to emit a particular call ('boom'). Nevertheless, I agree with Riede *et al.* (2008) that modern humans are capable of a greater and more refined increment of vocalizations thanks to the multiple combination of articulation points. Note in passing, that this mastery of vocal articulation would put for the moment into the same group the whole genus *Homo*, from *H. erectus* till *H. sapiens* (see Figure 3).

In the next table I show two pieces of data that we unquestionably know regarding the Neandertal man:

Trait	***H. neanderthalensis***	***H. sapiens***
Derived hyoid bone	√	√
Lack of air sacs	√	√

Table 5: Both species show a derived hyoid bone

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that (1) Denisovians were closer to Neandertals than to Sapiens, and (2) that Denisovians interbreed with the Sapiens that reached southeast Asia and Oceania (Reich *et al*. 2011, Vorobieva *et al*. 2011). What seems evident is the fact that the diversity and co-existence of hominids in some areas was something usual before the exit of *H. sapiens* from Africa.

[27] Such vocalizations provide information to orangutans helping them to confirm the discrimination of male individuals, but also information on the context, complemented by the responses of females (Spillmann *et al*. 2010).

3. On the Perceptive Capabilities of *H. Neanderthalensis*

The discussion carried on until now about the vocal space or over the possibility of emitting articulate sounds by Neandertals makes sense only if this hominid was capable of perceiving and processing them as we do: categorizing them linguistically, in order to reproduce them later on as audible signals. Could the *H. neanderthalensis* recreate the same sounds, and if so, in the same way as modern humans?

Sound is a phenomenic experience of our cognition: The information from the auditory organs arrives to particular cerebral regions that transform the neural activity into something perceived as sound. The organ of Corti, in the inner ear, contains the structures that allow the conversion of acoustic stimuli into neural activity.

Next, I will deal with aspects of sound processing that are shared by mammals — for example, formants — in such a way that I'll be able to offer relevant information about the speech processing of Neandertals.

3.1. Formant Perception by H. Neanderthalensis

An aspect of acoustic processing shared with the rest of mammals is formant perception. This observation was formulated in the 70's by Morton (1977) and recovered by Fitch (2002). Formants let us differentiate between vocal sounds, and in normal conditions only two formants (F1 and F2) are necessary. In 1969 Lieberman and collaborators already suggested that other non-human mammals could be able of perceiving formants. In effect, some years later it has been proven true in rhesus macaque (*Macaca mulatta*; Hauser *et al.* 1993) and vervet monkeys (*Chlorocebus aethiops*; Owren 1990).[28] Fitch (2002) holds that formants provide information about the emitter's body size: Indeed, several studies seem to corroborate a positive correlation of body size and decreased frequencies in humans, domestic dogs and monkeys (Fitch 1997, Fitch & Giedd 1999, Riede & Fitch 1999). These studies suggest that mammals with a larger body produce lower frequencies. According to Fitch (2002), our mammalian ancestors could have used the frequencies of formants in order to estimate the body size, on the basis of the emitter's vocalizations. Moreover, the author reminds us the fundamental role of formant recognition and the fact that most mammals and current birds can discriminate formants. According to Fitch, this suggests that the reptilian ancestor of both lineages was also capable of doing it. With all this in mind, I see no reason to think that Neandertals were not able to do the same as *H. Sapiens* and this in a similar way: The Neandertal man's body was more robust than the anatomically modern human's, but not so robust as to create a signifycant difference regarding formant perception.

The most basic micro-abilities related to speech perception are theoretically also available to *H. neanderthalensis*. There are plenty of data from the comparative method showing that a lot of perceptive micro-abilities we link today to language have their origins millions of years ago (see the cognitive cladogram in

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[28] This species was classified as *Cercopithecus aethiops* at Owren's time and still is today.

Figure 6): The ability to discriminate between different linguistic rhythmic patterns has been positively proven in five-month-old babies (Nazzi *et al.* 2000), but also in cotton-top tamarins (*Saguinus oedipus*; Tincoff *et al.* 2005), and in common mice (*Mus*; Toro *et al.* 2003); categorical perception has been put to the test with positive results in chinchillas (*Chinchilla*; Kuhl & Miller 1975).

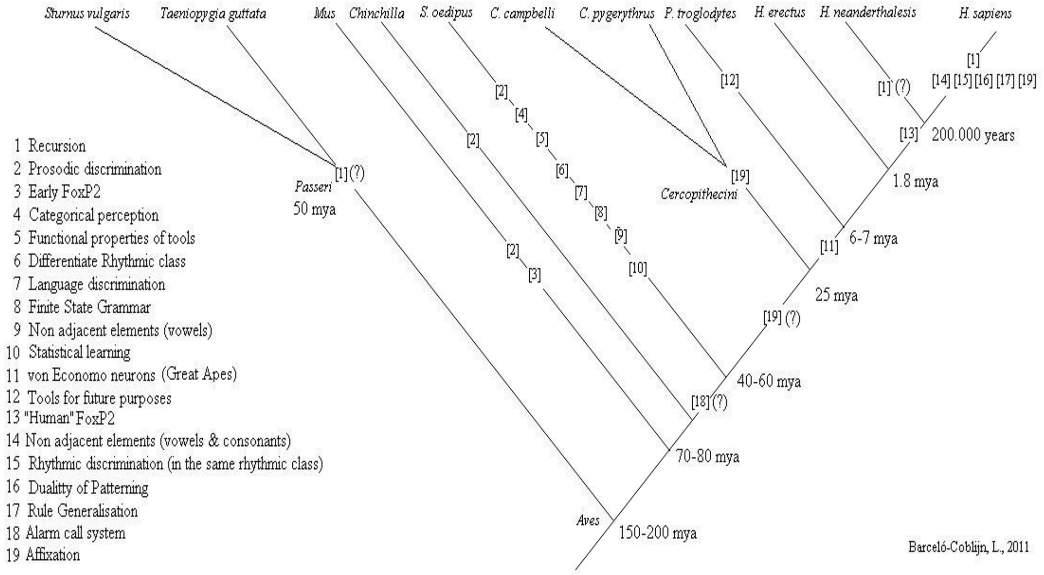

Figure 7: Cognitive cladogram showing traits and abilities proved in other species[29]

Therefore, if one pretends to relate these cognitive micro-abilities to those of modern humans and to postulate a unique origin as well, much the same should be applicable to *H. neanderthalensis*.

An aspect I cannot assure for the moment (pending improvements in new computational models, or even of new genetic data) is which range of frequencies and which combinations this hominid could produce. In this regard, I coincide with the consideration about the fact that the modern human is the extant mammal which can produce the broadest range of formant frequencies (Lieberman *et al*. 1972, 1969).

Hauser & Fitch (2003) have suggested that another kind of information could be extracted from formants: the speaker's identity. Given the individual differences in the oral and nasal cavities, some subtle differences are produced within speakers, which allow their individual identification. In this sense, it has been suggested that such information could be within the scope of other primates (for example, macaques; Rendall *et al.* 1998), but Hauser & Fitch (2003) consider that there is still not enough information about it, or verifications that allow us to conclude anything on this point. We know that the recognition of group hierarchy is not an exclusive trait of humans, but also of bonobos and other social

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[29] Previous versions of the cognitive cladogram and the references of traits can be found in Nadal (2009) and in Barceló-Coblijn (in press). The cognitive cladogram evolves parallel to research on animal cognition.

mammals (Seyfarth *et al*. 2005). Identity recognition by means of formants could be important, depending on the species. Although it is still debated what kind of familiar groups were predominant in the Neandertal society, it is generally agreed that this species could count on a strong social — and possibly also cultural — component (though it would be necessary a less broad definition of the concept of *culture*). If it is confirmed that this kind of information extraction was already available for hominids before the split between modern humans and Neandertals (as in the case of macaques, mentioned above), it seems that there is no reason to think that a source like that, so useful for social relationships, had fallen from grace in the evolutionary path followed by Neandertals. If the contrary is confirmed, it should be considered the possibility that this is an ability only developed by *H. sapiens* — but not by the rest of primates.

Ohala (1984) proposed a theory, from an ethological point of view, about the use of the fundamental frequency (F0) in aggressive contexts. According to his observations, it seems that when F0 is lower, the voice acquires a 'dominant' edge. Curiously enough, the author pointed out that, in humans, a high F0 is also used to calm an aggressor, trying not to look like a potential threat.[30] Thus, the author implicitly supports the positive interpretation by the hearer. Unfortunately, we are still far from knowing how the formants were produced by Neandertals.

3.2. *Looking for the Auditory Bases of H. Neanderthalensis*

Genetics has contributed enormously to the understanding of species evolution. In particular, maybe the Neandertal case is one of the most prominent in recent t times, given the narrow bond between both hominids and the endless debates surrounding them (this notwithstanding, Neandertals are not always mentioned in works on genetics, from which straightforward inferences can be drawn). In a recent work, Clark *et al*. (2003) have detected several genes which have been the target of positive selection during the evolution of mammals — by means of comparison of the genomes of mice, chimps and humans. Regarding the ear and acoustic perception, the authors point to the genes *DIAPH1*, *FOXI1*, *EYA4* and *OTOR*. Some genes of the *EYA* family[31] seem to play a relevant role in the development of eyes and ears; much the same can be said about *FOXI1* (Vidarsson 2007). Most interesting, however, is the fact that the malfunction in the majority of these genes could produce different kinds of deafness, as we can see in Table 6.[32]

For the moment, it has not been stated explicitly whether Neandertals had exactly the same variants of these genes. Taking into account the great genetic affinity of both species, it would not be surprising that Neandertals had also the same alleles. This notwithstanding, it should also be found out whether their expression was exactly the same for Neandertals. A work compiling the last years

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[30] Interestingly, Ohala pointed out that this could be a kind of infantile mimicry, and that for obvious reasons natural selection has left most species with a strong inhibition against infanticide.

[31] Whose name comes from 'Eyes Absent'.

[32] Besides the authors' cited works, the data also comes from *PubMed* > *genes*.

of genetic research on Neandertals has been published recently (Green *et al*. 2010). In the included list of genes (pp. 714f.), there are all genes that have had a change of amino-acids which are present in current humans, but are ancestral to Neandertals. All of the above mentioned genes are in this provisional list. There also appears the gene *EYA2*, a gene known to interact with the gene *GNAZ*. The latter gene has been related to the maintenance of the ionic balance of perilymphatic and endolymphatic cochlear fluids. Finally, we find a protein linked to the *tectorial membrane*,[33] *ALPHA-TECTORIN* (codified by the gene *TECTA*[34]) whose disruption provokes another form of deafness (Verhoeven *et al*. 1998).

Gene	***Gene ID***	***Related expression areas***	***Negative repercussions for the ear and other***
DIAPH1	*1729*	Regulation of actin polymerization in hair cells of the inner ear	Autosomal dominant, fully penetrant, nonsyndromic sensorineural progressive low-frequency hearing loss
FOXI1	*2299*	Kidney, organ of Corti, epydermis, cochlea and vestibulum	Human neurosensorial deafness
EYA4	*2070*	Organ of Corti	Postlingual, progressive, autosomal dominant hearing loss at the deafness
EYA1	*2138*	Kidney, eye, ear, branquial arches	Branchiootic syndrome[35]
OTOR	*56914*	Cartilage development and maintenance	Different forms of deafness

Table 6: Genes related to the inner ear formation and deafness

Altogether, there is firm evidence of the positive selection of some genes related to the ear, to the capacity of auditory perception, and it seems that they have played a fundamental role in the evolution of mammals, in this case in the genus *Homo*. Note that theirs is not a minor role, in the sense that it affects very basic structures and most of the time they get expressed in different parts of the body (ear, eye, kidney, etc.). Their selection and fixation is not produced suddenly and it would be surprising that Neandertals differed greatly from modern humans regarding these genes. The fact is that, given the high percentage of genetic similarity and the fact that *introgressive hybridization*[36] was possible (Green *et al*. 2010), one must contend that the individuals descendant of

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[33] An extra cellular matrix of the inner ear that contacts the streocilia bundles of specialized sensory hair cells.

[34] The Gene IDs of *GNAZ* and *TECTA* are 2781 and 7007 respectively.

[35] The hearing loss is variable with respect to severity and age of onset.

[36] We talk about hybridization of two species when the result of such process is a new one species, and the concomitant disappearance of the two precedent species. However, 'introgressive hybridization' takes place when two species partially interbreed in such a way that the mechanisms that support the species' structure have been violated only occasionally and, hence, the unity of species still endures (in other words, there still remain two species).

such interbreeding cases were in all respects fully biologically viable. Otherwise, this little percentage of genes with Neandertal origin would have disappeared from the non-African *H. sapiens* populations.

A similar approach to the one I propose regarding the auditory capabilities of Neandertals, led to Martínez *et al*. (2004) to carry out an experiment in order to find out which was the range of sensitivity in kHz of the ear of the hominids from Sima de los Huesos.[37] The authors created a model taking into account the morphological data from the original skulls, obtained by 3D computerized tomography. They also recreated the external and inner ear bones. Their intention was to measure the transmission of the acoustic power. Although what they obtained is not a true audiogram, one can see a strong correlation with the different frequencies. Martínez and collaborators compared the results from modern humans, chimpanzees and their Atapuerca hominid model. The curve of transmission of acoustic power in the two former cases differs clearly, whereas the curve of the Atapuerca hominid model looks like that of modern humans. Moreover, the results place the Atapuerca hominid model sensitivity between 2 and 4 kHz, exactly where *H. sapiens'* sensitivity is located.

3.3. Conclusions about the Auditory Bases of H. Neanderthalensis

Although there will always be some aspects to strengthen, the amount of data being provided from different fields of study always point to the same direction: Most aspects and traits nowadays related to the perception of modern human's oral language have been inherited in the course of time. Auditory perception, like visual perception, seems to be a vitally important basic structure for mammals, which is not subjected to frequent or abrupt changes. The Neandertal man, when split off from the group of African hominids, becoming a separated species, probably was endowed with an auditory perception system very similar or even equal to that of *H. sapiens*. The genes that have been selected positively since the division of our ancestors on the one hand and the comparative method results on the other hand, suggest such parsimonious conclusion. Then, the picture of the Neandertal cognition would became more refined than the traditionally offered one: The Neandertal man would have not only a physical structure related to vocalization very similar to that of modern humans, but also very similar, or even identical, perceptive auditory capacities, including, moreover, a set of micro-abilities that seems to be shared by most mammals or — in the worst of cases — by the rest of primates. According to this hypothesis, *H. neanderthalensis* would have been able to perceive the formants of sounds; it seems logic that they would also have been capable of extracting information from them, like the emitter's identification or its size. It is not at all clear, however, that Neandertals were capable of manipulating the fundamental frequency in order to look less aggressive, though the contrary, typical in many mammals, can be attributed to this species. Experimental studies place the auditory sensitivity of Atapuerca hominin (and ancestor of *H. neanderthalensis*) around 2-4 kHz, precisely the range where human speech is located. These conclusions are reflected in the following table:

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[37] At the Atapuerca Mountains, Burgos, Spain.

Trait	***H. neanderthalensis***	***H. sapiens***
Auditory capacity around 2-4 kHz	*	√
Formant perception	*	√
Micro-abilities related to vocalization	*	√

Table 7: On the auditory capacities of H. neanderthalensis

Finally, it should be noted that genetics is providing valuable information on the ear, concretely on the genes which seem to have been object of positive selection since the separation of humans from chimps 7 mya. Although we must be cautious regarding the positive evidence of *introgressive hybridization* between Neandertals and Sapiens, the proved interbreeding suggest the possibility that these genes were shared by Neandertals and modern humans (and Denisovians, for that matters).

4. Some Neuronal Aspects Related to Vocalizations

Next, in this section, I will talk about neuronal aspects related to vocalization, which could be ascribable to Neandertals. More concretely, I will talk about two kinds of neurons (von Economo neurons and mirror neurons) and one gene (FOXP2). I'll try to contribute to the question of Neandertal vocalization showing that it is conceivable that (1) the Neandertal man was endowed with both kinds of neurons and hence this hominid could have benefited from this neuronal apparatus; (2) that Neandertals had the FOXP2 gene and, hence, there is room for the possibility for a smooth vocalization ability in this species.

4.1. The von Economo Neurons and the Hominidae

Von Economo neurons (VENs) have recently become center-stage given their singularity within great apes and their location, in areas related to vocalization, emotions or facial recognition. VENs have been known for a long time, since their description by Ramon y Cajal himself in 1899 and later on by von Economo & Koskinas (1925). Their role is still not well understood, but recently they have attracted attention, since Nimchinsky *et al.* (1999) made a comparative study of these neurons. This kind of neurons is characterized by the shape of their soma, which is not pyramidal, but elongated (spindle shape, indeed[38]). Their dendritic structure has been described by Watson *et al.* (2006), who singles out its very reduced number of axons (only two), which are virtually symmetric; additionally, their cellular corpus is up to 4.6 times greater than the rest of (pyramidal) neurons from the layer V. Nimchinsky *et al.*'s (1999) study of the cerebral tissue of a lot of primate and prosimian species provides the surprising results that only great apes have this kind of neurons. This gives us an approximated dating of 15–20 million years for their inrush in the history of great apes. They are located in layer V of two areas: The first one and most important regarding the number of

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[38] Therefore they are also called *spindle cells*.

VENs is the *anterior cingular cortex* (ACC), and the second is the *frontal insula* (FI), with a lower number of such neurons. Watson and collaborators coincide with Nimchinsky *et al*. (1999) when pointing out that (1) these neurons are computationally simpler than the pyramidal ones, and that (2) their location in the ACC is a sign that they could carry out tasks related with intuition, complex social relationships, vocalization and face recognition.

Cytoarchitecturally, VENs are placed in Brodmann areas 24 and 25. Recent work has discovered the presence of VENs in the human *disgranular dorsolateral prefrontal cortex*, concretely in the Brodmann area 9 (Fajardo *et al.* 2008) as it is showed in Figure 8.

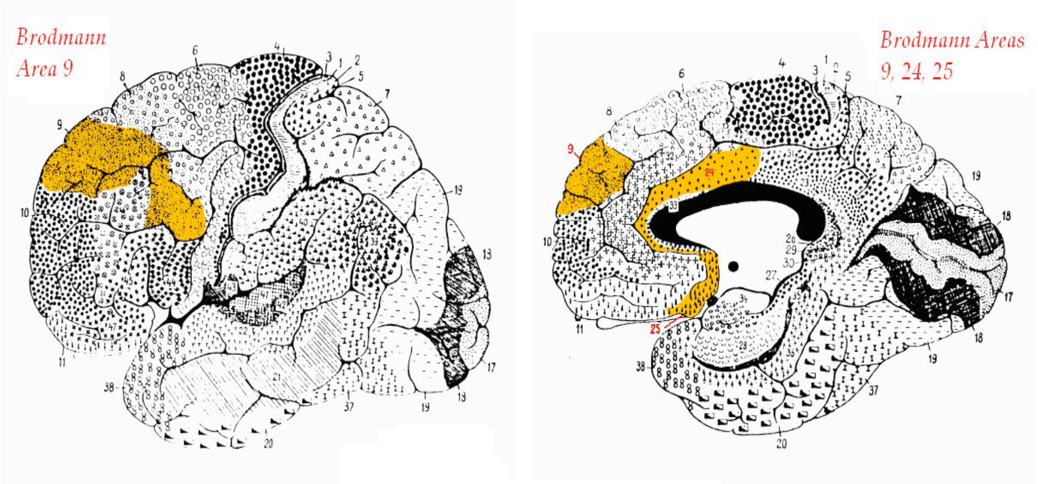

Figure 8: Human cerebral areas where von Economo neurons are located (picture modified from Wikimedia Commons)

Back to the comparative method, Nimchinsky *et al*. (1999) single out that this kind of neurons are clearly more elongated in chimpanzees and humans than in gorillas and orangutans. The authors conclude that the ACC could have been object of a particular selective pressure (not specified) in the last 15–20 millions of years.

Although previous works singled out the absence of VENs in other mammals[39] (among them several cetaceans), it has recently been discovered that VENs are present in the brains of elephants (Hakeem *et al*. 2005) and in some cetaceans (Hof & van der Gucht 2007). Curiously enough, these two groups of mammals coincide with great apes in the use of voluntary vocalizations for communication (for example, the famous whale songs) and in having a rather complex social structure. These coincidences have been considered motives of cellular specialization in these mammals (Hof *et al*. 2000), that is, a case of convergence or homoplasy.

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[39] In Hof *et al*. (1999), more than thirty mammals from different families: monotremes, marsupials, insectivores, micro- and megachiropterans, rodents, carnivores, artiodactyls, and cetaceans.

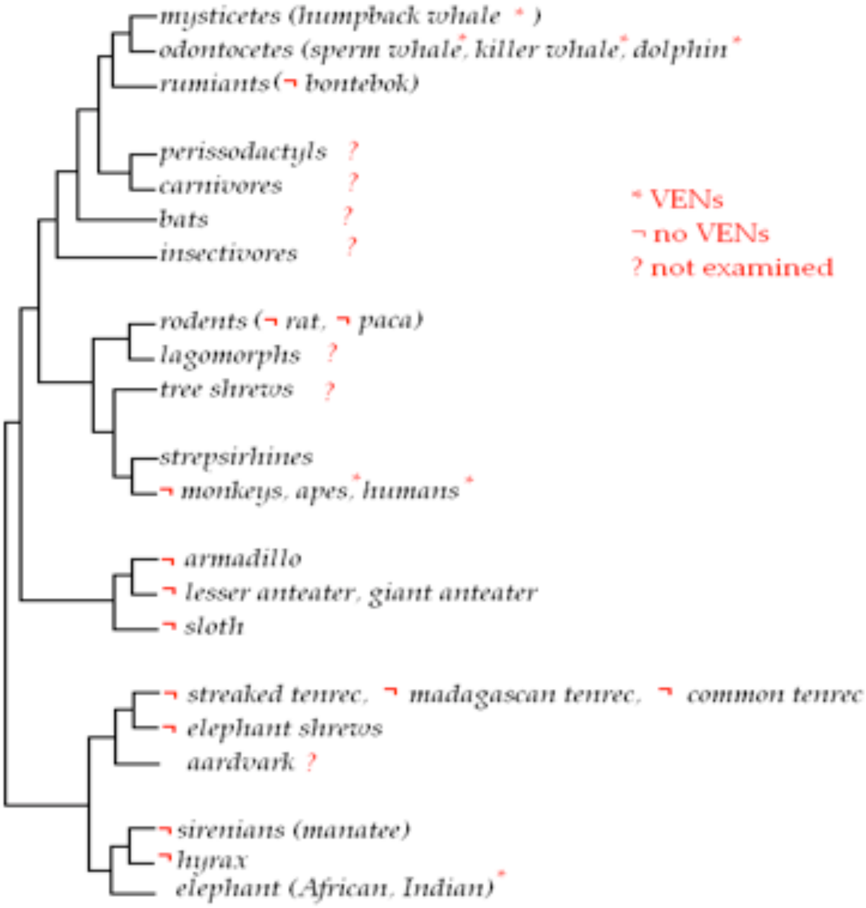

Figure 9: Phylogenetic tree with the species which either have or lack of VENs (adapted from Haakem et al. 2009)

In relation to the Neandertal man, it is interesting to note that it is possible to extract information from the application of the comparative method: The species which share this trait — the VENs — are species with a high level of social structure and the use of voluntary vocalizations in communicative actions. For the moment, no author has posited the presence of VENs as an archaic trait that has survived only in the extant species. On the contrary, the most accepted idea is rather that of an evolutionary convergence based in fitting structural principles. In other words, the morphology of VENs responds to the fact that they are more suitable for specific tasks in specific situations.

The strong correlation between the encephalization quotient has been put into question by data from pachyderms and cetaceans, though it is still outstanding in the case of great apes. The distribution of such neurons in bonobos looks especially like that of humans, being the only two species which have clusters of VENs (Hof *et al*. 2000). Hence, it doesn't seem risky to posit that Neandertals also had this kind of neurons, probably in similar number and distribution (to a greater or lesser number) to *H. sapiens*, or between the latter and the bonobo. Positing their absence in Neandertal brains would be counter-intuitive, if we take into account the current data referring the five great apes.

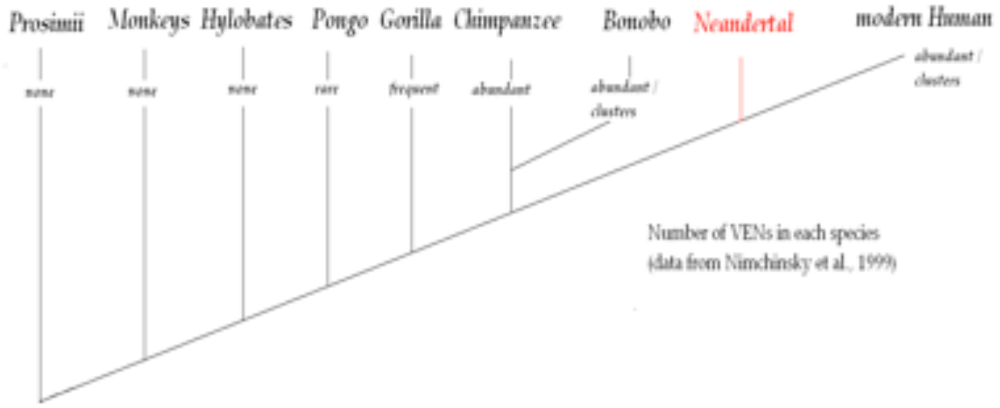

Figure 10: Phylogenetic cladogram and the distribution of VENs in primates

We must note that the role of these neurons is still not well understood, but their physical location provides us with some clues. It is known they have long-distance projections, but their exact connections still remain a mystery. For the moment, it seems that these neurons are suitable for rapid transmission of information to distant parts of the brain.

Trait	***H. neanderthalensis***	***H. sapiens***
Von Economo neurons	*	√

Table 8: Von Economo neurons in H. neanderthalensis

Next, I will briefly talk about the ACC and how functions which are attributed to it, like vocalization, can be interesting for the study of the cognitive and vocal capabilities of the Neandertal man.

4.2. The Anterior Cingular Cortex in Relation to VENs

The tentative inclusion of VENs in the neuronal landscape of *H. neanderthalensis* opens the door to several interesting research lines. Their presence in the ACC (in the area 24) indicates they have a relevant role in activities related to these areas. To the most basic ones, like breathing or cardiac rhythm, others should be added, which are actually related to emotions and vocalizations. The density of the neuronal population at the ACC is indeed low — actually, there is no layer IV in this area —, a fact that makes more conspicuous the presence of VENs in layer V. The area where VENs are placed is curiously the homologous one of an area known to be able to provoke vocalizations with some 'meaning' in squirrel monkeys (*Saimiri*) and in the voluntary fonation of macaques (Jürgens & Ploog 1970, Sutton *et al*. 1981). We will see that, in spite of the apparent differences between primates, it seems that VENs are not essential for voluntary vocalizing, but they could add emotional content to vocalizations.

Allman *et al*. (2001) have proposed that the ACC is a specialization of the neocortex and not a primitive stadium of cortical evolution[40] and, moreover, that it would participate in intuition or in pathologic cases like autism.[41] All this is consistent with what has been argued by Marino *et al*. (2007) about the cognitive abilities of cetaceans: Some cetaceans possess a complex brain and their cognition is equally complex, partially supported by VENs, also in layer V of their cetacean brains. Among other abilities, there stands out that cetaceans can recognize themselves in a mirror, can vocalize voluntarily and finally can make use of intuition. The two first abilities have also been positively proven in elephants, the other VENs mammal carriers.

All these lead me to think that the anterior cingular cortex is related to the production of vocalization with emotional content. If the presence of VENs gives us clues about some vocal abilities, diseases affecting them show us the pathological aspect that could affect vocalization: VENs are especially sensitive to fronto-temporal dementia (Seeley *et al*. 2006) and to Alzheimer (Nimchinsky *et al*. 1995). These diseases affect the three mentioned Brodman areas: 24, 25, and 9. Moreover, lesions in the ACC have been related to a kind of mutism.

If we assume that the brain of the Neandertal man had von Economo neurons at layer V in areas homologous to the above described ones, we have to assume a whole new perspective which goes far beyond that of the 'simple' production of categorized sounds. Hence, the picture of *H. neanderthalensis* becomes very different from that according to which this hominin was unable even of producing sounds voluntarily or, in the best case, his vocalizations would only be slightly better than the alarm calls of cercopithecines. On the contrary, according to my approach, it seems that Neandertals could have produced vocalizations with emotional content and, potentially, there could have been cases of mutism within this species due to dysfunction of VENs. We have also succinctly seen — since it is not my goal here — how these neurons could participate in some high cognitive processes and at the same time — in the case of pathologies — how they could be related to diseases of cognition which are traditionally considered exclusive of modern human.

Next, I will analyze the question of FOXP2, a gene that has been linked to the motor control of orofacial muscles, but also to superior order cognitive processes.

4.3. The FOXP2 Gene

Twenty years ago Hurst *et al*. (1990) described the disease of a British family —

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[40] To some extent, such conclusions are based in the alleged postnatal emergence of VENs, which was thought to start up in the fourth month of postnatal life. However, Hayashi *et al*.'s (2001) studies show that VENs appear firstly during the gestation in both humans and chimpanzees (between the weeks 32 and 35). The difference is that in humans VENs appear continuously until the fourth year of live, whereas in chimps their proliferation downs rapidly in postnatal life.

[41] Besides the above mentioned capacities, Allman *et al.* (2005) propose a hypothesis about the central role VENs could play in ACC, given that (1) they would be an important part of the circuitry responsible for intuition and (2) they are neurons that become dysfunctional in disorders in the autistic spectrum. According to this hypothesis, faulty VENs could partially be responsible for the lack of social abilities and this could lead to a lack of intuition.

under the pseudonym *KE* — which affected 50% of its members. These had speech problems and a kind of verbal dyspraxia was diagnosed. Some years later, the suspicions were confirmed: If three generations show this pathology, its origin must be hereditary.

Enard *et al*. (2002) published a letter announcing an analysis of FOXP2, a gene that codifies a protein belonging to the group of transcription factors called FOX. Enard and his team made sequences of this gene, located at the exon 7q31, and compared it with its ortologous one from a mouse. The authors saw that between *FOXP2* and *FoxP2* there are only three differentiated amino-acids.[42] Such difference is reduced to two different amino-acids when the comparison is made with chimpanzees. Moreover, the authors argued that it must be a fixed gene in the human species, given its low variation among different populations. The rest of primates share the same variant of the gene — the same allele. The modern human is the only primate who has a differentiated allele, with changes in two amino-acids. The date of the gene fixation in modern humans was calculated around 200k BP, coinciding more or less with the appearance of *H. sapiens* as species.

The family *KE* has a mutation of this gene[43] (Lai *et al*. 2001). Its effects have been directly related to their linguistic deficit by means of neuroimaging studies, which, once confronted to the results of the affected people with the non-affected people, show different patterns of activation in linguistic tasks: Affected individuals are less active in regions related with phonological processing (according to the authors this would explain the verbal dyspraxia) and in other sub-cortical areas like the basal ganglia.[44,45]

This gene was no more considered exclusive of *H. sapiens* when Krause *et al*. (2007) accomplished the difficult task of obtaining Neandertal DNA in good conditions[46] and manage to identify the gene *FOXP2* in the DNA of both Upper Paleolithic hominins. This fact changes radically the picture: If *H. neanderthalensis* had the same gene with the very same amino-acid substitutions than current

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[42] Genetic nomenclature can be a little bit confusing: (i) When talking about the gene in general, it is written FOXP2; (ii) when talking about the human variant of the gene (the allele), it is written *FOXP2*, and its protein is FOXP2; (iii) when talking about the variant of the mouse, it is written *FoxP2* and its protein is FOXP2. More information can be found in HUGO (http://www.genenames.org/guidelines.html#2.%20Gene%20symbols), the Guidelines for Human Gene Nomenclature.

[43] Specifically, a conversion G → A. Other patients who didn't belong to family *KE* had a chromosomal deletion that affects this gene (Feuk *et al*. 2006, Lennon *et al*. 2007). In the latter case the symptoms were different, though also within language disorders.

[44] As we will see, these areas are important according to Lieberman's hypothesis, which will be reviewed in section 4.5.

[45] A different kind of vocalization that has been linked to FOXP2 is that of ultrasounds: oral productions which are produced by air expiration throughout non-vibrating vocal cords. From mouse pups we know that they emit "ultrasonic whistles, clicks and harmonically structured calls of various frequency bandwidths containing varied amounts of noise" (Gaub *et al*. 2010: 391). It is also known that mothers reply to pups (Ehret 2005). Experiments with mice had pointed out that those individuals with Foxp2-KO (from 'knock out'; they are knocked out organisms, created by genetic engineering with inoperative genes) were unable of vocalize ultrasounds (Shu *et al*. 2005). However, a work that points out that pups could do that in stress conditions has been recently published (Groszer *et al*. 2008). Hence, it seems that Foxp2 is not essential for vocalizing ultrasounds (Fisher & Sharff 2009).

[46] From two individuals found by Rosas *et al*. (2006) in El Sindrón, Asturies.

modern humans, it could have got the same benefits (and damage) humans have. More interesting, though, was the debate (Benítez-Burraco *et al.* 2008, Coop *et al.* 2008) about how it was possible that both hominids shared this gene, when previous results (Green *et al.* 2006) concluded the opposite. Benítez-Burraco and collaborators (2008) argued that the scenario that would explain the origin of the mutation of *FOXP2* in Neandertals was still not sure, given the small size of the sample both Green *et al.* (2006) and Krause *et al.* (2007) counted with. Finally, a very recent publication shows results confirming the emergence of this mutation as ancestral in Neandertals, that is to say, as appearing before the split between modern humans and Neandertals (Burbano *et al.* 2010).

Trait	***H. neanderthalensis***	***H. sapiens***
FOXP2	√	√

Table 9: Gene FOXP2 in both hominins

The importance of this gene is not only due to the fact that it is a derived trait present today only in humans, but also due to its implications in motor operations aimed at vocalization. Thus, before they split off, both *H. neanderthalensis* and *H. sapiens* had incorporated a mutation in this gene that made them different from the rest of great apes which did not belong to the genus *Homo*.[47] The great similarity between both species, however, does not allow us to affirm with any certainty whether the expression of *FOXP2* was exactly the same in both species. Finally, I will recall that there are also problems and mental diseases related with a dysfunction of this gene (diseases that will not be reviewed here due to the scope of the present work): autism (Alarcón *et al.* 2002, Wassink *et al.* 2002), though very controversial (see Newbury *et al.* 2002), and schizophrenia (Levy *et al.* 2010, Sanjuan *et al.* 2005, 2006). I simply point out that the Neandertal man could have been a potential subject of such pathologies in case the supposed role the *FOXP2* gene plays therein is finally confirmed.

4.4. Mirror Neurons and Motor Theories about Language Origins

Mirror neurons have been detected in the brain of rhesus macaque (*Macaca mulatta*). They are known to fire when the monkey is observing a conspecific grasping an object as well as when the monkey grasp the object itself. When the action consists in grasping something, the action is called *transitive*. When it consists in a similar movement, though without grasping anything, the action is

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[47] I make this specification about the genus *Homo* due to the DNA proofs mentioned above about the mysterious hominid at Denisova cave (Krause *et al.* 2010). Reich *et al.* (2010, 2011) have recently analyzed nuclear DNA and mtDNA from the Denisovan. The results clearly show that Denisovans were a sister group split from Neandertals. Denisovans interbred with the new African incomers so that today some Melanesian populations still have Denisovan inheritance in their genomes. Though FOXP2 is not discussed in these works, the (expected) presence of the 'modern' allele has been confirmed. I thank Martin Kircher for confirming this piece of data. Anyway, the most recent common ancestor (either *H. heidelbergensis* or *H. antecessor*) of these hominins should also have the modern version of the gene.

called *intransitive*. These neurons have still not been detected in *H. sapiens* brains directly, though there is a considerable amount of indirect data which point to the existence of a mirror neurons system in the human brain, probably very similar to the mirror neurons system of macaques (Stamenov & Gallese 2002, for a collection of works about this kind of neurons, which also covers other non linguistic issues). The discovering of this kind of neurons along with the indirect data suggesting their existence in modern humans has led to the formulation of a hypothesis about their central role in language evolution (Arbib & Bota 2003, Arbib 2005, 2006, Rizzolatti & Arbib 1998). The idea is that this neuronal system helps us to understand motor actions like grasping objects and that, in the course of evolution, vocalizations would have been recruited by this system. This theory is supported by the localization of mirror neurons in the macaque, in the so-called area F5, which seems to be the homologous one to the Brodmann areas 44 and 45, in other words, Broca's area. As we see, the physical location triggers the whole theory: These two Brodmann areas have been related to both motor control and syntax (though the latter is still under discussion). Hence, in the case of hominids, this area would have become specialized and evolved into the current one, in order to favor the perception, control and production of vocalizations. This theory is supported by Corballis (2002a), who argues that it is not by happenstance that the motor control and the traditional linguistic areas (Wernicke and Broca's area) are located in the left hemisphere. This has been related to the lateralization of language and the difference between both hemispheres (Corballlis 1993).[48] Corballis has affirmed that, in comparison to other non-human primates, our hominin forebears were better 'pre-adapted' and had more intentional control on bodily movements than on intentional vocalizing (Corballis 2010: 27). In the same line of argument, he explains that gesture was *conventionalized*[49] in the first place and from this point "[i]n the course of human evolution, vocalizations have replaced manual acts as the main medium of language, although manual languages persist, especially in the form of signed languages invented by deaf communities" (Corballis 2010: 28). Corballis considers that this change was gradual, almost imperceptible. An explanation of this evolutionary process has been put forward in Gentilucci & Corballis (2006).[50] Nonetheless, in my view, this proposal still has several pitfalls. The authors rightly point out that the cortical control on speech is a fundamental element. However, their hypothesis relies on arguments that are not supported anymore by the last findings in evolutionary studies, as I have noted elsewhere (Barceló-Coblijn 2011). For example, the emergence of *bipedalism*, which would have freed the hands (Gentilucci & Corballis 2006: 951), has serious problems, since Lovejoy and colleagues (Lovejoy, Owen, Simpson *et al.* 2009, Lovejoy, Owen, Latimer *et al.* 2009, Lovejoy, Owen, Suwa *et al.* 2009) show that (1) *Ardipithecus ramidus* was biped, and (2) *knuckle walking* is a derived trait independently appeared in both

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[48] Lateralization has been confirmed in gorillas (Cantalupo & Hopkins 2001) and in chimps (Hopkins & Cantalupo, 2004; Hopkins *et al*. 2007).

[49] Notion borrowed from Burling (1999).

[50] See section 5, on the problems of dating the emergence of speech. I thank Michael Corballis for reminding me that Gentilucci & Corballis (2006) have already addressed the question of the gradual evolution of speech.

chimps and gorillas. Gentilucci & Corballis (2006: 953) assume Liberman *et al.*'s (1967) theory of speech as gesture, considered quite controversial by many scholars. The authors then assume that "the basic unit of speech, known as phoneme, does not exist as a discrete unit in the acoustic signal". I prefer the separation of phonemes as abstract psychological entities belonging to phonology, and phones or sounds as the units of speech. Some emphasis is also put on signed languages. However we must not forget that these languages are the exception rather than the rule. In fact, today there are many. Most of them emerge due to pathological reasons and it is not until recently in our history that important communities of deaf people have come together and made possible the emergence of such languages. Altogether there seems to be indications of the plasticity of the human brain which makes possible the externalization of language through other non-oral channels. Along Hockett's lines, if our evolutionary history had been different and we had evolved a prehensile tail like some American monkeys, we surely would have add this 'fifth limb' to our external expressions, as well as in signed languages too (Hockett 1964: 144). Another argument is the exclusivity of FOXP2 gene, nowadays problematic due to the aforementioned reasons. Finally, Gentilucci & Corballis (2006: 954) say that "[t]he course of events in the evolution of language may be paralleled by those in the development of language in children". I really think that today we have to resist arguments based on the idea that "ontogeny recapitulates phylogeny", an apparently superseded debate (Gomila 2010). The connections between hands and mouth are there. This is something clear, at least at the neurological level. However, what it is not so clear is why one should posit a linguistic stadium were signs are fully dominant, when mammals' main communication channel is oral. Indeed primates stand out in oral signs along with other gestures. Perhaps we should not posit stages without sounds or without signs, but stages where both externalization possibilities are present, complementing each other. Indeed multimodal approaches like this are all but new (Hockett 1978: 295–300).

4.5. The Fitting of Neandertals in the Motor Theories

Linguistic motor theories assume the difficulty of merging in a whole hypothesis the origin of speech and the origin of language. The principal problem we find in such theories is that they do not take into account the rest of hominids or the rest of species, or that they do so only partially. Rizzolatti & Arbib (1998) argue that a plausible hypothesis would be that the transition from gesture to vocalization has taken place in the transition from the genus *Australopithecus* to the genus *Homo*. The first problem comes from paleoanthropology, a discipline which does not ensure that current humans are descendants of australopithecines. Besides this rather intra-theoretical reef, an additional problem is represented by the species *H. neanderthalensis*. Motor theories of language evolution or on speech evolution have been built onto the basis represented by *H. sapiens*, partially because the Neandertal man was far from him, as far as cognition and available data are concerned. Currently, however, we know that hybridization between anatomically modern humans and Neandertals, although rare and not generally practiced, was possible in a biological sense and, furthermore, not condemned to sterility (Green *et al*. 2010). A recent work shows that this was also the case between *H. sapiens* and Denisovans — an Asiatic hominid branch separated from Neandertals after leaving Africa (Reich *et al*. 2010, 2011).

Corballis' theory is always based on the argument that *H. sapiens* were capable of using the mirror neurons system in order to produce controlled voluntary vocalizations. Altogether it seems, rather on the contrary, that we were not the only species of hominins able to do that. It would be quite surprising that precisely Neandertals — who could potentially have the neuronal and physical elements I have mentioned — were a species of hominin incapable of controlling the vocalizations because they did not reach the mirror system with the alleged incorporation of vocalizations into this system. It is possible to conceive differences between Neandertal cognition and *H. sapiens* cognition and, in fact, there has always been a trend defending such differences, which would have tipped the balance in favor of the supremacy of the African hominin (*H. sapiens)* over the European one (*H. neanderthalensis*). The Neandertal man found a comfortable enough ecological niche in Europe and near Asia and occupied this area for almost half a million years. This would have been a period of 'cultural stasis' (Ballari *et al*. 2008). This stasis can be interpreted in two different ways: (i) a lack of stimuli or pressures that favor variation or (ii) an incapability to develop more complex technology and material culture. This issue is currently debated. I just point out that differences in cognition should not be due to the lack of mirror neurons, if only because of phylogenetic proximity. However, if, as Corballis has always proposed (2002a, 2002b, 2009, 2010), the transition from the gestural system to the vocal system freed the hands for other tasks, the argument is equally valid for Neandertals, who are known to produce a huge quantity of lithic and other material culture.[51]

In short, I do not find any strong argument to move away *H. neanderthalensis* from the primates with mirror neurons, nor arguments against their potential mastery and use of mirror neurons which, according to Corballis, would have originated language as we find it today in *H. sapiens*.

On the other hand, although it is difficult to fit the Neandertal man in the current Corballis' theory, as it is right now, without substantial modifications, we can perfectly concede some room for variation in the configuration of the neuronal cables and, especially, variation in the configuration of the mirror neuron system that Neandertal could have had. Then, some differences at that level could certainly have divided both hominins. Not in vain both hominids were phylogenetically separated for hundreds of thousands of years.

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[51] Corballis recalls that most archaeologists, though, say that the sophistication of material culture in *H. sapiens* vastly exceeded that of the Neandertals from about 75.000 years ago. I agree with that. The point is that the quantity and quality of Neandertal material culture (MC) is quite different in both quality and quantity from the Oldowan MC or even the Acheulean MC. New evidences of quite modern behavior are appearing day by day, so that it is difficult to speak about species-specific limitations: It seems that Neandertals purposely collected feathers (Peresani *et al*. 2011), had beds and hearths (Cabanes *et al*. 2010) and knew about pigments (d'Errico *et al*. 2010). Of course, one can easily detect qualitative differences between objects arguably left by Neandertals and objects made by Sapiens.

Trait	***H. neanderthalensis***	***H. sapiens***
Mirror neurons	*	√

Table 10: Mirror neurons probably were present in the Neandertal man's brain

The last of motor theories to consider, is Lieberman's, who argues that the 'key factor' in language evolution would be the configuration of the basal ganglia (Lieberman 2002). As I have already mentioned above, this subcortical structure is affected by the gene *FOXP2* when it has a mutation like the one of the *KE* family. One notable difference between Lieberman's and Corballis' theory — besides the fact that each author identifies a different element of the cerebral structure as the responsible one — is that Lieberman incorporates subcortical structures, like the basal ganglia, to the already superseded theory on both the areas of Wernicke and Broca.[52] Lieberman reminds us that an aphasia of Broca — an argument used by Corballis — is never only cortical, but also and necessarily subcortical. Therefore, Lieberman considers that the human capacity for creating motor sequences — vocalizations among them — would have been the basis upon which syntax developed. Enard *et al.*'s (2009) experiments show modifycations in the neurons that make use of dopamine as a neurotransmitter, and in the basal ganglia.[53] Lieberman considers that this supports his theory (Lieberman 2009). However, as in Corballis' or Rizzolatti and Arbib's model, the Neandertal does not fit there. This hominin has also the 'human' version of the gene. Hence, it is expected that its expression would also have affected somehow the basal ganglia of Neandertals in the same way it affects the basal ganglia of the modern humans and mice. The only argument Lieberman uses against the inclusion of *H. neanderthalensis* in his theory is, in my view, null and void, because it is grounded in the fact that *H. sapiens'* tongue is too elongated for the Neandertal cavity (Lieberman 2009: 802). He does not offer instead any argument about why basal ganglia would have not done the same tasks of sequencing in the brain of both hominins.

The great contribution of these motor theories on language evolution is that they furnish us with new data about brain functioning regarding linguistic activities. Moreover, they show us how the brain behaves as an extremely complex machine where the elements interact with each other. Nevertheless, until these theories do not abandon the strategy of placing both their starting and finishing points in the *H. sapiens* species, that is to say, until they integrate the rest of organisms (or at least the closest hominins), these theories will never be complete, because there will always be new data from one or other species calling into question the alleged specificity of the trait in question (be it mirror neurons or basal ganglia). In my view, none of these theories invalidate the inclusion of

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[52] Other authors have also pointed out that this duality and localization of the linguistic areas is too simple and that the evidences in neurolinguistics point since long ago to a net of areas integrated in one only system. Activations in both hemispheres in several linguistic tasks suggest that the model needs a revision and it must be approached from the point of view of complexity; it also seems that this would be a counterpoint to the most classical theory of modularity of mind. See Stowe *et al.* (2005) for a revision in depth of these questions.

[53] Three kinds of mice are compared: a mouse with its natural FoxP2, a second mouse with the (human) FOXP2, and a third mouse with FOXP2-KO (inoperative gene; see fn. 45).

the Neandertal within them, rather on the contrary they need it, along with the concomitant modification not only of the theory but also of the perspective they have on the figure of *H. sapiens*, as a virtually extraordinary species (regarding its cognitive capabilities).

5. On the Dating of Speech

Dating the 'emergence' of the vocal tract is something quite difficult because it has been progressive. Data regarding the vocal tract and the neuronal apparatus that supports it are more conclusive: During the process of *hominization*, there has been a continuous stream of changes and exaptations that has flowed into the vocal tract we have nowadays. If *modern* is linked to *H. sapiens*, then the answer is easy: The emergence itself of this species marks the starting point of this trait (the same could be said of Neandertal 'speech'). On the other hand, if *modern* means the capability to emit vocalizations which have previously been analyzed by a system which is able to put elements in order, to sequence and to categorize sounds, then we must be cautious, because other species (not only primates) have some of these aspects as their own, and it seems that such species have acquired some of these traits from the ancestors humans and non-humans have in common.

In the same sense, if we refer to vocalization in *H. sapiens*, we have to take into account that many of the factors that intervene therein, have been appearing in the long process of *hominization* and that they could perfectly be shared with other hominids (see Figure 3). To date, a gradual process like this is very difficult.

The gradual perspective is coherent with data that have been found thanks to the comparative method, in such a way that they indicate a previous stadium of phylogenetic unity (e.g., the air sacs) or of evolutionary convergence (e.g., von Economo neurons). In this sense I coincide with Fitch, Lieberman or Corballis, though my approach tries to integrate what we know from different fields of study. Coincidences between authors in this respect are few indeed: Lieberman (2007) shows his disagreement with Corballis' (2002b) datations, who places 50k BP the emergence not only of modern speech, but also of the grammatical capacity all humans around the world share. I agree with Lieberman (2007) that this seems to be impossible: the datation of humans' arrival to Australia — as confirmed, by example, by studies of mitochondrial DNA (Cann *et al.* 1983) —, or the artifacts with inscriptions based in geometrical patterns found in South Africa (Henshilwood *et al*. 2002), suggest that, as it has been argued in Barceló-Coblijn (in press), we must move back both capabilities to a temporal point previous to the out of Africa. Dates about the 'emergence of vocalization' used by Corballis have changed in time: 50k BP (Corballis 2002b), qualified by Lieberman as "virtually impossible" (Lieberman 2007: 559); 200k — 170k[54] BP (Corballis 2009b), this time taking into account Lieberman (1998); finally this author has chosen 170k BP as the most probable dating for the appearance of vocalization (Corballis 2010). These datations would not be especially relevant but for two reasons: (1) If

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[54] In the abstract it says 170.100, probably a misprint.

we talk about a gradual process of biological evolution, how can limits be established? It is not clear why we should select a datation, when the trait evolves so slowly and, hence, with very diffused limits; (2) part of Corballis' arguments are based on the FOXP2 gene. For this gene, the author sees as more feasible the dating Coop *et al*. (2008) offered, around 42k BP (the authors argued contamination in Enard *et al*.'s 2002 work). This seems unlikely, as we have already seen when talking about FOXP2. The last results (Burbano *et al*. 2010, Green *et al*. 2010, Reich *et al*. 2011) point to two important facts: (i) the dating of the appearance of *FOXP2* with two different amino-acids regarding the *FoxP2* gene of great apes would be at least previous to the split between *H. sapiens* and *H. neanderthalensis*; (ii) the hybridization with Neandertals and Denisovans has been proven, though, as above mentioned, to be of the *introgressive* type. So, it has been proven the hypothesis that Neandertal would have not obtained the derived *FOXP2* due to interbreeding with *H. sapiens*.

6. Conclusions: On the Capabilities for Vocalization of *H. Neanderthalensis*

In the present work I have gathered several pieces of information from different areas of research, which have a common nexus: All of them tell us something about vocalization. The traits I have analyzed, now gathered in Table 11, show us a richer picture of the potential vocal capabilities of Neandertals than the traditional one, only based in paleomorphic studies or computational studies based in such morphology. In this sense, I have shown how, as claimed by Fitch (2000), the comparative method is a powerful tool in order to infer information, and even more powerful if we can combine it with techniques of genetic analysis, neuronal histologic analysis, or neuroimaging.

Trait	***H. neanderthalensis***	***H. sapiens***
Low Larynx	*	√
Vocal Tract	*	√
Von Economo Neurons	*	√
Derived hyoid bone	√	√
Lack of air sacs	√	√
Mirror neurons	*	√
FOXP2	√	√
Auditory Capacity 2-4 kHz	*	√
Perception of formants	*	√
Micro-abilities related to vocalization	*	√

Table 11: Summary of traits related to speech

Looking at Table 11, my conclusion can only be one: Current data point to the fact that *H. neanderthalensis* (among all, the phylogenetically closest hominin to *H. sapiens*) had a capability for vocalizations which could be qualified as sophisticated. In effect, not all the traits we see in the table are confirmed, though I think I have shown arguments enough to take them into account. It is possible that some doubts will always remain in some cases, but, let me insist, the data from many fields of study lead me to believe that all of them were part of the Neandertal man.

Thus, Neandertals would have had a physical structure which would not disable them in order to emit articulate sounds, very similar to that we modern humans produce when talking. Their perceptive capacities seem to go in the same direction: They would have been able to identify sounds, the formants in the case of vocalizations, as it seems that they had a 'modern' ear.

A related question is whether or not this species could articulate words as these are understood in the context of the modern human language; whether or not they were able to inflect words or make chains of words until sentences are reached, or produce a derived word from another. None of the proofs shown here point to such a conclusion. I do not think that from a study about the physical and perceptive capacity like the one I have offered it is possible to draw the conclusion that this species already had a syntactic capability or even a morphological capability like that current *H. sapiens* show. I think that these capabilities are essentially bound to computational capacities proven until now only in *H. sapiens*.

Noam Chomsky has defended for more than forty years the idea that syntax has a (very powerful) structural component, which allows us to build linguistic structures virtually limitless or only limited by external components — like the working memory, or the pulmonary capacity, which are elements that put a 'physical' limit to a computational system theoretically so powerful. Such a computational system, in case of being real, cannot be measured with the data gathered here, though these data can open new research lines. For example, an interesting approach is the one developed by Camps & Uriagereka (2006). They try to test the presence of syntactical capabilities from non-linguistic activities which could follow a recursive patterning, like knotting. Making knots, but also other activities like basket weaving, suggests the participation of a mind ready to sequence complex patterns like these (Barceló-Coblijn & Gomila, in press).

The goal of this work was to go deeply into the biological aspects that seem to support voluntary, articulated vocalization in Neandertals. I have also shown which of them could be attributed to the Neandertal man and to which extent. Future research will confirm some aspects that remain doubtful and perhaps also single out new ones, regarding both *H. neanderthalensis* and *H. sapiens*, in order to shed some light onto the biological history of these two hominins, and figure out which cognitive differences — if any — separate and distinguish them.

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★ BRIEFS ★

Recursion as Derivational Layering: An Amendment to Zwart

Andreas Trotzke & Antje Lahne

1. Introduction

In a recent issue of *Biolinguistics*, Zwart (2011a) argues against the prominent view that recursion in natural language should be understood in terms of syntactic embedding and proposes that recursion is instead evidenced by derivational layering: The more derivational cycles (= layers) are needed to derive a structure S, the more recursive S is. Derivational layering is also taken as a measure for complexity: The more derivational layers are needed to derive a structure S, the more complex S is. According to Zwart, this model correctly predicts that recursive center embedding of a certain type (which we will call type A) is more recursive and thus more complex than two other types of center embedding (types B and C) as its computation involves more derivational layers.

In this amendment, we will first point out how the notion of recursion within the derivational layering approach differs from other prominent concepts of recursion. In Section 3, we will demonstrate that the approach does not make the prediction assumed by Zwart; rather, the model predicts that type B center embedding structures involve an equally high number of derivational layers as type A structures. Section 4 concludes and points out the consequences of this amendment for theories based on derivational layering as an indicator for recursion.

2. Recursion as Derivational Layering

In current minimalist theory, recursion is encapsulated in the basic operation Merge. As (1) illustrates, Merge recursively generates syntactic objects, since it takes the output of a previous application (1a) as part of its input when running the next time (1b):

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(1) a.

$$\begin{tikzcd} & \{\alpha\{\alpha,\beta\}\} & \\ \alpha \arrow[rr, leftrightarrow] & \arrow[u] & \text{Merge}\ \beta \end{tikzcd}$$

b.

$$\begin{tikzcd} & \{\alpha\{\gamma\{\alpha\{\alpha,\beta\}\}\}\} & \\ \{\alpha\{\alpha,\beta\}\} \arrow[rr, leftrightarrow] & \arrow[u] & \text{Merge}\ \gamma \end{tikzcd}$$

Due to the category-independent, derivational nature of this approach to structure-building, understanding recursion in terms of Merge renders most of the recent debates on the universality of syntactic recursion a non-issue because this notion of recursion implies that "a language that lacks recursion would be considerably [...] exotic. No sentence in such a language could contain more than two words" (Nevins *et al.* 2009: 366, n. 11).

Another prominent notion of recursion does not make use of the relational understanding of projection levels. This concept has a strong representational aspect in that it refers to specific, categorially defined types of syntactic embedding (cf. Everett 2005 and subsequent work). For instance, the right-branching structure in (2) is said to imply recursion, since a CP is embedded in another CP:

(2) [$_{\text{CP}}$ John thinks [$_{\text{CP}}$ that Mary said [$_{\text{CP}}$ that it was raining]]]

Zwart (2011a) diverges from both notions of recursion. As opposed to the 'recursion-as-Merge' view, he proposes the operation Split-Merge, which works iteratively rather than recursively. That is, according to Zwart's approach, the basic generative procedure can be carried out by a finite-state machine because the derived sequences are computed in a strictly local manner: by simple iteration (cf. Abelson & Sussman 1984: 29–33). More precisely, Split-Merge works from top down and from left to right, iteratively splitting off identified elements from the residue set, that is, the Numeration N (cf. Zwart 2009):

(3) Split-Merge

a. $N = \{\alpha, \beta, \gamma\}$
b. Merge: split $x \in N$ off from N, yielding $\langle x, N - x \rangle$
c. Merge α yielding $\langle \alpha\ \{\beta, \gamma\} \rangle$
d. Merge β yielding $\langle \alpha\ \langle \beta\ \{\gamma\} \rangle \rangle$
e. Merge γ yielding $\langle \alpha\ \langle \beta\ \langle \gamma\ \{\ \ \} \rangle \rangle \rangle$

At each step in the derivation, Split-Merge turns the numeration into an ordered pair $\langle x, y \rangle$, where $x \in N$ and $y = (N - x)$. A general condition on the derivation is that at each derivational step, the residue { _ } must form a constituent, in the sense of meeting any of the classical constituency tests (substitution, ellipsis, movement/topicalization, clefting, and coordination).[1] It follows that, as soon as a sequence involves a complex non-complement (as in [*The dog*] *barks*), this sequence cannot be derived by splitting off the leftmost element of the complex non-complement, since the residue ({*dog barks*}) does not form a constituent.

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[1] Of course, this condition must be seen in the context of more fundamental assumptions of Zwart's top-down approach (such as the relation between N and the workspace). Since a full explication of the model would take us too far afield, we refer the reader to the programmatic paper Zwart (2009).

Therefore, complex non-complements must be derived in a separate derivational cycle, that is, a subderivation of [*the dog*], which is then integrated into the Numeration as an atom (cf. Uriagereka 1999).[2] In the model described here, each derivational cycle is called *derivational layer*.[3]

In opposition to the 'recursion-as-syntactic-embedding' view Zwart (2011a) claims that recursion is not necessarily evidenced by embedding; rather, he argues that recursion in language should be understood in terms of derivational layering.[4] According to Zwart, derivations are layered because the output of a previous derivation can function as one single item in the numeration of the next derivation. This derivational procedure is recursive in the sense that its output is part of the input of the same procedure. Crucially, if a sequence is derived in one derivational layer (as it is in example (2)), it does not imply recursion. In contrast, simple clauses containing complex specifiers (like *The dog barks*) imply recursion.

Assuming this notion of recursion, Zwart argues throughout his paper "that we cannot tell that a grammar is recursive by simply looking at its output; we have to know about the generative procedure" (Zwart 2011a: 43). In what follows, we demonstrate that in one case, the paper actually does draw conclusions that refer to the output (representation) rather than to the procedure (derivation). In other words, within the derivational layering model, recursion is understood in terms of embedding again, not in terms of derivational layering. The consequence of this change of perspective is that the derivational layering model is attributed a prediction about the complexity of center-embedded clauses. We show, by strictly adhering to the derivational procedure, that the model does actually not make this prediction.

3. Amendment

Zwart (2011a) makes a statement about the grades of complexity of different kinds of center embedding. The examples in (5) exemplify the core data:

(5) a. The dog **the cat** *the man kicked* **bit** barked. (A)
b. The dog t**hat the cat bit** *that the man kicked* barked. (25) (B)
c. The dog **that bit the man** *that kicked the cat* barked. (26) (C)

The examples in (5) differ in that (5a) involves self-embedding, whereas (5b) and

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[2] The mechanism described here yields the effect of the Extension Condition (cf. Chomsky 1995) while actually stating it from a top-down perspective: According to the Extension Condition, "Merge always applies at the simplest possible form: at the root" (Chomsky 1995: 248). Accordingly, *dog* and *barks* cannot be merged, excluding *the*, as *the* would then have to be merged with the non-root node *dog*. In the Split-Merge model, *the* cannot be split off from the residue {*dog barks*}, as the residue does not form a constituent.

[3] Interestingly, the derivational layering approach has a feature that moves it close to so-called 'dynamic' approaches to phases (as proposed in Svenonius 2001, den Dikken 2007, Gallego & Uriagereka 2007, Gallego 2010): Derivational layers are not defined in terms of syntactic categories; for example, a CP may be the output of a separate derivation layer, but it does not have to be (cf. (2)). Thus, phase status is not a fixed property of certain heads (such as C).

[4] The layers themselves, as already mentioned above, are generated by the operation Split-Merge, which works iteratively rather than recursively.

(5c) contain right-branching.[5] Given these output-related observations, Zwart claims that type A center embedding is more complex (i.e. contains more derivational layers) than type B and type C, which are equal in complexity. In particular, he states that "the difference may be accounted for by the fact that (25) and (26) contain fewer derivational layers (due to right-branch embedding) and hence less recursion" (Zwart 2011a: 49). However, when running through the computation, we noticed that type B does not contain fewer derivational layers than type A; (6) shows the results of applying Split-Merge to all three types (see the appendix for the detailed derivations):[6]

(6) a. [[[The dog] [[**the cat**] [*the man*] *kicked*] **bit**] barked] A: 6 layers
b. [[[The dog] **that** [[**the cat**] **bit**] *that* [*the man*] *kicked*] barked] B: 6 layers
c. [[[The dog] **that bit** [**the man**] *that kicked the cat*] barked] C: 4 layers

As (6) illustrates, type A center embedding patterns with type B in that they involve an equal number of derivational layers; they are thus equally recursive and equally complex, as opposed to type C, which is less complex. Of course, where type B has complex subjects ([*the cat*], [*the man*]), type C has gaps (as the subjects are relativized: *that Ø bit the man*, *that Ø kicked the cat*). But the distinction between B and C concerning their derivational complexity cannot be removed by replacing *the cat* etc. by, for instance, *cats*, as suggested by Jan-Wouter Zwart (p.c.), because then we arrive at:

(7) a. [[[The dog] [cats people kick] bite] barked] A: 4 layers
b. [[[The dog] that [cats bite] that people kick] barked] B: 4 layers
c. [[[The dog] that bites people that kick cats] barked] C: 3 layers

What is instead relevant here is the head-finality of type B. So, both [*the cat bit*] and [*cats bite*] constitute separate subderivations, since they imply the following derivational steps, where *bit that the man kicked* and *bite that people kick* do not form constituents:

(8) a. * ⟨ [the dog] ⟨ that ⟨ [the cat] { bit, that, the, man, kicked } ⟩ ⟩ ⟩ (6b)
b. * ⟨ [the dog] ⟨ that ⟨ cats { bite, that, people, kick } ⟩ ⟩ ⟩ (7b)

In English, of course, this head-finality is the result of fronting the object. Unfortunately, it is not clear so far how to implement movement phenomena in the Split-Merge approach. Zwart (2009: 181) concedes that in this respect the derivational layering system is very tentative and that "[o]ther approaches, mimicking movement, might also be pursued". Accordingly, although we have shown that Zwart's claim that type A is more complex than type B cannot be upheld without

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[5] To facilitate reading, we call the center embedded configuration that contains self-embedding *type A* center embedding (5a), and those that do not contain self-embedded relative clauses *type B* (5b) and *type C* (5c) center embeddings, respectively.

[6] Note that the bracketing does not indicate constituency but derivational layers. We follow Zwart in using this notation.

further assumptions about movement, the fact that the Split-Merge model is very preliminary in this regard makes the claim a moving target for us. However, when turning to German, we will show now that Zwart is forced to assume an additional layer in type B structures anyway, and thus concurs with the results we find for type A and type B structures in this language:

(9) *German*

a. [[Fido [der Hans der Eva liebt] beißt] bellt] A: 3 layers
Fido who Hans who Eva loves bites barks
'Fido, who bites Hans, who loves Eva, barks.'

b. [[Fido der [Hans beißt] der Eva liebt] bellt] B: 3 layers
Fido who Hans bites who Eva loves barks
'Fido, who bites Hans, who loves Eva, barks.'

Zwart (2011b) assumes, in contrast to Kayne (1994), that not all head-finality is 'pseudo-finality' and that, therefore, languages like Dutch and German show head-final linear order in relative clauses without any movement. Crucially now, Zwart claims that this head-final order implies a separate derivation, but not because of structural (configurational) reasons; rather, he proposes a separate derivation because of 'interpretive criteria'. Note that Zwart (2009: 173) assumes that there are two criteria for determining that an element is the output of a subderivation: (i) configurational criteria (the condition of yielding constituents, as sketched above in section 2) and (ii) interpretive criteria (showing interface effects). Zwart (2011b) demonstrates, based on many syntactic observations from Dutch, that head-final linear order (in Dutch) shows interface effects (idiosyncratic sound/meaning pairings), and that, therefore, head-finality implies an additional derivational layer. Many of the Dutch facts also hold for German. For example, nonspecific indefinites like *ein* lose their nonspecific reading as soon as they are not adjacent to the verb:

(10) *German*

a. … dass Hans selten ein Buch liest
that Hans rarely a book read
'that Hans rarely reads a book.'

b. … dass Hans ein Buch selten liest
that Hans a book rarely read
'… that Hans rarely reads a book.'
(= 'What Hans rarely does to a book is read it.')

Based on such facts, Zwart (2011b: 115) proposes the generalization that "[h]ead-finality is a linguistic sign, signaling derivation layering".[7] If this generalization

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[7] In accordance with a reviewer, we would like to point out that this looks more like the beginning of a theory and not like a generalization. In particular, Zwart states that head-finality is a sign for the presence of an additional layer without formally implementing how exactly the layer is actually triggered.

holds, then type B center-embedding (at least in languages like German) involves an additional derivational layer anyway, due to 'interpretive criteria', so that type A and type B involve an equal number of derivational layers and are thus equally recursive. However, we have demonstrated that type B implies the same amount of subderivations because of *configurational criteria*, namely because of the non-local dependency between antecedent and relative pronoun (i.e., when the antecedent [*the cat*] is split off, the residue *bit that the man kicked* does not form a constituent).

In the last section of this amendment, we will point out consequences of this finding for theories based on derivational layering as an indicator for recursion.

4. Conclusion and Outlook

In this amendment, we first have shown how the derivational layering model differs from two other prominent notions of recursion. After that, we have demonstrated that the derivational layering approach does not make the prediction assumed by Zwart; rather, the model predicts that type B center embedding structures involve an equally high number of derivational layers as type A structures. To make this finding immune to rejections that could be raised due to the somewhat vague status of syntactic movement in Zwart's theory, we have shown that the same ratio of derivational layers between type A and type B structures holds for German. We ended with the observation that Zwart assumes that type B structures imply the same amount of layers as type A structures due to interpretive criteria, while we have shown that this is the case due to purely configurational criteria. As a last point, let us briefly sketch why it might be important to note that type A and type B structures are equally recursive because of configurational criteria.

First of all, Zwart (2011a) suggests drawing inferences between the amount of derivational layers and processing constraints of center-embedded structures. According to him, "center-embedding cannot be performed indefinitely, unlike right-branch embedding […]. It seems, then, that recursion (as understood here) comes with a cost" (Zwart 2011a: 49). Of course, this difference in 'cost' (memory load) refers to the configurational situation that type A involves two non-local dependencies between subjects and the respective verbs, while type B and C only involve one dependency of this type. In general, we find nothing objectionable about drawing those inferences, especially since Split-Merge works top-down and from left to right, and it is well known that this derivational perspective can contribute to a theory of the interface between grammar and the parser (cf. Phillips 1996, 2003, Weinberg 1999). However, coming back to our finding again that type B is equally recursive as type A, Split-Merge gives significance to the non-local dependency between antecedents and relative pronoun and thereby reflects the psycholinguistic insight that this relation plays a significant role in affecting relative clause extraposition in German (cf. Shannon 1992, Uszkoreit *et al.* 1998). So, while the general plausibility of Split-Merge concerning performance preferences can be maintained, we arrive at a more fine-grained picture than Zwart.

With this issue in mind, we would like to conclude by suggesting a distinction between a notion of recursion that is related to basic ('atomic') structure building and a more global notion of recursion that refers to the whole (sub-) derivation. In particular, there is good reason to assume that the grammar is recursive in a more elementary sense because "[t]he fact that Merge iterates without limit is a property at least of LIs […]. EF [edge feature] articulates the fact that Merge is unbounded, that language is a recursive infinite system of a particular kind" (Chomsky 2008: 139). We propose that this notion coexists with the concept of recursion exemplified by Zwart's approach of derivational layering. This notion of recursion is compatible with other recent approaches within minimalism: A widely assumed view is that Spell-Out is a cyclic procedure targeting the complements of (certain) heads. Thus, after Spell-Out, the derivation is split up into chunks reduced to their head and their left edge. These structural primitives are inserted in the numeration for the next derivational step. This property of the grammar has already led other scholars to claim, although for different reasons than Zwart, that recursivity arises from the cyclicity of derivations, or, in other words, that recursion is organized 'phasally' (cf. especially Arsenijević & Hinzen 2010, Hinzen & Arsenijević to appear). Given that recursion, in these approaches, is dealt with as an interface phenomenon, it is reasonable to assume that this form of recursion is (at least partly) subject to principles that belong to external components like the performance systems (cf. Trotzke *et al.* submitted). In this regard, it is not surprising at all that type B structures are more complex than type C structures and (at least regarding the ratio of derivational layers) equally complex as type A structures, given the psycholinguistic insight mentioned above and given that, at least in German, the difference between type A and type B structures concerning performance preferences seems to be less significant than widely assumed (cf. Bader 2011).

In sum, understanding recursion as derivational layering may point toward a notion of recursion that can be informed by processing data, and thus this perspective offers a promising interdisciplinary domain of research within the biolinguistic approach to language.

Appendix

This appendix provides the detailed derivations of all three center-embedding types given in (6); for further explication, please contact the authors.

A. Type A: 6 layers

(6) a. [[[The dog] [[**the cat**] [*the man*] *kicked*] **bit**] barked]

Derivation:

(A1) a. N = {the, dog, the, cat, the, man, kicked, bit, barked}
b. * ⟨the {dog, the, cat, the, man, kicked, bit, barked}⟩

(A2) a. N = {the, dog}
b. ⟨the {dog}⟩
c. ⟨the ⟨dog { }⟩⟩ => derivational layer

(A3) a. N = {[the dog], the, cat, the, man, kicked, bit, barked}
b. * ⟨[the dog] {the, cat, the, man, kicked, bit, barked}⟩

(A4) a. N = {[the dog], the, cat, the, man, kicked, bit}
b. ⟨[the dog] {the, cat, the, man, kicked, bit}⟩
c. * ⟨[the dog] ⟨the {cat, the, man, kicked, bit}⟩⟩

(A5) a. N = {the, cat}
b. ⟨the {cat}⟩
c. ⟨the ⟨cat { }⟩⟩ => derivational layer

(A6) a. N = {[the dog], [the cat], the, man, kicked, bit}
b. ⟨[the dog] {[the cat], the, man, kicked, bit}⟩
c. * ⟨[the dog] ⟨[the cat] {the, man, kicked, bit}⟩⟩

(A7) a. N = {[the cat], the, man, kicked}
b. ⟨[the cat] {the, man, kicked}⟩
c. * ⟨[the cat] ⟨the {man, kicked}⟩⟩

(A8) a. N = {the, man}
b. ⟨the {man}⟩
c. ⟨the ⟨man { }⟩⟩ => derivational layer

(A9) a. N = {[the cat], [the man], kicked}
b. ⟨[the cat] {[the man], kicked}⟩
c. ⟨[the cat] ⟨[the man] {kicked}⟩⟩
d. ⟨[the cat] ⟨[the man] ⟨kicked { }⟩⟩⟩ => derivational layer

(A10) a. N = {[the dog], [[the cat] [the man] kicked], bit}
b. ⟨[the dog] {[[the cat] [the man] kicked], bit}⟩
c. ⟨[the dog] ⟨[[the cat] [the man] kicked] {bit}⟩⟩
d. ⟨[the dog] ⟨[[the cat] [the man] kicked] ⟨bit { }⟩⟩⟩
=> derivational layer

(A11) a. N = {[[the dog] [[the cat] [the man] kicked] bit], barked}
b. ⟨[[the dog] [[the cat] [the man] kicked] bit] {barked}⟩
c. ⟨[[the dog] [[the cat] [the man] kicked] bit] ⟨barked { }⟩⟩
=> derivational layer

=> 6 derivational layers

B. Type B: 6 layers

(6) b. [[[The dog] **that** [[**the cat**] **bit**] *that* [*the man*] *kicked*] barked]

Derivation:

(B1) a. N = {the, dog, that, the, cat, bit, that, the, man, kicked, barked}
b. * ⟨the {dog, that, the, cat, bit, that, the, man, kicked, barked}⟩

(B2) a. N = {the, dog}
b. ⟨the {dog}⟩
c. ⟨the ⟨dog { }⟩⟩ => derivational layer

(B3) a. N = {[the dog], that, the, cat, bit, that, the, man, kicked, barked}
b. * ⟨[the dog] {that, the, cat, bit, that, the, man, kicked, barked}⟩

(B4) a. N = {[the dog], that, the, cat, bit, that, the, man, kicked}
b. ⟨[the dog] {that, the, cat, bit, that, the, man, kicked}⟩
c. ⟨[the dog] ⟨that {the, cat, bit, that, the, man, kicked}⟩⟩
d. * ⟨[the dog] ⟨that ⟨the {cat, bit, that, the, man, kicked}⟩⟩⟩

(B5) a. N = {the, cat}
b. ⟨the {cat}⟩
c. ⟨the ⟨cat { }⟩⟩ => derivational layer

(B6) a. N = {[the dog], that, [the cat], bit, that, the, man, kicked}
b. ⟨[the dog] {that, [the cat], bit, that, the, man, kicked}⟩
c. ⟨[the dog] ⟨that {[the cat], bit, that, the, man, kicked}⟩⟩
d. * ⟨[the dog] ⟨that ⟨[the cat] {bit, that, the, man, kicked}⟩⟩⟩

(B7) a. N = {[the cat], bit}
b. ⟨[the cat] {bit}⟩
c. ⟨[the cat] ⟨bit { }⟩⟩ => derivational layer

(B8) a. N = {[the dog], that, [[the cat] bit], that, the, man, kicked}
b. ⟨[the dog] {that, [[the cat] bit], that, the, man, kicked}⟩
c. ⟨[the dog] ⟨that {[[the cat] bit], that, the, man, kicked}⟩⟩
d. ⟨[the dog] ⟨that ⟨[[the cat] bit] {that, the, man, kicked}⟩⟩⟩
e. ⟨[the dog] ⟨that ⟨[[the cat] bit] ⟨that {the, man, kicked}⟩⟩⟩⟩
f. * ⟨[the dog] ⟨that ⟨[[the cat] bit] ⟨that ⟨the {man, kicked}⟩⟩⟩⟩⟩

(B9) a. N = {the, man}
b. ⟨the {man}⟩
c. ⟨the ⟨man { }⟩⟩ => derivational layer

(B10) a. N = {[the dog], that, [[the cat] bit], that, [the man], kicked}
b. ⟨[the dog] {that, [[the cat] bit], that, [the man], kicked}⟩
c. ⟨[the dog] ⟨that {[[the cat | bit], that, [the man], kicked}⟩⟩
d. ⟨[the dog] ⟨that ⟨[[the cat] bit] {that, [the man], kicked}⟩⟩⟩
e. ⟨[the dog] ⟨that ⟨[[the cat] bit] ⟨that {[the man], kicked}⟩⟩⟩⟩
f. ⟨[the dog] ⟨that ⟨[[the cat] bit] ⟨that ⟨[the man] {kicked}⟩⟩⟩⟩⟩
g. ⟨[the dog] ⟨that ⟨[[the cat] bit] ⟨that ⟨[the man] ⟨kicked { }⟩⟩⟩⟩⟩⟩
=> derivational layer

(B11) a. N = {[[the dog] that [[the cat] bit] that [the man] kicked], barked}
b. ⟨[[the dog] that [[the cat] bit] that [the man] kicked] {barked}⟩
c. ⟨[[the dog] that [[the cat] bit] that [the man] kicked] ⟨barked { }⟩⟩
=> derivational layer

=> 6 derivational layers

C. Type C: 4 layers

(6) c. [[[The dog] **that bit** [**the man**] *that kicked the cat*] barked]

Derivation:

(C1) a. N = {the, dog, that, bit, the, man, that, kicked, the, cat, barked}
b. * ⟨the {dog, that, bit, the, man, that, kicked, the, cat, barked }⟩

(C2) a. N = {the, dog}
b. ⟨the {dog}⟩
c. ⟨the ⟨dog { }⟩⟩ => derivational layer

(C3) a. N = {[the dog], that, bit, the, man, that, kicked, the, cat, barked}
b. * ⟨[the dog] {that, bit, the, man, that, kicked, the, cat, barked}⟩

(C4) a. N = {[the dog], that, bit, the, man, that, kicked, the, cat}
b. ⟨[the dog] {that, bit, the, man, that, kicked, the, cat}⟩
c. ⟨[the dog] ⟨that {bit, the, man, that, kicked, the, cat}⟩⟩
d. ⟨[the dog] ⟨that ⟨bit {the, man, that, kicked, the, cat}⟩⟩⟩
e. * ⟨[the dog] ⟨that ⟨bit ⟨the {man, that, kicked, the, cat}⟩⟩⟩⟩

(C5) a. N = {the, man}
b. ⟨the {man}⟩
c. ⟨the ⟨man { }⟩⟩ => derivational layer

(C6) a. N = {[the dog], that, bit, [the man], that, kicked, the, cat}
b. ⟨[the dog] {that, bit, [the man], that, kicked, the, cat}⟩

c. ⟨[the dog] ⟨that {bit, [the man] that, kicked, the, cat}⟩⟩
d. ⟨[the dog] ⟨that ⟨bit {[the man], that, kicked, the, cat}⟩⟩⟩
e. ⟨[the dog] ⟨that ⟨bit ⟨[the man] {that, kicked, the, cat}⟩⟩⟩⟩
f. ⟨[the dog] ⟨that ⟨bit ⟨[the man] ⟨that {kicked, the, cat}⟩⟩⟩⟩⟩
g. ⟨[the dog] ⟨that ⟨bit ⟨[the man] ⟨that ⟨kicked {the, cat}⟩⟩⟩⟩⟩⟩
h. ⟨[the dog] ⟨that ⟨bit ⟨[the man] ⟨that ⟨kicked ⟨the {cat}⟩⟩⟩⟩⟩⟩⟩
i. ⟨[the dog] ⟨that ⟨bit ⟨[the man] ⟨that ⟨kicked ⟨the ⟨cat { }⟩⟩⟩⟩⟩⟩⟩⟩
=> derivational layer

(C7) a. N = {[[the dog] that bit [the man] that kicked the cat], barked}
b. ⟨[[the dog] that bit [the man] that kicked the cat] {barked}⟩
c. ⟨[[the dog] that bit [the man] that kicked the cat] ⟨barked { }⟩⟩
=> derivational layer

=> 4 derivational layers

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★ REVIEWS ★

Basquing in Minimalism

Piattelli-Palmarini, Massimo, Juan Uriagereka & Pello Salaburu (eds.). 2010. *Of Minds & Language: A Dialogue with Noam Chomsky in the Basque Country*. Oxford: Oxford University Press, 472pp.

by Alex Drummond & Norbert Hornstein

In 1975, in an old abbey in Royaumont, France, Massimo Piattelli-Palmarini gathered linguists, philosophers, psychologists and computationalists for the first biolinguistics get-together to discuss what the linguistics of the period told us about the structure of the mind/brain. The participants defended two contrasting viewpoints. One group, centered on Piaget's work, claimed that language mastery supervened on very general laws of cognitive development, language acquisition and use reflecting mental powers continuous with those witnessed in other areas of cognition. A second cohort, with Chomsky's work as cynosure, countered that the specificity of linguistic competence argued against any general psychological processes that encompassed both linguistic knowledge and other forms of cognition. It is fair to say that Chomsky's side got the better of the argument. The main problem for the Piagetians was explaining in non-metaphoric detail how their proposed general cognitive and developmental mechanisms could result in the particular kinds of phenomena linguists had discovered (e.g., how does the structure dependence of grammatical operations follow from Piaget's constructivism?). Chomsky's challenge was simple: Deduce the "laws" of grammar from the more general laws of psychology/development, or concede that the mind/brain contains very specific linguistically dedicated mental structures that guide the emergence of linguistic competence and performance.

The Royaumont volume (Piattelli-Palmarini 1980) serves as an interesting backdrop to the one under review here. Once again, **Massimo Piattelli-Palmarini** serves as impresario (this time in concert **with Juan Uriagereka and Pello Salaburu**) and convenes a group of distinguished linguists, psychologists, biologists, and neuroscientists in a very scenic spot (San Sebastian in Spain's Basque Country) to discuss what current linguistic theory suggests about the structure of the mind/brain. This time round, however, the relevant linguistic perspective is the one offered by the Minimalist Program (Chomsky 1995 *et seq.*). Consequently, much of the discussion aims to minimize the degree to which language mastery requires specifically linguistic mental powers and emphasizes the continuation between linguistic competence and other cognitive capacities.

It is tempting to conclude from this that, although the Piagetians lost the earlier battle, they decisively won the peace by converting their antagonists.

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Tempting but inaccurate. At Royaumont, Chomsky & Co. had no principled objections to Piaget's conclusions. Rather they argued that the Piagetian claim was too thin to be of interest for it failed to show *how* the postulated general cognitive principles could explain the specific properties of the faculty of language.[1] It is this rather large hole that the Minimalist Program aspires to plug, and unless it does so, minimalist aspirations will prove to be no hardier than the Piagetian ones that Chomsky & Co. so successfully routed at Royaumont. In what follows, we discuss both how to specify the gap that needs filling and how minimalism is going about filling it.

Like the earlier Royaumont volume, the papers here are uniformly thoughtful and provide an excellent guide to some of the best thinking on biolinguistic themes. The discussion sections at the end of each paper are particularly intriguing (and invariably amusing). They give the reader the feel of being part of the festivities. It is clear that the participants had a really good time and this makes for a very good read. We recommend that your reading be accompanied by snacks — preferably tapas and a good wine (a nice Rioja would suit) — to help recreate the atmosphere. In what follows we will touch on some of the themes discussed. However, a magisterial review of all the papers herein is beyond our abilities. As excuse, we borrow Chomsky's (p. 379):

> I've tried to think a little about how to organize some comments. An awful lot of fascinating material has been presented here, some of which I understood, some of which I didn't. What I'll try to do is pick out some points that come to mind […] expressing an apology in advance to everyone whose work I misrepresent.[2]

1. The Minimalist Program

The volume begins and ends with some useful history (**Noam Chomsky**'s Opening Remarks in chapter 2 and Conclusion in chapter 23), part of which rationally reconstructs the generative enterprise. The generative enterprise has its origins in a 17th century question: In what way is language a window on the mind? By giving the question some contour, we can generate a modern research program as follows: (i) Specify the properties of human natural languages, (ii) figure out what kind of mind/brain could assimilate and manipulate objects with these properties, and (iii) determine which mind/brain properties are proprietary to dealing with objects like these and which are features of a more generally competent cognitive device. (i)–(iii) correspond to the three principle epochs in generative research: the early years, which investigated the kinds of rules and constructions characteristic of natural languages (recursive, structure-dependent, with both local and non-local dependencies); the middle years, when these rule systems were simplified by factoring out their general properties (A/A'-move-

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[1] The same holds for claims reducing linguistic knowledge to a species of "general intelligence". It is not so much that these positions are wrong as they are of dubious utility if one's aim is to understand the attested structure of cognitive capacities. For some relevant discussion of these themes, see Rochel Gelman's discussion of core domains versus HoWs (hell-on-wheels) in chapter 15.

[2] Our addition: And those that we ignore or only briefly mention.

ment dependencies, local and non-local construal systems, domains of opacity for movement and construal like islands and binding domains, core notions like c-command, abstract licensing conditions such as case and ECP); and finally the current minimalist project, which tries to segregate the general properties (e.g., identified by Principles-and-Parameters theories like Government-and-Binding Theory) into those requiring specifically linguistic principles and operations and those reflecting general aspects of cognition/computation.

Chomsky's remarks sketch how the current concerns of the minimalist program "arose in a natural way from the successes of the P&P approach". They did so in two ways. First, the results of P&P theories allowed "the basic contours of an answer to the problem of explanatory adequacy" to come, at least faintly, into view and thus allowed research to "turn more seriously to the 'why' questions that transcend explanatory adequacy". Second, Chomsky remarks that P&P theories removed "the major conceptual barrier to the study of the evolution of language […]. [By] divorc[ing] the principles of language from acquisition, it no longer follows that the format that "limits admissible hypotheses" must be rich and highly structured to satisfy the empirical conditions of language acquisition, in which case inquiry into evolution would be virtually hopeless " (p. 25).

For those who found these last remarks as cryptic as we did, here is what Chomsky meant.[3] Pre-P&P syntactic theory consists of a "format for rule systems and an evaluation metric (EM) to rank them". This EM is "language internal", not a domain independent "general simplicity measure".[4] EM seeks to specify "what […] a legitimate generalization in language" is. This account permits a possible theory of language acquisition — "run through all possible grammars from the shortest on and stop when you have one conforming to PLD [primary linguistic data — NH & AD]. But that's unfeasible […]. So it's not really a theory". The P&P theory is feasible as it requires "answering a finite questionnaire". Pre-P&P theories, in contrast, require the language acquisition device to make global decisions about the structure and format of entire grammars, and this, though logically coherent, is computationally unfeasible as it involves powers beyond what it is reasonable to suppose that children (or adults) have. Thus, the intractability of EM and its linguistic specificity made evolutionary considerations quite hopeless. Put positively, P&P's distillation of a set of general and useable generalizations ("laws of grammar") makes it possible to address the question of the etiology of these generalizations.

A quick digression: The idea that P&P theories should be computationally tractable because acquisition involves "answering a finite questionnaire" (i.e. setting a finite number of parameters) has proven to be very difficult to demonstrate. **Janet Fodor** (chapter 17) reviews the issues and explains why the problem has proven so refractory. The main difficulty is that the parameter values are not independent, their correct values often depending on the values of other parameters. This makes it very hard to set them correctly one at a time and this makes

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[3] Thanks to Noam Chomsky (p.c.) for clarifying discussion. Quotes are from correspondence with Chomsky. See also Fodor's paper in chapter 17, where she outlines and evaluates the *Aspects* theory (Chomsky 1965).

[4] It is unlikely that non-trivial versions of this kind of simplicity measure exist. At least no successful one has been proposed to date.

incremental learning very difficult as parameters once set need not stay set. In effect, having a finite set of questions to answer does not simplify the acquisition problem if the answers require a global evaluation of the data. Fodor suggests that an adequate answer will require reconsideration of evaluation metrics with all of the (apparent[5]) language particularities of the one in *Aspects* (Chomsky 1965). The model she describes, however, has the virtue of being feasible in the sense that "neither online computation, nor memory is overtaxed" (p. 269).[6]

Back to the main thread: Chomsky's historical reconstruction highlights two important points: First, the generative enterprise has been cumulative with results from prior periods of research conditioning later investigations. Second, with each epoch, the generative enterprise added a new question to the research agenda. This, of course, is exactly what one would expect from a successful program; break general questions down into a series of manageable bite size very specific research accessible topics, feed ensuing results into another set of related more ambitious research questions, then reiterate.

Though this is what one should expect, it is useful to rehearse this simple point at this time. Why? Because outsiders (and even some insiders) apparently look at the generative enterprise and see a blooming buzzing confusion motivated by seemingly irrational changes in fashion, MP simply being the latest rage. The logic of the enterprise has clearly passed them by. A good remedy is a concise Whig history, and Chomsky provides one. Second, because understanding how linguistic theory has developed affects how we should now understand the latest minimalist phase. For example, it modulates a widespread (if often inchoate) view that takes MP and GB to be competitors.[7] Given the discussion above, they cannot be, at least in one sense (but see below), for the results specified by P&P (GB being P&P's most auspicious incarnation) serve as the data to be analyzed by MP. As noted above, minimalist inquiry starts from the assumption that the "laws of grammar" as specified by GB are roughly empirically correct. They correctly describe key features of linguistic competence, at least to a good first approximation. MP embraces this conclusion and adds the following item to the research agenda: Why *these* laws? Why do we find the locality principles we find (e.g., A-over-A, islands, binding domains, minimality, cyclicity)? Why are dependencies coded in chains with hierarchical requirements (e.g., the head is a case position, the tail a theta position, and the links locally c-commanding one another)? Why are operations structure-dependent? Why are phrase markers hierarchically organized endocentrically? These and many other 'facts' are the explanada. MP's aim is to furnish an explanans. If so, MP will not

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[5] But see her discussion on pp. 269–270.

[6] Dresher & Kaye (1987) is the first work to observe this problem with parameter-setting models. They propose a solution that requires a linguistically specific learning algorithm (one with specified paths through the parameter space; cf. Dresher 1999), in contrast to the general learning algorithms generally deployed. Their discussion involves stress learning and so it is not clear that their solution generalizes to the syntactic case. At any rate, it appears that there is now a general consensus that the P&P picture, though alluring, does not easily translate into a workable model of acquisition.

[7] Following the approach of Hornstein *et al.* (2005), we will take GB to be the best worked out P&P theory, and we will tendentiously use it as proxy for the accepted wisdom. However, for what follows, the results of other frameworks would do just as well.

(and cannot) in general challenge these GB 'facts' (though they may modify them at the margins — recall that the assumption is that these are *roughly* empirically correct) but must assume them to be accurate. As corollary, MP cannot render obsolete GB-style research for there will always be value in discovering additional laws of grammar, new kinds of operations and generalizations. Rather, MP adds new questions to the research agenda; what principles (if any) do these generalizations follow from (e.g. economy, inclusiveness) and are these principles specific to linguistic computations (e.g. Merge) or are they reflections of more general principles of cognitive computation (e.g. No Tampering, Minimal Search)? In this sense, then, MP does not and cannot replace GB, any more than GB replaced the Standard Theory's project of specifying the formal properties of linguistic constructions. In both cases, the results of the prior investigations were to a large extent conserved and served to set the scene for the next set of questions. Indeed, how could it have been otherwise! It is hard to factor out the common features of grammar rules without specifying any grammar rules and it is hard to ask what is specifically linguistic in a law of grammar without having any laws to consider.[8]

This said, there *is* a sense in which MP aims to replace GB. The MP problematic has two parts. The first is that the generalizations that generative grammar has discovered over the last 50 years are roughly empirically accurate. The second is that the generalizations unearthed are *not* ontologically fundamental. The first assumption sets a target for the second: how to deduce the specific properties of the faculty of language described by the laws of GB from more fundamental principles of computation and cognition. If so, there is a sense in which MP and GB *do* conflict. What GB takes as constitutive of UG, MP takes as accurate description but ultimately derivative. Physicists have good terms for this. They distinguish 'effective' theories (which are empirically roughly right) from 'fundamental' theories, which limn the metaphysical basics. Given this distinction, we can say that though GB is an 'effective' theory, it is not fundamental.

Chomsky's remarks in both the introduction and the conclusion make a second important point: Not only is the time 'ripe' for minimalist inquiries, given the discovery of plausible laws of grammar whose properties we can investigate, but logic dictates that the linguistic specificity and complexity of the GB conception of UG *must* be illusory. Here's the argument: Assume that complexity is the product of natural selection (NS), and assume that long time spans are required for natural selection to work its magic (say, on the order of many hundreds of thousands or millions of years). If so, UG's properties cannot be the work of NS for there is reason to think that the human faculty of language is about 50–100,000 years old at most. Conclusion: The basic architecture of the Faculty of Language (FL) is not the result of an arduous and long process of selection but reflects an at most small addition to an already existing system of computations. This argues against the internal modularity of FL and against

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[8] A corollary: It cannot be a criticism of a proposed analysis that it is "not minimalist" because it invokes GB-style technology. In fact, if MP succeeds in reducing GB to a more basic theory (see below), then any decent GB analysis of a given phenomenon will have a smooth translation into a more fundamental minimalist account.

wildly idiosyncratic operations or principles of organization.[9] The minimalist project aims to factor out those operations and principles that are part of cognitive computation more generally and see what small bit has to be added to get what we in fact see. In effect, on this view, FL/UG is an interaction effect, the output of the interaction of one (or two) linguistically bespoke operation(s) in interaction with more general, cognitively extant principles of computation.

This animating general picture suggests at least two specific kinds of research projects. The first is reductive. If complexity bespeaks long periods of evolutionary time, then short time scales suggest simple organization. If so, the complex modular structure that P&P accounts like GB postulate for FL/UG must be illusory. In other words, an FL/UG with seven different modules, with many different grammatical operations, defined over different primitives, and invoking different domains of application must be illusory. The illusion is made manifest by reducing the different modules to a common set of operations and principles.

The earliest versions of MP (e.g. Chomsky 1993) initiated this line of inquiry by reducing case checking to movement theory, case checking being a special instance of A-movement (subject to minimality restrictions) to a dedicated case position. More (indeed, far more) controversial instances of the same logic have aimed to reduce control and anaphora to movement as well.[10] Assuming Chomsky's recent proposal that phrase structure-building and movement are special instances of the same basic operation — Merge (External and Internal Merge, respectively) — there is no fundamental distinction between movement and phrase structure construction either.

The logic mimics Chomsky's reduction of Ross's (1967) islands to a common set of operations (Move α), subject to a common set of conditions (Subjacency). If successful, this envisioned reduction significantly reduces the internal modularity of FL/UG. Case, control, binding, agreement, and movement are at bottom all the same kind of thing (*viz*. instances of Internal Merge), subject to the same restrictions and conditions in much the same way that topicalization, relativization, question formation, and focus movement are all instances of A′-movement, subject to Subjacency in Chomsky (1977). The best possible outcome is the elimination of all FL/UG-internal modules, and this possibility is clearly in sight theoretically.

The second project aims to build up the basic operations from computationally simple, well-behaved natural operations. Merge, which Chomsky has proposed is responsible both for phrase formation *and* displacement (aka movement), is the minimalist operation of choice. If we assume that Merge is compu-

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[9] This point has been put well in Fodor (1998: 12):

> If the mind is mostly a collection of innate modules, then […] it must have evolved gradually, under selection pressure. That's because […] modules contain lots of specialised information about the problem-domains that they compute in.

Thus, a highly modular FL goes in tandem with an extended period within which NS operates. Conversely, a short period for NS to operate argues against a highly structured and modular FL.

[10] See, among others, Lidz & Idsardi (1998), Hornstein (2001), Kayne (2002), Zwart (2002), Grohmann (2003), and Boeckx *et al*. (2010).

tationally well behaved (methodologically, the best assumption) we expect it to respect certain plausible conditions such as No Tampering and Inclusiveness. If we are clever (and lucky), we will discover that Merge, subject to its nice computational restrictions, will suffice to describe all the different kinds of dependencies that are treated as separate in GB; in other words, we will discover that, once an operation like Merge arises, which is able to combine atomic elements together in a computationally simple and natural way, language-like systems will emerge, able to represent (all and only) dependencies of the kind described by GB-like versions of FL/UG. In other words, once phrase structure emerges, displacement subject to the restrictions we find also arises. We are not at that point yet, but as Chomsky notes we are at least able to contemplate what such accounts might look like, and that is very exciting.

2. Some Disputations

This is the bare bones MP problematic. For the past two decades, Chomsky has offered a variety of different proposals to add meat to these bones. Some of these have proven to be controversial even among those moved by the animating picture. And this is how it should be! There are many ways to pursue the goals of the Minimalist *Program*. Inquiry will advance through the exploration of different specific minimalist *theories* — and over time, it is hoped, normal scientific practice will produce a fuller and fuller outline of the truth. Some provocative additions to the ongoing debate can be found in this volume. Let's consider a couple.

There is decent evidence that phrases in natural language are endocentric. Minimalists have coded this fact by supposing that Merge is a complex operation that involves two sub-operations, one that unites two elements merged and one that labels the subsequently created object in terms of one of the two mergees. The element that provides the label is the head. This results in a phrase that is both complex and categorized as having the same type as its head. The properties of endocentricity, it is hoped, reflect (indeed, follow from) this labeling process. The natural question to ask is why natural languages have labeling? This volume provides two different answers.

Chomsky denies that labels are fundamental objects or that labeling is a basic grammatical operation. Rather, he suggests that labels/labeling reflect minimal search, a design feature of a well-behaved computational system. The idea is that a computational system that embodies minimal search will isolate one element of a combined pair (i.e. roughly a binary-branching phrase) as more accessible than the other. This element is the head. Or, as Chomsky says (p. 393):

> It's close to true that Merge is always a head and another object (a head is just a lexical item, one of the atoms, so Merge is a lexical item and some other object). To the extent that this is true — and it is overwhelmingly true — you eliminate the last residue of phrase structure grammar (projections or labels) because the head is just the thing that you find by minimal search.

On this view of things, (i) endocentricity is a product of minimal search and (ii) mini-mal search suffices to capture "everything about headedness". Given (i) and (ii), we can reasonably conclude that (iii) endocentricity is a defining property of well-designed computational systems and (iv) we should expect to find that most natural (and artificial?) computational systems exploit endocentric structures.

Cedric Boeckx presents a different view in chapter 3.[11] He proposes that the process of labeling a complex via one of its constituents is an operation unique to language and one of its basic defining properties.[12] As he says (pp. 47–48):

> [T]his [labeling — AD & NH], as far as I can tell, is very, very specific to language as a kind of hierarchical structure. If you look elsewhere in other systems of cognition […] you find a lot of evidence for hierarchical structuring of systems, possibly recursive ones, but as far as I can tell, these hierarchical structures are not headed or endocentric […]. That […] is very specific to language, so while you find hierarchies everywhere, headed or endocentric hierarchies seem very central to language.

On this view, whereas a "grouping procedure that puts X and Y together" is likely to be very common across cognitive modules, the operation of "selecting one of these two members and basically using that member as the next unit for recombination" (p. 48) is unique to language. Thus, we do not expect it to be part of computational systems in general but to arise predominantly (perhaps, exclusively) in linguistic structures.

We are partial to the second view of labels for several reasons: First, Boeckx's observation that we do not find endocentric structures in other areas of cognition argues against analyzing it as a product of minimal search for were this its source such structures should be ubiquitous features of any moderately well designed computational system, apparently contrary to fact.[13] Second, it is not clear to us how minimal search suffices to derive the properties associated with endocentricity. Take, for example, head-to-head selection or subcategorization, e.g. the relation between *interest* and *in* in phrases like *interest in Bill*. This relation is severely restricted. Nouns might select/subcategorize for the prepositional head in a PP complement, e.g. *interest* requires *in*, but there is never selection for the complement of the prepositional head. Thus, there are no nouns like *interest* that require that the nominal complement be *fish* (or a DP headed by *the*) but allow for any preposition whatsoever, e.g. **interest (over/under/to/with) fish*. Why not? Because only the head within a phrase can be seen by an outside head. And this is not because the preposition is less complex than its complement for make the complement of *in* as simple as possible, e.g. *it,* and the complement will still

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[11] The reader should be warned that one author is a partisan here, as this view is also proposed and defended in Hornstein (2009).

[12] Hornstein (2009) proposes that it is the adventitious addition which, when added to the rest of cognition, results in a grammatical distillate.

[13] It is not easy to find examples of artificial computational systems that code endocentricity. Basic arithmetic seems to do well without it, for example. Programming languages also tend to avoid endocentric means of combination (though here there is more room for interpretation). In both cases, the most common (and in arithmetic, only) form of combination is homogenous: Multiple expressions of the same kind combine to form another expression of that kind. For example, the integer expressions 2, –2, and 3 combine to form the expression [2 * [–2 + 3]], or a sequence of statements combines to form a single complex statement. In the case of programming languages, one also finds clear examples of non-homogenous but nonetheless non-endocentric combination. For example, a list of variable and method declarations combines to form a class (which itself is neither a variable nor method declaration).

never be visible to an outside selecting head. This suggests that minimal search is besides the point for, hierarchically speaking, the preposition and its simplex complement are equidistant from any higher element.[14] As such, searching for either should be equally 'minimal'. Third, labels make computational sense when one considers the fine structure of Merge. Labeling closes the 'combine' operation in the domain of the lexical atoms. As Chomsky (2008) has noted, mergeable items must have edge features. All lexical atoms have these inherently. Furthermore, when two atoms combine to form a unit, that unit must also have an edge feature for it is further combinable. Whence *this* edge feature? It cannot be a lexical property as complex units are not primitive atoms. Thus, it must inherit this property, presumably from the elements combined. In other words, even if we define Merge as the operation that combines lexical atoms, we need to explain how this combinatorial power extends to lexical complexes. Given standard assumptions, it is natural to think that labels endow complexes with the powers of atoms: A lexical atom inherently has an edge feature and a phrase inherits an edge feature insofar as it shares the properties of its head.[15] In other words, edge feature inheritance supervenes on labels as labeling is what allows complex units to combine in just the way that simplex lexical atoms do. In effect, labels create an equivalence class of expressions centered on primitive lexical atoms. This is the secret to the kind of hierarchical combination natural languages exploit.

There is, no doubt, more to say on either side of this 'debate'. However, what is important here is the recognition that neither proposal is *a priori* more or less 'minimalist'; both fit the spirit of the program. Which is correct is (ultimately) an empirical matter, not an ideological one. The Minimalist Program is compatible with various minimalist theories, and which versions should prevail is an empirical (albeit, no doubt, complex) matter.

Luigi Rizzi's discussion of two notions of locality (chapter 11) offers a second illustration of a nice, ripe minimalist research question. Movement is currently subject to what appears to be two different locality restrictions, Relativized Minimality (RM) and Phase Impenetrability (PI). The former forbids dependencies between two elements X and Y over an intervening Z of the same type, with type being defined featurally. The latter prevents dependencies that span the complement domains of specific elements, *v* and C being (at least) two. Both notions are part of the current minimalist technical apparatus and both have empirical virtues. Rizzi asks the obvious minimalist question: Do we really need both?[16] More specifically, given that locality is a way of bounding domains of computation and given that bounded computations are invariably efficient and

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[14] Note that endocentric labels will help define minimal dependencies and heads will be more prominent than anything else in a phrase. However, this reverses the explanation: Minimal search supervenes on headedness, not the other way around.

[15] This follows from what a label is. For example, it is the standard assumption that the label *is* the head (cf. Chomsky 1995). Moreover, in virtue of this, X'-theory assumes that "a head and its projections share *all* [our emphasis — AD & NH] properties apart from bar-level […]" (Chomsky 1986: 18). Thus, if a head has an edge feature, the phrase that it labels does too. This suffices to project edge features in the required way: A phrase inherits an edge feature from one of its daughters and the daughter it inherits it from is the head.

[16] Our impression from the dearth of discussion of minimality in Chomsky's latest papers is that he too is attracted to the idea that there is only one operative locality condition, *viz*. PI.

well behaved, we expect on conceptual grounds that linguistic computations should be bounded and local. However, bounding the computations twice over via two different locality conditions does not obviously make computations *more* well-behaved or *more* efficient. So, why two?

The question becomes more insistent when one notes that current versions of RM and PI are somewhat redundant. So, for example, each only kicks in when two or more expressions of the same type are lurking and neither applies to put an absolute bound on a grammatical domain. Here's what we mean. Consider a case of multiple movement as in (1):

(1) John$_1$ seems to be believed to be likely to be kissed t_1.

There is a long-distance relation (potentially unbounded) between *John* and its base position indicated by t_1. RM places no bound on this dependency as there is no relevant intervening element, i.e. no other DP in this domain. PI, at least Chomsky's version, treats (1) as a single phase.[17] So, both RM and PI countenance arbitrarily long distance dependencies in similar circumstances. Moreover, both block them in similar cases. So for example, in (2) both RM and PI block movement of the object to the higher subject position:

(2) *John$_1$ seems that it was told t_1 that Bill left.

For RM, *it* intervenes between *John* and its base position and with PI the complement of *that* is a phase and hence its complement is impermeable. This suggests that in different ways, the two conditions apply to the same configurations and hence are partly redundant. This only strengthens the above-mooted why-question: why *these two* locality restrictions?[18]

One of the more interesting arguments for PI comes from successive cyclic movement. Indeed, the derivation of successive cyclicity from Subjacency was one of its more endearing properties. There is evidence that in examples like (3) we have moved C-to-C:

(3) How$_1$ did you think [$_{CP}$ t'_1 that we will solve Plato's Problem t_1]?

Note that if C is a strong phase, then PI requires movement of *how* via the phase edge. This results in successive cyclic movement. It is less clear how to accommodate these data given RM. Rizzi proposes a solution. In effect, he postulates features in finite C (Q-features) that via RM will prevent *wh*-operators like *how* from traversing them. He further provides empirical evidence from Chamorro for the selective nature of this restriction, some *wh*'s able to cross the finite C — others not.

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[17] This is not quite right, there is no *strong* phase between *John* and *t*. There are several weak phases, but as these fail to block grammatical intercourse, they do not bound computations.

[18] That RM and PI should roughly overlap is suggested by the fact that, on both, transitive clauses are expected to be local domains for A-movement. This is true for RM because at least two DPs live in a transitive clause and hence one should block movement of the other. In PI this is because transitive *v* is strong. We will see momentarily that there are ways of endowing finite C, which is a strong phase head within PI, with features that block non-local dependencies across them in an RM-approach.

The discussion is subtle and interesting, albeit more suggestive than conclusive.[19] The paper is too cursory to settle the question of whether all unbounded dependencies can be regulated using just RM.[20] For example, the discussion is limited to weak island effects and the classical cases of locality are those involving strong islands.[21] However, the question *is* a very good one and the kinds of considerations that Rizzi brings to bear in the discussion well illustrate the empirical utility of raising such minimalist concerns.

Tom Bever (chapter 18) offers a third example in his discussion of the EPP, the requirement that sentences have subjects. This principle has resisted attempts at rationalization or deeper explanation, and it stands to date as a theoretical diacritic enforcing the requirement that sentences have subjects. Bever's paper proposes to remove the EPP from FL/UG and instead locate it in learning theory. It is an expression of the Canonical Form Constraint (CFC), which requires that "sentences […] must sound like they are sentences of the language to afford the individual child a statistical entrée into acquiring it" (p. 279). If correct, this serves the minimalist ambition of tidying up FL/UG by repositioning a requirement that is an *ad hoc* idiosyncrasy when understood grammatically to a principled effect when understood as part of a more general theory of learning.

The ambition and form of the argument Bever presents is impeccable. Less clear is whether it covers all the relevant cases. The chapter concentrates on cases of the CFC and the EPP in main clauses. The discussion ranges over expletive constructions and passives and one can see how the CFC could be deployed to account for the presence of expletive subjects and movement of objects under passivization. As Bever notes, "the vast majority of sentences and clauses have a canonical form with a subject preceding a correspondingly inflected verb" (p. 289). Expletives serve to gather sentences that have no apparent logical subjects under the same statistical rubric, as would moving objects to subject position in cases like passive, where subjects are left unexpressed, or unaccusatives, where there are no subject-like arguments to be had. This all fits nicely together, especially in finite matrix domains. Note, however, that in *these* domains, there are also plausible and natural grammatical candidates to explain the EPP, for example, the Inverse Case Filter; the requirement that there be lexical expressions able to support the (nominative) case that finite T^0 assigns to their specifiers. Here, no "conspiracy" (p. 290) of interacting constraints is required, only the natural assumption that morphological case needs to be assigned, and thus something needs to be there to receive it.[22] The real testing ground for the EPP is non-finite

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[19] In addition, there exists some evidence that arguments like *who/what* trigger inversion in languages like French and Spanish but adjuncts like *why/how* do not. This might at first blush be taken to indicate that only *who/what* undergo successive-cyclic movement. This raises non-trivial variation issues if correct, since in the work Rizzi cites, Chung (1994: 29) has argued that both adjuncts and arguments trigger *wh*-agreement in Chamorro.

[20] This is *not* a criticism of the paper, given the venue in which it was presented. We are sure that Rizzi would agree that the argument he provides was intended to be illustrative (and in this it succeeds admirably) rather than definitive.

[21] It is not clear that the approach of adding features to heads can be applied to yield strong island effects. Then again, extending PI to accommodate strong islands seems to require adding D to the inventory of phases, and this sits poorly with Chomsky's interest in tying the inventory of phases to propositional heads.

[22] Note that we are not saying that this is the correct explanation. Rather, we are observing

embedded clauses; do these also require subjects? A case-based account would answer in the negative, as would, we surmise, an account based on the CFC.

At present, there is not overwhelming evidence that non-finite clauses require subjects.[23] However, this is where the consequences of the EPP are the most interesting and least obviously tied to what one finds in matrix clauses. This said, Bever's proposal is interesting for it provides a picture of how one might try to give a non-grammatical account of what would appear to be a paradigmatic example of a grammatical restriction. This is the sort of thing that minimalists should welcome for simplifying UG it makes it easier to explain its remaining properties.

All three discussions illustrate another important point. The take-home message from San Sebastian is that it takes a lot of work to show how linguistic competence relates to cognition in general. Serious illumination requires identifying specific linguistic features (e.g., endocentricity, selective islands, EPP), isolating some plausibly general cognitive operations or computational restrictions and showing how the former follow from the latter. The examples above attempt this. If they succeed then we have managed to isolate the cognitively/computationally general from the linguistically specific. Unless we do this, what we have is the sound of one hand waving.

3. Words and Concepts

Chomsky's introduction touches on a second very important feature of linguistic systems. In addition to the unique kind of recursion we find in language, human linguistic systems contain atoms, lexical items (words) that exhibit a distinctive (perhaps unique) set of properties. They are remarkably flexible things, at least when compared with what we find in other biological communication systems. As Chomsky (p. 27) notes:

> The basic problem is that even the simplest words and concepts of human language and thought lack the relation to mind-independent entities that has been reported for animal communication: representational systems based on a one-one relation between mind/brain processes and "an aspect of the environment to which these processes adapt the animal's behavior," to quote Randy Gallistel [Gallistel 1990]. The symbols of human language are sharply different. […] Communication relies on shared cognoscitive

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that this is a natural grammar-internal explanation, that it is not theoretically idiosyncratic, and that it serves to remove the diacritical nature of the EPP. This kind of proposal exists in the literature, cf. Castillo *et al*. (1999) and Epstein & Seely (2006).

[23] In our opinion the best argument concerns cases like (i):

(i) John$_i$ appears to Mary$_k$ to seem to *herself$_k$/himself$_i$ to be intelligent.

The unacceptability of *herself* and the acceptability of *himself* follows if there is a trace of *John* in the subject of the first embedded clause, which in turn follows if the EPP holds in all clauses, including non-finite ones. The main problem with such examples concerns the relative poor status of (ii). It should be perfect given the unacceptability of (i) with *Mary* anteceding *herself*, given that pronouns are licensed by the binding theory precisely where reflexives are barred. To our ears, (ii) is not particularly good. This renders the datum in (i) hard to interpret.

(ii) John$_i$ appears to Mary$_k$ to seem to her$_{??k}$ to be intelligent.

> powers, and succeeds insofar as shared mental constructs, background, concerns, presuppositions, etc. allow for common perspectives to be (more or less) attained. These semantic properties of lexical items seem to be unique to human language and thought, and have to be accounted for somehow in the study of their evolution.

It is not at all clear what the cognitive basis of these distinctive features of words is. It is not even clear whether this is a specifically linguistic property of words or a more general cognitive feature of human concepts. It appears, however, that this is a unique characteristic of human words/concepts and is key to any future understanding of the creative aspects of language use. To date, we lack a systematic description of the basic semantic properties of words/concepts or a systematic comparison between human words/concepts and those found in other animals.

An interesting line of current inquiry prompted by recent minimalist concerns compares animal and human cognition to isolate those features that are specifically 'human'.[24] **Marc Hauser**'s contributions (chapters 5 and 19) directly advance this project, as does **Randy Gallistel**'s, albeit more indirectly. Animal cognition is very elaborate. Gallistel's paper (chapter 4) describes the elaborate "foundational abstractions of time, space, number and intentionality [that] inform the behavior of birds and bees (p. 61)". It appears that birds can reason about time: They "compute elapsed intervals and compare them to other intervals in memory (p. 63)". They can reason about number: They are able to "first subtract the current number from a target number in memory and then compare the result to another target number in memory (p. 63)". And they can evaluate "the likely intentions of others and reason from their own actions to the likely future actions of others (p. 64)". All of this requires rather formidable computational powers as well as primitive foundational (innate) abstractions concerning time, number and other minds of a robust sort. Interestingly, these elaborate capacities appear to exist independently of a facility for language and so we can conclude that linguistic capacity is not necessary for a rich mental life (see the discussion of Gelman's chapter below).

In chapter 5, Hauser pursues this comparative methodology to consider concepts that are characteristic features of linguistic systems. He reports work indicating that rhesus monkeys distinguish singular from plural concepts as well as mass from count. If this is correct, then despite the grammatical exploitation of these notions, their etiology is independent of FL. This does not imply that the concepts operative grammatically are in every way identical to those that Hauser's monkeys have. He points out that infants do less well than rhesus monkeys when forced to enumerate objects versus masses (p. 81). Thus, even if the count/mass distinction does not require linguistic underpinnings, it may well be that absorption into FL endows the distinction with distinctive properties. Nonetheless, these results support an important minimalist intuition; that language is a very complex system and that linguistic competence is an interaction effect with various non-linguistic parts of our cognitive apparatus making

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[24] See the distinction between the broad and narrow faculty of language in Hauser et al. (2002), where the minimalist sources of this kind of research is clearly outlined.

important contributions.

If concepts do indeed predate language, we might ask what else was present in the pre-linguistic conceptual-intentional (C-I) systems. This question is raised by **Wolfram Hinzen** in a provocative chapter 9, which argues for the abolition of the C-I interface. Chomsky believes that the structures derived in the syntactic component are fed into a set of highly intricate interpretative processes; let us term this the 'interpretavist' position. These interpretative processes are standardly taken to fix certain aspects of, for example, scopal and anaphoric interpretation. As such, they must have logical as well as conceptual resources at their disposal. In particular, they must be able to 'read' the recursive hierarchical structures offered up by the syntax. This, Hinzen argues, is a fatal flaw in Chomsky's conception of the relation between the syntax and the C-I interface. Chomsky has famously claimed that recursion is a, or perhaps even *the*, property specific to language.[25] But, of course, this is not a coherent position if his conception of the C-I interface presupposes recursion in other cognitive faculties.

There are various technical moves that could be made at this point in defense of the interpretavist position. For example, it may be that Hinzen is inferring too much from the fact that the C-I interfaces are able to read the recursive structures generated by the syntax. This does not necessarily imply that "recursive thought" was possible prior to the development of language. Rather, it may be that the capacity to interpret recursive structures lay dormant until the corresponding ability to generate them developed. In any case, we will not quibble further on these points, since we think that Hinzen has hit on a genuine puzzle. Our main concern is with Hinzen's sketch of a solution to this puzzle, which we find problematic.

Hinzen's approach is in the tradition of 'abstract syntax'. On the interpretavist model, as developed within the Minimalist Program, syntactic processes are for the most part semantically blind. Some syntactic units may receive no interpretation at all (e.g. dummy prepositions such as *of*), and the same syntactic configuration may admit of multiple interpretations. In contrast, Hinzen argues that syntactic structures are inherently meaningful. They do not *receive* interpretations; they *bear* meanings.

We see two primary difficulties with Hinzen's approach. The first problem is one that his approach has in common with its historical antecedents. There is a multitude of gnarly technical difficulties involved in attempting to capture semantic generalizations in syntactic terms. Consider, for example, judgments of entailment and synonymy. Some of these might plausibly be read off syntactic structure directly (e.g., that if John met Bill on Tuesday, he met Bill). Others can be farmed out the conceptual component (e.g., that if John ate breakfast, he ate some food). There remain, however, awkward intermediate cases. For example, though competent English speakers know that (4a) is near-synonymous with (4b) the contrast between (5a) and (5b) suggests (ingenious arguments to the contrary notwithstanding[26]) that this relationship cannot be captured in syntactic terms:

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[25] Or to be more precise, a/the property specific to the faculty of language in the narrow sense (Hauser *et al*. 2002).

[26] See Jackendoff (1969: chap. 5) and references cited therein for relevant discussion.

(4) a. John rarely makes mistakes.
b. It is rare for John to make mistakes.

(5) a. John deliberately makes mistakes.
b. * It is deliberate for John to make mistakes.

Attempts to give syntactic analyses for phenomena of this sort have inevitably led to the postulation of highly complex and abstract syntactic structures. Complex and idiosyncratic rules are then required to ensure that, for example, (4b) can surface as (4a), but that (5b) cannot surface as (5a).

This brings us to the second difficulty, which unlike the first is proprietary to minimalism. Suppose that the abstract syntax project is one day executed successfully. (The technical difficulties may, after all, be overcome.) Could the resulting theory be in any sense minimalist? A virtue of the interpretativist approach is that it permits us to separate out a small domain — a relatively modest syntactic component — in which a reductionist approach is feasible. On almost anybody's account, the interpretative systems are far less tractable. A cautiously optimistic person might hope to see the basic properties of A-movement derived from "minimal search" in his or her lifetime. But it seems fantastic to suppose that the whole semantic circus will submit to this sort of treatment. In our view, then, minimalism is quite fatal to any attempt to break down the barrier between syntax and the C-I system — or to put it in less guarded terms, between language and thought. Whatever may be the correct division of labor between mental faculties, we suspect that the bit that does the merging and moving will turn out to be a lot smaller than the bit that does the thinking.

Chomsky's remarks (especially in the conclusion) touch on his own views regarding the relation of language, meaning and thought in general. He briefly mentions classical conundra, such as the ship of Theseus, and Kripke's (1979) puzzle regarding the meaning of belief-ascribing sentences. What he does not belabor here, but has explicitly noted elsewhere, is that more often than not, natural language runs roughshod over classical semantic distinctions; temperatures can be 90 and rising, books that weigh five pounds can be engrossing, the average man can have 5.4 children, hexagonal European states can be republics, London, a city of 5 million, can burn down and be rebuilt three miles down the Thames, there can be flaws in arguments, etc. These constructions seem to conflate values and functions, refer to impossible entities, run-together geographical and constitutional conceptions, treat abstract and concrete dimensions on a par etc. All of this should discourage the jump from linguistic or conceptual analysis to metaphysics, since, "if you give a metaphysical interpretation to these things, you run right off into impossible conundrums" (p. 382). If we are interested in what is really distinctive about human language, it behooves us to find out just how labile our words/concepts are.[27]

Several other papers in the volume directly speak to this enterprise. For example, **Rochel Gelman** (chapter 15) distinguishes between *core* and *non-core* domains of knowledge. The former consist of "skeletal" innate mental structures

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[27] Paul Pietroski has recently emphasized this (cf. Pietroski 2010).

that "direct attention and permit the uptake of relevant data in the environment" (p. 226). Such a skeleton seeds the acquisition process by providing "a set of coherent principles that form a structure and contains unique entities that are domain specific" (p. 226). Gelman provides a concise description of the properties of core domains (pp. 228–229), which should look familiar to readers of this journal as it well describes how linguists have been thinking of principles of UG for the last 50 years, as Chomsky notes (p. 384). Gelman also provides an illustration for the domain of natural number and isolates three principles that seed the counting process (p. 231). It would be interesting to consider how these specific principles may or may not be related to those that undergird linguistic knowledge, especially given speculations that "the numbering system is just a trivial case of language" (p. 33) and the apparent fact noted in Gallistel's chapter that birds are pretty good arithmeticians.[28]

Lila Gleitman (chapter 16) offers a second illustration, closer to home. She has long observed how hard word learning actually is, in part because of the "richness of the stimulus" (p. 210). The hard problem, she persuasively demonstrates, is to understand how the infant is able to "represent […] context 'in the right way'" (p. 207). How do children know to ignore some features of a scene as irrelevant to lexical meaning in some contexts while attending to them correctly in others? She illustrates the problem by reviewing some experiments by Gordon (2003) on 10-month-olds (who as yet utter no words) in which a stuffed bear passed between two participants is gauged relevant in scenes depicting givings but not huggings. This suggests that the capacity to index participants to event types is 'epistemologically prior' to word learning and serves as a boost to that process. Similarly, later on the richness of the stimulus is similarly tamed by considering how discourse and thematic roles are mapped into syntactic frames and how these can be used to constrain mappings to meaning. For example, it appears that both adults and children use an interlocutor's eye gaze to pick out the subject of a sentence and this in turn helps to disentangle fleeings from chasings (p. 245). Mapping words to concepts is a very complex affair in which "a mosaic of conspiring cues […] are exploited […] to converge almost errorlessly on the lexicon of the native tongue" (p. 211). Not surprisingly, these cues appear culled from various parts of our cognitive apparatus and interact with the specifically linguistic properties of FL/UG in very complex ways.

4. There Is Much More

There are many more interesting papers in this volume that we do not have the competence or space to discuss in detail. All of these are summarized in the useful and detailed introduction by **Massimo Piattelli-Palmarini, Pello Salaburu and Juan Uriagereka**. In chapter 6, **Gabriel Dover** argues strongly against the idea the "laws of form" significantly constrain biological variation, and makes some speculations regarding free will.[29] **Donata Vercelli and Massimo Piattelli-**

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[28] See the very brief discussion between Gelman and Higginbotham on p. 236 as well.

[29] With regard to free will, we are not persuaded that Dover really addresses the problem at all. Dover's focus is on the first-person subjective problem of free will. He suggests that free will is "a situation of rapidly and subtly changing outcomes as degenerate neuronal net-

Palmarini (chapter 7) give a useful introduction to epigenetics (the study of heritable changes which are not caused by changes in DNA), and discuss with Dover its implications for the biology of language. **Christopher Cherniak** outlines the case for "non-genomic nativism" with regard to the optimization of neural connections (chapter 8). **James Higginbotham** makes a case for a robustly truth-conditional approach to semantics, and presents a brief analysis of typological variation in the availability of resultative constructions (chapter 10). **Juan Uriagereka** discusses the problems posed by uninterpretable Case features, and a possible solution in terms of the "viral" theory of Case (chapter 12). **Angela Friederici** presents fMRI data suggesting that different regions of the brain are responsible for processing local dependencies vs. hierarchical structures (chapters 13 and 22). Chapter 14 contains a fascinating round table discussion with **Cedric Boeckx, Janet Dean Fodor, Lila Gleitman, and Luigi Rizzi** on "Language Universals: Yesterday, Today and Tomorrow". **Itziar Laka** clarifies certain questions regarding the domain-specificity of UG principles (chapter 20). **Núria Sebastián-Gallés** discusses neuro-imaging studies which shed light on the question of why there is so much variation in the extent to which second language acquisition is successful (chapter 21).

It is fashionable nowadays to lament the current state of generative grammar in general, and minimalist syntax in particular. This volume serves as an excellent antidote. In fact, it is a pretty good advertisement, in our view, for the intellectual vitality of the current enterprise. Minimalism has added a new set of questions to the research agenda, both within syntax proper and in the neighboring domains of psychology, neuroscience, and biology. Some of these links are more tenuous than others, as we should expect. Nonetheless, it is clear, at least to us, that research on the structure of FL is thriving, and that this is partly due to the fecundity of the core ideas of the Minimalist Program. From where we sit, syntax has rarely looked healthier.

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works switch from one quasi-stable state of topology to others […]. At the level of biology […] there is an unbroken route of cause and effect […] but at the level of our sense of what happened, we feel that at the threshold of the final step […] is one for us alone to decide". Leaving aside the decorative references to degenerate neural networks, this is just a statement of the standard determinist position, i.e. that for any given pair of a pre-decision state S1 and post-decision state S2, S1 lawfully and deterministically brings about S2, even though we (erroneously) believe that S2 is contingent both on S1 and some additional act of volition. It remains unclear on Dover's account why we feel as if we're making free decisions. (Why don't we just feel as if we're switching between quasi-stable states?)

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★ REVIEWS ★

Signs Pointing in a New Direction: A Biosemiotic Framework for Biolinguistics

Favareau, Donald. 2010. *Essential Readings in Biosemiotics: Anthology and Commentary*. Biosemiotics 3. Berlin: Springer.

by Liz S. Swan

Biosemiotics is considered by some to be a new philosophy of biology in the sense that it recasts our understanding of the living world as being *replete with meaning* from the interaction of molecules to the intercommunication of human societies — a grand idea which is systematically missing from the modern biological framework. But because contemporary philosophy of biology grew out of the analytic tradition and thus is mainly concerned with analysis of the key terms and concepts endemic to biology and, to a lesser extent, the philosophical recasting of outstanding puzzles in biology, others instead argue that biosemiotics is a new kind of science, an empirical method through which we can discover the codes of life (see especially Barbieri 2008, also contained in the present volume).

As a philosopher of science, I think both views are correct: No great revolution in science occurs without an attendant sea change in our philosophical world view. A lesson from Thomas Kuhn is that paradigm shifts affect not only the practice of science but also our thinking about science insofar as how and to what extent it contributes to our understanding of the world. I believe that **Donald Favareau**'s new volume in the Springer *Biosemiotics* book series, *Essential Readings in Biosemiotics: Anthology and Commentary*, demonstrates that biosemiotics has captured the attention of both those who do science and those who think and write about science, as well as that special third category of those who do both — a category that is well represented within biosemiotics.

Why should biolinguists take an interest in biosemiotics? First, there is the sentiment held by some in the biosemiotics community that it would be counterproductive for the two fields to operate in isolation from each other, as has traditionally been the case, since there is considerable overlap between the empirical and philosophical questions pursued by each (see, for example, Augustyn 2009). For starters, the two fields are in agreement that human language is a biological phenomenon. However, a distinction between the two fields can be identified with regard to their respective methodological foci: While biolinguistics focuses on human language and tries to embed it conceptually and empirically among grander patterns in the natural world, biosemiotics focuses more fundamentally

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on sign processes in the living world, of which human language is but one example. A central task of biosemiotics thus becomes one of elucidating the continuity between human language and all other forms of meaningful sign processing in the biosphere, and as such, biosemiotics can provide a comprehensive theoretical framework necessitated by the biolinguistic thesis that language emerges from human biology.

A more specific point is that the concept of organic codes surfaces in much of the biolinguistics literature — but when it is not explicitly acknowledged and elaborated on, an opportunity for fruitful collaboration between the two fields is regrettably lost. For at least these reasons, researchers in both fields would do well to acquaint themselves with their conceptual neighbor — a task that is made more feasible for biolinguists by Favareu's *Essential Readings*, which offers a very comprehensive coverage of the field's historical depth and contemporary theses.

The extremely rich historical introduction, written by Favareau, constitutes reason enough to acquire this book. It provides an essential roadmap of the history of the emerging interdiscipline of biosemiotics, critical for those new to the field, and relays the very engaging story of how thinkers from different parts of the world working on seemingly very different problems related to the origins and nature of life, and the evolution of meaning in organisms, found one another and established the now definitive field of biosemiotics. In four main parts, the almost 900-page volume covers everything from key historical figures in both the life sciences and philosophy whose research and thinking paved the way for what would become biosemiotics, to contemporary approaches and outstanding problems in the field today. Each of the twenty-four entries in the volume is preceded by a helpful introductory commentary by Favareau. As such, the book is a must-have for those well-entrenched in the field as well as those just beginning to learn about this novel approach to conceptualizing the role of signs and sign processes in the biosphere.

With selections from Thomas Sebeok, Jakob von Uexküll, **Charles Sanders Peirce**, **Charles Morris**, and **Juri Lotman**, Part I of the book, "Sebeok's Precurosrs and Influences", provides a necessary historical overview of the seminal 19th and 20th century scientists and philosophers whose early efforts to legitimate scientific investigations into meaning, and to understand the role of signs, signals, and symbols in the natural world (including in the context of human language) eventually gave way to the contemporary field of biosemiotics. We learn, for instance, how the contemporary notion of *strong continuity* between life and mind, credited in large part to the thought of 19th century philosophers such as Herbert Spencer and John Dewey, has important overlap with the work of experimental and theoretical biologists of the same time period who were keen on identifying what, if anything, distinguished human symbol use from that found in all organisms in the natural world (see Swan & Goldberg 2010 on exactly this question).

In Part II, "The Biosemiotic Project of Thomas A. Sebeok", we read excerpts from some contemporaries of Sebeok, such as the very interesting excerpt from the Swiss zoologist **Heini K.P. Hediger**, whose work goes a long way in explaining what went so horribly wrong in the lab of former Harvard primatologist Marc Hauser, a salient example of observer bias based on strongly wished for results, which Hediger notes as one of animal psychologist Otto Koehler's strict warnings regarding animal behavior studies. In **Marten Krampen**'s very interesting thesis on *phytosemiotics* we learn that semiosis can be extended to the world of plants, a notion that is reminiscent of Aristotle's tripartite system of the vegetative, animal, and rational modes of the natural world. Favareau interestingly points out in his introduction to this entry that Krampen's excerpt makes a postmodern contribution to biosemiotics in that the very idea of phytosemiosis challenges the duality of mind and matter articulated by Descartes. The last essay in this section, a manifesto of sorts written in 1984 by **Myrdene Anderson** and her colleagues, advocates for semiotics as a progressive conceptual bridge between the sciences and the humanities, and is followed by nine pages of references and thus, in and of itself, serves as an excellent sampling of the important literature in the field over the past three decades.

Part III of the book, "Independent Approaches to Biosemiotics", starts off with a wonderfully insightful essay on the history of theoretical biology and its crossover with biosemiotics, written by **Kalevi Kull**, the first ever full professor of biosemiotics and founder of the first Ph.D. program in the field at his home institution, the University of Tartu in Estonia. Next is an essay by **Friedrich Rothschild**, the very first researcher to use the term 'biosemiotics' at a psychology conference in New York in 1961 (Rothschild 1962). Also in this section are essays by preeminent biochemist **Marcel Florkin**, physicist **Howard Pattee**, and anthropologists **Gregory Bateson** and **Terrence Deacon**, all of whom have made major contributions to biosemiotics from their respective disciplines.

The fourth and last part of the book, "The Contemporary Interdiscipline of Biosemiotics", begins with an excerpt from **Jesper Hoffmeyer**'s eloquently written *Biosemiotics: An Examination into the Signs of Life and the Life of Signs,* wherein we learn about his and Claus Emmeche's code-duality hypothesis according to which the essence of living things is their ability to represent themselves in both digital and analog codes — in genetic transmission and phenotypic expression, respectively. This section also includes entries from **Claus Emmeche** and colleagues, **Anton Markos**, **Søren Brier**, and **Günther Witzany** — all influential actors in contemporary biosemiotics.

The section ends on a high note with an entry from **Marcello Barbieri**, who was working in the field long before it was called biosemiotics; his earlier work referred to the discipline as 'semantic biology'. In this selection (a reprint of his 1998 article), Barbieri explains his code-based approach to biosemiotics and how it provides "a new understanding of life" (which is the subtitle of the paper) in that it overturns three fundamental assumptions of modern biology. He argues that (i) the cell is a true semiotic system (and not a genotype-phenotype duality), (ii) the genetic code is a real code, and thus not amenable to physical reduction, and (iii) evolution is due not just to natural selection, but also to natural conventions, which are the many and varied codes of the living world.

The volume concludes with very comprehensive bibliographies for the editorial commentary that accompanies each section of the book, as well lists of suggested reading. Though the book's size might seem daunting, it is important to note that most of the first half of the book presents historical content that helps to contextualize the emergence of biosemiotics as a distinct field of research which is very helpful to those new to the field. The book is a must-have addition to the libraries of research institutes and university departments that are engaged in progressive approaches to the nature of life and organic cognition, specifically in its offering a new way to conceptualize the role of signs and sign processes in cultivating meaning in the natural world from the ground up.

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★ FORUM ★

A Report on the *Workshop on Complexity in Language: Developmental and Evolutionary Perspectives*

Tao Gong & Christophe Coupé

1. Overview

Complexity can be viewed as "the property of a real world system that is manifest in the inability of any one formalism being adequate to capture all its properties" (Mikulecky 2001: 344). In the past few decades, this notion has raised significant interest in many disciplines, from physics to biology, mathematics, artificial intelligence, etc. (Waldrop 1992, Simon 1996, Dahl 2004, Gell-Mann 2005, Hawkins 2005, Friederici *et al.* 2006, Risager 2006, Boogert *et al.* 2008, Larsen-Freeman 2008, Liu 2008, Riecker *et al.* 2008, Lee *et al*. 2009, Faraclas & Klein 2009, Givón 2009, Mitchell 2009, Pellegrino *et al.* 2009, Cyran 2010, Trudgill 2011, and McWhorter 2011, among others); more recently, this cross-disciplinary endeavor has reached linguistics, and scholars of various theoretical backgrounds have been keen to test its relevance to language (e.g., Gibson 1988, Changizi & Shimojo 2005, Papagno & Cecchetto 2006, Lee *et al*. 2007, Suh *et al*. 2007, Miestamo *et al*. 2008, Givón & Shibatani 2009, Sampson *et al*. 2009). However, it is still unclear what complexity actually means in linguistics, what yardstick can be used to measure complexity, especially in comparing language varieties, and what conceptions are relevant to accounts of structures and functions of languages.

For the purposes of fostering a dialog between scholars of diverse but complementary backgrounds on these topics, Salikoko S. Mufwene, then a fellow at the Collegium de Lyon, in collaboration with researchers at the Laboratoire "Dynamique du Langage" at the Université de Lyon, convened a workshop on *Complexity in Language: Developmental and Evolutionary Perspectives* on 23–24 May 2011. Participants included linguists, anthropologists, statistical physicists (modeling communal aspects of language), computer scientists, and mathematicians. Most of them agree on seeing language as a *complex adaptive system* as described by Steels (2000) and Beckner *et al.* (2009). According to this view, a linguistic system involves a number of interacting units and modules that generate structural and interactional complexity on several levels. Meanwhile, these scholars, based on their expertise, have distinct research foci on linguistic complexity. The workshop consisted of 14 talks touching upon various topics related to linguistic complexity, such as (i) where linguistic complexity lies, (ii) how it emerges and evolves ontogenetically or phylogenetically, and (iii) how it is measured using

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approaches adopted and adapted from disciplines other than linguistics. In this review, we will go over these talks and present our opinions on future research of linguistic complexity.

2. Describing Complexity in Linguistics

Among the participants, linguists presented their complementary theories of linguistic complexity. The organizer of the workshop, **Salikoko S. Mufwene** (University of Chicago & Collegium de Lyon), divided linguistic complexity into (i) *complexity within a communal language*, which deals with the dynamics that produce communal norms, (ii) *bit complexity*, which reflects the number of units and rules in the lexicon, syntax, phonology, and other linguistic modules, and (iii) *interactional complexity*, which refers to the interactions of units and rules within their respective modules and of latter with one another. He pointed out that language evolved as a communal technology for mapping conceptual structures onto physical structures, and that all these forms of complexity emerged due to ecological and social factors (Mufwene, in press), interaction constraints and self-organization (Camazine *et al*. 2001).

William Croft (University of New Mexico) viewed linguistic complexity as *structural complexity* existing in modern languages and *evolutionary complexity* echoing the increasing structuration that led to modern languages. He claimed that the selective pressure for structural complexity came from the necessity of establishing common ground in joint actions (Bratman 1992, Tomasello *et al*. 2005), and that it was human social cognitive abilities that helped build up such common ground. Based on the evidence from language acquisition and the evolution of semasiographic systems such as images, notations and writing, he summarized the key features of the evolution of social cognition in humans, including gradualness, context-dependency and multimodality, which further inspired some speculations on evolutionary complexity in language.

William S-Y. Wang (Chinese University of Hong Kong) argued that language was a diffusive and heterogeneous system. Socio-cultural reasons drove the evolution of linguistic complexity, and therefore measuring current linguistic complexity, such as phonological complexity, could shed light on some age-old controversies regarding past linguistic communities, such as whether language origin was a monogenetic (Atkinson 2011) or polygenetic (Freedman & Wang 1996, Coupé & Hombert 2005). He also discussed one outcome of linguistic complexification, namely lexical and construction ambiguities. He argued that cross-language studies of these ambiguities could yield important insights into linguistic and cognitive universals.

Barbara Davis (University of Texas at Austin & Collegium de Lyon) proposed *a biological-functional perspective* to phonological acquisition, viewing the acquisition of phonological complexity as a consequence of interactions of biological and social components of language to achieve maximal functional efficiency (Davis *et al*. 2002). She introduced *frame-content theory* (MacNeilage 1998), which follows this perspective and aims to explain the acquisition of one type of phonological complexity, namely the C(onsonant)V(owel) co-occurrence patterns in the world's languages. This theory is supported by the experimental results of

English or Korean learning (Lee *et al*. 2010) infants, as well as the acquisition data of other languages.

Albert Bastardas-i-Boada (University of Barcelona) generalized a philosophical, holistic view of language contact. He conceived of an ecosystem of language, including brain/mind, social interaction, group, economics, media, and political factors. All these dynamic factors co-produced and co-determined the forms, usage, and evolution of language.

Unlike linguists, the anthropologist **Thomas Schoenemann** (Indiana University) focused on complexity in the physical substrate of language (the human brains) and of human behaviors. He advocated the theory of *language-brain coevolution* (Deacon 1997), and suggested that an increasing complexity of hominin conceptual understanding led to an increasing need for syntax and grammar to fulfill efficient communications (Schoenemann 1999). He argued that concepts were based upon networks connecting different brain regions, and that the size of those regions across species was proportional to the degree of elaboration of the functions they underlie. In the past, the increase in brain size was correlated with the increase in degree of specialization of parts of the brain (Schoenemann 2006).

The genetic linguist **Jean-Marie Hombert** (University of Lyon 2) focused on the relation between population size, social complexity and language dispersal. Based on genetic and demographic data, he suggested that Pygmy hunter-gatherers and Bantu-speaking farmers in Central Africa shared a common ancestry (Quintana-Murci *et al*. 2008, Berniell-Lee *et al*. 2009). This case study illustrated that population size and hierarchy could be two important factors within linguistic communities that helped develop linguistic complexity.

3. Measuring Complexity in Linguistics

Apart from describing and circumscribing linguistic complexity, many talks tried to propose general procedures or quantitative measures to evaluate different aspects of linguistic complexity. Artificial intelligence expert **Luc Steels** (University of Brussels & Sony CSL, Paris) presented a general procedure to account for linguistic complexity. This procedure includes five steps: (i) describing a complex linguistic structure, (ii) identifying its function, (iii) reconstructing processing and acquiring mechanisms for this structure, (iv) surveying its variations in languages, and (v) identifying its selective advantage. Such an approach helped pinpoint the different factors that contributed to linguistic complexity. In addition, Steels presented several simulation studies that explored the evolution of complexity in semantics and syntax. These studies supported his *recruitment theory* of language evolution (Steels 2009), stating that (i) strategies and structures that could satisfy communicative needs, reduce cognitive efforts, and increase social coherence could be adopted by language users and survive in languages, and (ii) the emergence of linguistic complexity was a process of self-organization of existing systems and of recruitment of new mechanisms in a cultural environment.

Statistical physicist **Vittorio Loreto** (Sapienza University of Rome & Institute for Scientific Interchange, Torino) pointed out that statistical physics served as an efficient means to study linguistic dynamics and complexity (Loreto *et al.* 2011). Relying on language game simulations (Baronchelli *et al.* 2006, 2010, Puglisi *et al.* 2008), he argued that this approach could help understand: (i) How collective behaviors (e.g., common lexical items or linguistic categorization patterns) originated in local interactions, (ii) what were the minimum requirements for a shared linguistic feature to emerge and diffuse, (iii) how to examine asymptotic states in language evolution, and (iv) what roles population size and topology played in language evolution.

Mathematician **Ramon Ferrer-i-Cancho** (Universitat Politècnica de Catalunya) analyzed the effect of two quantifiable constraints on the word order bias in languages, namely, the predictability of a sequence of words and the amount of online memory for handling the head-modifier dependencies (Ferrer-i-Cancho 2008). The results obtained from this mathematical analysis, and also observed in simulation studies (e.g., Gong *et al.* 2009), illuminated empirical findings (e.g., Dryer 2008) and inspired further discussions (e.g., Cysouw 2008).

Linguist **Lucía Loureiro-Porto** (University of Palma de Majorca), collaborating with statistical physicist Maxi San Miguel and Xavier Castelló, defined two quantitative parameters, *social prestige* of different languages and *individual volatility* (speakers' willingness to shift their current language to another), to examine the effect of social complexity on language competition. Using agent-based modeling and two sets of abstract equations of language competition (Abram & Strogatz 2003, Minett & Wang 2008), these scholars compared language competition in different social networks, and observed that (i) volatility was more powerful than prestige to cause language death and (ii) bilingualism accelerated language death (Castelló *et al.* 2008).

Apart from artificial simulations, evolutionary linguist **Bart de Boer** (University of Amsterdam) argued that *the cultural learning paradigm* (Galantucci 2005, Scott-Phillips & Kirby 2010) could help distinguish the effect of cultural learning from the effect of cognitive biases on linguistic complexity. His case experiment of whistle transmission in chains of human subjects revealed that the emergence of complex combinatorial structures was mainly due to cultural learning, with only limited influence from cognitive biases.

Psycholinguist **Fermin Moscoso del Prado** (University of Lyon 2) adopted the general framework of information theory and applied Gell-Mann's (1995) notion of 'effective complexity' to language. Accordingly, the complexity of a linguistic system could be reflected by the length of the most compact grammar that describes the structural regularities of this system. He showed how to mathematically apply this approach to large text corpora, and how different linguistic components — lexicon, syntax, pragmatics or morphology — could be evaluated independently. A comparison between English and Tok Pisin corpus indicated that it was erroneous to claim that creole/pidgin grammars are simpler (McWhorter 2001).

Linguists **Christophe Coupé, Egidio Marsico, and François Pellegrino** (University of Lyon 2) concentrated on phonological systems and proposed a quantitative approach to analyze their complexity. Based on a genetic linguistics balanced dataset of 451 phonological inventories, namely the UPSID database (Maddieson 1984, Maddieson & Precoda 1990), they measured the strength of interactions between phonemes, and suggested that the degree of complexity of the inventories was actually quite low. An evolutionary model was then derived from the synchronic data in an effort to further assess the extent to which the structure of the inventories could be understood and predicted (Coupé *et al.* 2009).

4. Future Research of Complexity in Linguistics

The workshop gathered many state-of-the-art studies on linguistic complexity, and offered opportunities for interested scholars to exchange ideas, methods, and findings across research areas and disciplines. It provided several important guidelines for the future research in linguistic complexity. First, complexity in linguistics is manifest in many aspects, including not only linguistic structures, but also population interactional dynamics, and cultural environments. As for the linguistic structures, variation and diversity of languages provide a rich repertoire of phenomena, which should be considered when we devise general theories of structural complexity (Evans & Levinson 2009). To this end, the typological database, namely the *World Atlas of Language Structures* (*WALS*, see http://wals.info), which records different types of structural variations across many of the world's languages, serves as an important resource for future research.

As for the language users, neurolinguistic and psycholinguistic research, which examines empirical bases of linguistic behaviors in the human brain and traces individual differences in language acquisition and processing, will bear significantly on the embodied aspects of linguistic complexity. Meanwhile, structural complexity reflects conceptual/cognitive complexity. Examining structural complexity could help us better understand the *Sapir–Whorf Hypothesis* (Sapir 1929, Whorf 1940) and discuss how linguistic structures and usage influence human thoughts and non-linguistic behaviors.

As for the cultural environment, research on language contact from historical linguistics, sociolinguistics, population-based studies (e.g., Mufwene 2001, 2006, Ansaldo 2009) as well as simulations (e.g., Steels 2000, Brighton *et al.* 2005, Gong 2010) will yield useful insights on how interactions and cultural variations affect linguistic complexity, and vice versa.

Regarding these various approaches, challenge remains to cross the gap between quantitative approaches and wider and more conceptual notions, such as *bit complexity*, *evolutionary complexity*, or *structural complexity* — to recall only a few mentioned earlier. Quantitative approaches may provide figures, but these figures sometimes fail to necessarily uncover the true mechanisms at hand. Meanwhile, conceptual notions are instructive, but sometimes these notions suffer from a lack of empirical studies to support them. Therefore, revisiting earlier theories with the vocabulary and concepts of complexity theory is undoubtedly useful to better frame intricate phenomena, and further articulation with smaller scale aspects could be even more precious.

Second, a significant question concerning linguistic complexity that requires further investigation is the degree to which all languages are equally complex, or, whether there is compensation among linguistic components, say, whether a language with a rich morphology tends to exhibit a simple phonology or syntax. Such assumptions are found in most introductory textbooks to linguistics, yet there are very few attempts to provide strong arguments for or against them. A reason for this is the difficulty in coming with complexity measures that can address the various linguistic components, such as lexicon, syntax, phonology, morphology, and so on in an integrative manner. Most current studies are usually confined to one of these domains, and need to be revised to reach beyond. To this end, databases like *WALS* may once again come in handy, and corpora or entropy based approaches or measures (such as Fermin Moscoso del Prado's) seem promising for the future research.

Third, unlike previous theories that relied upon biological evolution to explain the origin of language and the evolution of linguistic universals (e.g., Pinker & Bloom 1990), modern theories pay great attention to cognitive abilities in humans and cultural processes in which language is acquired and transmitted. Language is inseparable from its socio-cultural environment and cultural evolution is too rapid for biological evolution to fix adaptations to arbitrary features of language (Christiansen & Chater 2008). Therefore, many universal properties of language should be ascribed to general cognition and cultural evolution (Evans & Levinson 2009, Dunn *et al.* 2011). The different angles adopted by many talks in the workshop to describe and explain linguistic complexity — from joint action, shared intentionality, brain-language co-evolution, individual processing and memory constraints, to human migration, population size and hierarchy, social networks and cultural learning — are actually falling into these two perspectives. Incorporating these perspectives into linguistics will greatly change the nature of this discipline (Levinson & Evans 2010).

Finally, it seems that no single discipline can alone account for all aspects of linguistic complexity. On the one hand, although linguists can carefully record in detail different types of variations in modern languages, powerful methods are needed to bring light to the correlations hidden in surface structures, to notice the selective pressures cast by other relevant factors, or to reconstruct the evolutionary trajectories leading to those variations.

On the other hand, although the research methods from other disciplines, such as genetics, anthropology, statistical physics, mathematics, and computer modeling, can quantitatively shed light on aspects of linguistic complexity, without sufficient guidance from linguistics, studies adopting those methods may pay unjustified attention to trivial factors or overlook more significant ones. For example, Atkinson's (2011) mathematical analysis based on the phonemic diversity in languages was questioned by some of the workshop attendees for disregarding the influence of population size or language contact. Dunn *et al.*'s (2011) approach to word-order typology, inspired by evolutionary biology, was also criticized for ignoring the powerful effect of contact on typological change.

Therefore, questions concerning linguistic complexity have to be tackled based on a multi-disciplinary approach, a prerequisite to making sense of seemingly contrary positions, providing alternative perspectives, and ruling out solutions plausible only in the framework of a single discipline. This approach could offer the best prospect of arriving at an adequate and comprehensive understanding of linguistic complexity (Bickerton & Szathmáry 2009).

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