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★ EDITORIAL ★

# A Brief Note on the Scope of *Biolinguistics*

Cedric Boeckx & Kleanthes K. Grohmann

We do not endorse Paul Postal's opinion expressed in this contribution. However, we offered him the space to respond to John Collins' remarks, which he used amply. In addition, we would like to stress that, as journal editors, we always welcome negative replies to what gets published in *Biolinguistics*, whether an article, a brief, a review, or a forum piece.

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# Computational Phenotypes: Were the Theory of Computation Meets Evo–Devo

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This article argues that the Chomsky Hierarchy can be reinterpreted as a developmental morphospace constraining the evolution of a discrete and finite series of computational phenotypes. In doing so, the theory of Morphological Evolution as stated by Pere Alberch, a pioneering figure of Evo–Devo thinking, is adhered to.

*Keywords:* cognition; computational complexity; evolutionary developmental biology; faculty of language; morphological evolution

## 1. Introduction

This article offers an internalist explanatory model for the evolutionary origins of the computational system of the human faculty of language (FL), inspired by ideas worked out during the last decades in the field of Evolutionary Developmental Biology (Evo–Devo). In particular, we adopt the framework of 'morphological evolution' elaborated by Pere Alberch (1954–1998) in a series of papers published in the eighties and nineties of the last century (namely, Alberch 1980, 1989, 1991).

Evo–Devo aims to explain the origins and evolution of natural designs (phylogeny) by means of hereditary perturbations affecting the developmental plans of organisms (ontogeny); see in particular Hall (1999, 2002), Hall & Olson (2003a), Robert (2004), Carroll (2005), García–Azkonobieta (2005), and Laubichler & Maienschein (2007). Against this background, the essence of Alberch's proposal can be summarized by the following statements. A plan for the devel-

We are particularly grateful to Jordi Alberch for putting us on the right track to find some of his late brother's works and to Laura Nuño de la Rosa for making them available to us. Thanks again to Laura for her detailed reading and comments on an earlier version of this article. We ourselves are responsible for any remaining errors. This work was carried out through the project *Biolingüística: Fundamento Genético, Desarrollo y Evolución del Lenguaje* (HUM2007-60427/FILO) of the Spanish Ministerio de Educación y Ciencia and partially co-funded by FEDER funds (Balari & Lorenzo), also receiving support from the Generalitat de Catalunya through grant 2005SGR 00753 *Lingüística Teòrica* provided to the Centre de Lingüística Teòrica of the Universitat Autònoma de Barcelona (Balari).

opment of an organism consists of a set of morphogenetic parameters (not just genetic factors). Interactions among these parameters are complex and they relate non-linearly with phenotypic outcomes. This means that a parameter can be continuously changing without any significant consequence. However, once a certain critical value is reached, a minor change of the same parameter can be enough to introduce some radical phenotypic manifestation. These minor but far reaching perturbations on development can eventually attain evolutionary significance. Thus, evolutionary novelties can emerge without introducing new developmental factors or changing the nature of interactions. Finally, a system of morphogenetic parameters sets the limits of the forms attainable from such a developmental plan. As a consequence, the direction that development can take from a certain phenotypic state is strongly constrained by the geometry of the parametric space so defined.

The idea that evolution is strongly constrained by the very same factors that strongly constrain the development of individuals is common ground for every Evo–Devo oriented approach. Conversely, this idea is not congenial with the more classical stances of neo-Darwinian selectionism. According to the adherents of Modern Evolutionary Synthesis (MES), natural selection acts on the diversity randomly introduced into populations by point genetic mutations, totally unrestricted in scope (see the classical formulations of Morgan *et al.* 1915, Dobzhansky 1937, Mayr 1942, Huxley 1942, and Simpson 1944; see also Mayr & Provine 1980 for a general overview). Consequently, natural selection is believed to be the only creative force capable of organizing an otherwise amorphous material. Evo–Devoists, in contrast, attribute part of this creative capacity to constraints acting upon development and that limit the scope of attainable designs: They redefine natural selection as a mechanism filtering out those designs which fit environmental and populational conditions more efficiently (see Goodwin 1994: 143 or Wagensberg 2004: 125, as well as Alberch 1980: 664, Oster & Alberch 1982: 455, Alberch 1989: 46–48, and Alberch 1991: 16).

In this article we contend that the Evo–Devo theses thus far advanced can be readily extended to the evolutionary study of the nervous system and cognition (see Griffiths & Stotz 2000, Amundson 2006, Finlay 2007, and Griffiths 2007, for some programmatic attempts in this direction). Our interest will be focused on the case of language, seen as a particular aspect of human cognition and, more specifically, on the computational system in charge of generating internal linguistic expressions (Chomsky 1986). This system is thought to contain some unique features in the context of animal cognition, hence its special interest (Hauser *et al.* 2002).

The main claim of the present contribution is that the different levels of computational complexity reflected in the Chomsky Hierarchy (Chomsky 1956a, 1959) are the possible phenotypes of a cognitive parametric space defined by a restricted set of morphogenetic factors. These parameters are non-linearly related to the development of the cortical resources that supply the memory requirements of each computational model. We argue that the kind of (mildly) context-sensitive grammar which can be attributed to the computational system of FL is an emergent consequence of a minor perturbation affecting the development of a cortico-striatal circuit, once the value of a morphogenetic parameter attains a certain critical value in the course of human evolution. Thus, the adoption of this computational regime by FL can be seen as an evolutionary outcome strictly channeled by the organic conditions settled by the parameters at hand. We present this idea as an alternative view to that held by contemporary evolutionary psychology, whose practitioners defend the view that the mind is a collection of purpose-specific modules, each one an adaptively meticulous answer to environmental (or external) conditions (see Pinker 1997, Plotkin 1997, or Buss 2007, as well as the application of Pinker & Bloom 1990 and Jackendoff 2002 to the evolution of FL). The internalist proposal put forward in this article is based on the idea that the internal organization of the mind is in itself a constraining system that biases evolution in favor of certain forms of cognition and limits the power of the environment in the shaping of the organic design of minds.[1]

This is not to say that environmental and populational factors can be completely put aside by internalist-oriented theories. Actually, no evolutionary theory that ignores the external factors acting as selective criteria for the diversity independently brought into being will be complete. In this sense, we advance a proposal according to which the originally maladaptive character of this feature of the human cognitive phenotype would, in an apparently paradoxical way, have played a crucial role in its fixation as a species feature.

The article is organized as follows. Section 2 is devoted to presenting the basic tenets of Evo–Devo as well as Alberch's model of morphological evolution. Section 3 introduces an extension of this model to cognition and applies it to the evolutionary origins of the computational system of FL. We conclude with some reflections concerning the application of our ideas to explaining the origins of FL in a broader sense.

## 2. Evolving through Development

A somehow unexpected episode in the history of biology was the recent divorce between the study of individual development (the classical subject matter of Embryology) and that of evolution at the species level (in charge of Population Genetics for the most part of the twentieth century). This fact was directly related to the fixing by the MES of the concept of evolution as a series of changes in the patterns of allelic distribution among the members of the same population (see Amundson 2007: Part I and West–Eberhard 2003: chap. 1). The idea has been used to justify the exclusion of the path leading from the variants of genes to the adult features from evolutionary explanations. The underlying argument can be summed up as follows. If the only thing that matters in natural selection is having of those versions of features capable of increasing the reproductive rates of organisms, then evolution can safely be thought of as a continuous redistributive process of the alleles that correlate with those features within the gene pool of a population. As a consequence, (i) natural selection (i.e. the external factors challenging the organism's endurance and, above all, its reproductive

1 On the contrast between externalist and internalist approaches, as well as on the history of the debate, see Alberch (1991: 25–28).

success) can be conceptualized as a mechanism acting upon point mutations randomly introduced on certain genomic positions, and (ii) natural selection can be credited as a creative force working on an unrestricted source of diversity. According to Amundson's analysis, this argument is the agency ultimately responsible for having converted the processes that transform genes into features (or, more generally, the genotype into a phenotype) within MES-oriented biological thinking into a 'black box' (see Amundson 2007: 157 — especially his Figure 6 — and Reid 2007: chap. 1; see also Gould 1977 and Bonner 1982).

### *2.1. Evolutionary Developmental Biology (Evo–Devo): Common Background and Alternative Views*

In our opinion, Amundson's statement is not completely accurate, because MES's vision of the evolutionary process does not only rely on ignoring the complexities of individual development and declaring it inert for any evolutionary concern. Underlying this vision there is also a model of organic development based on a very simple and linear conception of the phenotypic expression of genes, schematically represented in Figure 1 [A]. Such a conception is a necessary condition for upholding a view on organic evolution such as that represented in Figure 1 [B]:

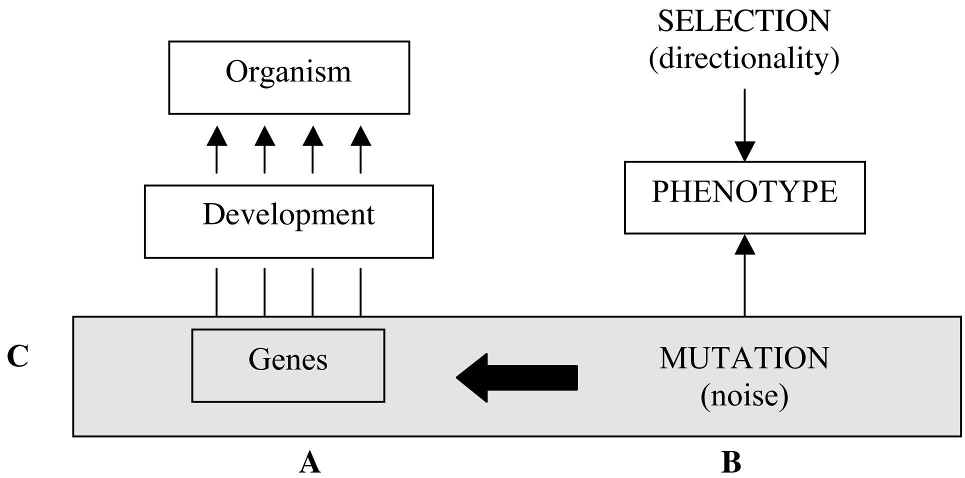


Figure 1: Modern Evolutionary Synthesis.
[**A.** Development. — The role of genes is central in the evolutionary process; they correlate with the phenotype in a simple and linear fashion. **B.** Evolution. — Natural selection acts creatively, imposing order on random point mutations, a noisy and unrestricted source of diversity. **C.** As a consequence, development (**A**) lacks any causal role in the evolutionary process (**B**).]

There are many factors that lead us to the conclusion that the omission of development from evolutionary matters cannot be sustained as straightforwardly as the argument seems to indicate. Some of these reasons are even coherent with other basic tenets of MES. For example, it should be clear to everyone that the simple fact of being capable of reaching the adult state is as important as being an adult optimally fitted to overcoming all kinds of environmental aggression. This implies that alternative routes of organic development can also reasonably be deemed targets of natural selection. As Gilbert (2003: 3) aptly puts it: "Every animal has to function as it builds itself". Consequently, a developmental path that makes the organism more robust from the start or allows an earlier emergence of certain key features, among other possibilities, is probably to be selected instead of other paths. The organism will obviously flourish and proliferate within the population. With a move like this, development can be added 'without tears' to the agenda of MES oriented approaches.[2] One only needs to conceptualize a developmental path as a specific phenotypic manifestation that, according to the Mendelian–Morganian idealization, correlates with certain genomic positions in a simple and linear fashion. Actually, this is the theoretical direction taken by a particular Evo–Devo trend, which relies on the assumption that evolution is mainly due to point mutations affecting genomic positions in charge of the regulation of the genetic activity during development. This view, accessibly introduced in Carroll (2005), can safely be judged a constructive enlargement of the strictly genocentric model of the MES.

However, this model is explicitly rejected by a number of Evo–Devo practitioners who share the belief that (i) genes are not the only causal agents in development, and that (ii) they are not the only developmental material capable of being transmitted from one generation to the next (see Figure 2 [A] below). These assumptions introduce a new and extended notion of heredity into evolutionary theory and they open an important conceptual breakdown with more orthodox forms of neo-Darwinism (see Jablonka & Lamb 2005 as a case in point). Underlying this theoretical move is the following line of reasoning:

(A) Genes are part of complex developmental processes in which other non–genetic factors are also causally involved (such as cellular products, mechanisms of cellular communication, intermediate phenotypic states, environmental factors, behavioral practices, and so on);

(B) to the extent that (i) all these factors exhibit some degree of individual variation, (ii) the variation is persistent throughout generations, and (iii) it can have repercussions on the unequal reproductive rates attained by the individuals so differentiated, it is possible to conclude that they all have a role in evolution not different from that customarily credited to genes;[3]

(C) hence, the Mendelian–Morganian idealization (establishing that the only evolutionary relevant correlation is that between genetic factors and phenotypic features) is untenable.

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2 Waddington's (1957) concept of 'canalization' can be seen as a pioneering formulation of this stance. Waddington contended that developmental paths become stabilized and strengthened by continuous exposure to the rigors of natural selection. As a consequence, certain biases or constrictions on development could be explained as a direct consequence of standard Darwinian selection (for some comments on this matter, see Maynard Smith *et al.* 1985: 270).

3 In Hall & Olson's (2003b: xiv) words: "Phenotypes and the processes that produce them are subject to selection; cells, embryos, and modifications of genetic and developmental processes are as much the raw material of evolution as are genes and mutations".

This is a non-trivial achievement of modern evolutionary thinking. However, it has not given place to a monolithic theoretical frame, but to a cluster of different and somehow confronted perspectives (Figure 2 [B]).[4] The main theoretical parameters underlying the diversity of approaches in this research field have to do with the number and levels of factors involved in developmental processes, the role and relative significance of each one, the nature of the interactions they sustain and the way they correlate with the phenotypic outcomes (see Robert *et al.* 2001 and especially Robert 2004). As a consequence, Evo–Devo houses theoretical stances as varied as those that maintain intact the "genetic program" metaphor (as Carroll 2005), those that extend the metaphor beyond the genetic factor and introduce the idea of a multidimensional "developmental program" (see Keller 2000 and Moore 2001), or even those that abandon the quasi-algorithmic image of development of the "program" meta-phor and endorse instead a concept of "developmental system" with complex interactions among factors of very different sorts that relate non-linearly with the successive phenotypic stages (see Griffiths & Gray 1994, Oyama 2000a, and Oyama *et al.* 2001).

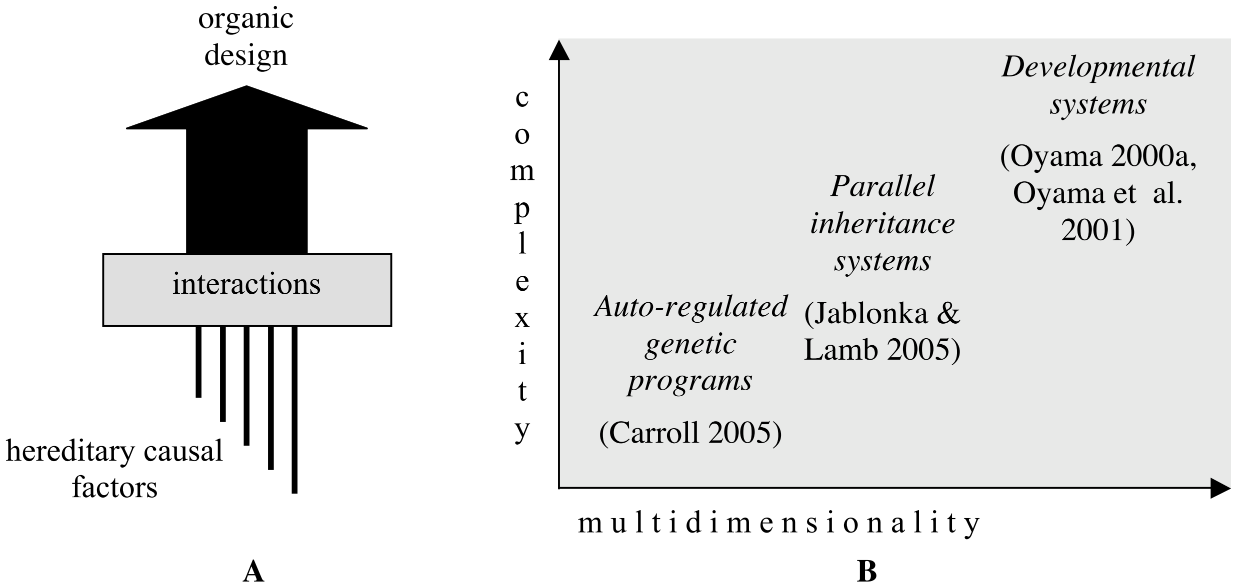


Figure 2: Evolutionary Developmental Biology (Evo–Devo)

[**A.** Organic designs are the outcome of complex developmental processes involving different types of factors, all relevant from an evolutionary point of view. **B.** The diversity of the factors (multidimensionality) and the complexity of their interactions (complexity) are the main theoretical parameters underlying the plurality of Evo–Devo thinking.]

The following contentions, even if there is not complete consensus about them, are unquestionably at the core of Evo–Devo thinking:

[4] As Hall & Olson (2003b: xv) put it: "No unified theory of Evo–Devo exists".

(A) Neither natural designs nor the successive stages of an organism until attaining its adult steady state are preformed in genotypes;

(B) every particular stage in development is the result of complex interactions among multiple factors,[5] including the phenotypic state previously attained;

(C) genes are not the only developmental agents with a causal role in the evolutionary process; and

(D) the role of genes in development and evolution is not in a higher rank with respect to all the remaining factors.

Within this framework, the rigidity of the MES image on the evolutionary process, based on the centrality of genes and the omnipotence of natural selection in the shaping of organisms, is being left behind by Evo–Devo practitioners. In its place, a much more worked out image of development is currently allowing an understanding of ways in which the evolutionary process can be biased in favor of certain formal solutions even at the cost of losses in terms of fitness. In this regard, the following contentions are also highly representative of present-day Evo–Devo thinking:

(A) Certain developmental pathways are extremely conservative, in the sense that they are manifested, with minor modifications, across very distant taxa. The use of those very similar pathways in very different developmental contexts, resulting in a high degree of phenotypic variation, is a significant feature of evolution;[6]

(B) the above-mentioned modifications of the developmental pathways can be classified using a limited (and in itself constraining) inventory of descriptive categories: Displacements of onset and offset points, modifications of the rates of growth, alterations of the terminal state, changes in the plan of execution, and so on (see, among other sources, Gould 1977: Part II, Parker 2000, and Alba 2002);

(C) both the persistence of developmental pathways and the restrictive character of their possible routes of change must be seen as forces counteracting the pressures of the environment. As such, they must be acknowledged with a creative character similar (if not superior) to that of natural selection.

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5 Johnston & Edwards (2002) is an especially significant model, based on fourteen different kinds of factors and almost thirty paths of interaction.

6 This is a very old idea, probably due to Aristotle. It is also one of the defining notions of nineteenth century (pre- and post-Darwinian) embryology, especially for the defenders of the idea that ontogeny recapitulates phylogeny. See Gould (1977: Part I) for a detailed historical account. It is fair to say Evo–Devo has rescued and updated this idea. Shubin (2008) contains an interesting and accessible revision of the similarities of the genetic and developmental background across the species.

These statements do not entail that natural selection is completely neglected by Evo–Devoists, who have just opened a discussion concerning the extent of creativity of the Darwinian mechanism relative to that of the constrictions imposed by development. Actually, the different Evo–Devo approaches introduced so far maintain divergent positions on this issue, ranging from classical "genic selectionism" (Carroll 2005) to a radical redefinition of selection, understood as a filter rather than as a creative mechanism (see Goodwin 1994 and Kauffman 1995).

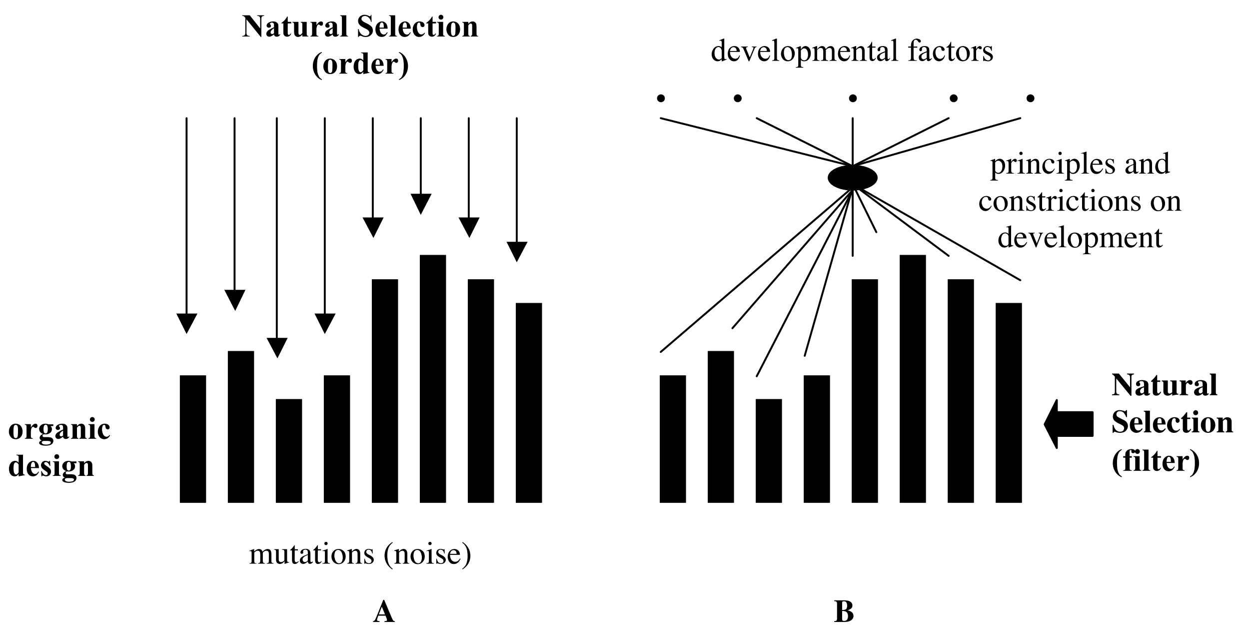


Fig. 3: Two views on Natural Selection.

[**A.** Creative selection. — The unrestricted variation introduced by random point mutations into populations is selected in favor of the variants of those individuals showing more resistance to environmental aggression and higher reproductive rates. **B.** Stabilizing selection. — Organic designs resulting from principles and constraints on development are filtered attending to their resistance to environmental aggression and their reproductive success relative to other designs also present in the population.]

### 2.2. *Pere Alberch's Concept of 'Morphological Evolution': Principles and Applications*

The proposals of Pere Alberch concerning the phylogeny of organic designs (or 'morphological evolution') are entirely sympathetic with the core Evo–Devo contention: The evolution of the formal patterns of organisms is due to perturbations on the parameters underlying their ontogeny. We start this section by presenting his ideas on individual development, strongly connected, in our opinion, with those of the most innovative of contemporary Evo–Devoists.

As a starting point, Alberch rejects the idea that development could be explained as a simple or direct mapping of the genotype onto the phenotype. He defends instead the idea that complex systems of genetic and embryonic factors underlie the growth of organisms and that those factors correlate non-linearly with the resultant morphologies. This amounts to saying that minor perturbations affecting any of the 'morphogenetic parameters' (genetic or non-genetic) can introduce wide-ranging consequences on development and, eventually, readdress the evolutionary course of a whole lineage of organisms. Thus, developmental paths show, according to Alberch, the properties of "complex dynamic systems" (see Kelso 1995, as well as the synthesis in Thelen & Smith 1994: chap. 3), which are summarized in the following points:

(A) A 'dynamic system of development' is composed of a high number of factors ('morphogenetic parameters') of very different characters (genetic, cytological, tissular, and so on);

(B) there is parity in their causal capacities and, thus, none of the factors acts as a 'central control parameter';

(C) they establish complex interactions and, consequently, no factor correlates directly and exclusively with any particular aspect of the resulting phenotype;

(D) the values of parameters can be subject to continuous perturbations without any significant consequence on the phenotype; however, once certain 'critical values' are attained, small perturbations can be enough to trigger a 'qualitatively new' morphology; and

(E) as a consequence of the diversity of causal agents, the complexity of their interactions and the non-linear character of their relation with the outcomes, identical results can be attained by means of perturbations of the values of different parameters of the systems.

In other words, from Alberch's point of view, neither the course nor the outcome of development is 'preformed' or 'programmed' within genotypes, the interactions among the different parameters at work being non-trivial, and both the order of the process and the heftiness of the results being emergent properties of the dynamics of the system. Figure 4 captures all these ideas:

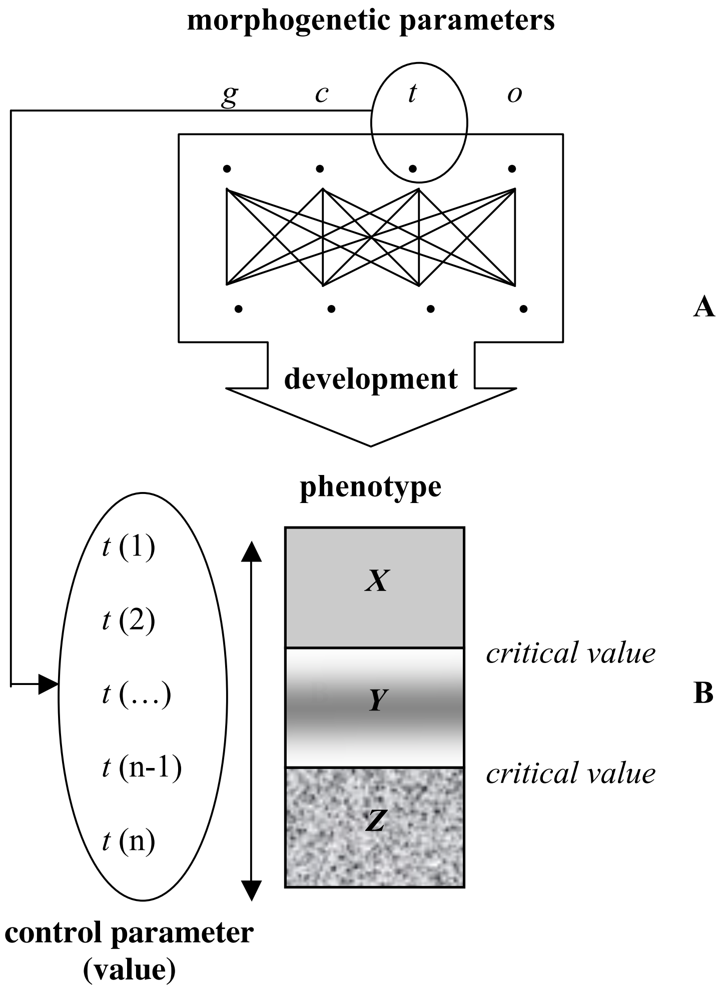


Fig. 4: Dynamism and complexity of organic development

[**A.** A system of development is characterized by the diversity and causal equality of the compounding morphogenetic parameters, the complexity of their interactions and the emergent character of the resulting morphologies. **B.** Once certain critical values have been attained, small perturbations on any of the parameters of the system (parameter t, in the figure) can result in discontinuous variety (i.e. different and qualitatively new designs: X, Y, Z).]

This set of assumptions situates Alberch's ideas not far from current Developmental Systems Theory (DST; see, among other sources, Griffiths & Gray 1994, Griffiths & Knight 1998, Oyama 2000a, Oyama *et al.* 2001, and Griffiths & Gray 2005), except for two non-negligible details. Alberch, in a strictly internalist vein, does not acknowledge the role that DST concedes to environmental factors, whereas the idea that opposing 'organism vs. environment', 'nature vs. culture' or 'internal vs. external' is completely artefactual is at the core of DST thinking (see, particularly, Oyama 2000b). Alberch's framework, on the contrary, takes only morphogenetic parameters of an internal sort into account (rates of kinetic activity of cellular diffusion, viscoelastic properties of the cellular matrix, mitotic rates, and so on; see, for example, Oster *et al.* 1988). Furthermore, Alberch points out in certain passages (e.g., Alberch 1991: 15) that morphogenetic parameters are ultimately genetically determined, which represents a (somewhat) residual genocentric stance. DST, in contrast, abolishes any version of the thesis of the centrality of genes.[7]

Another important component of Alberch's framework is the relevance of the concept of 'heterochrony' as the chief mechanism underlying morphological changes and evolutionary innovations. A 'heterochrony' is basically an alteration of the chronogram and/or the rate of growth for unit of time along the process of development leading to some significant impact on its final product,[8] something to which Alberch was very attentive throughout his career (for a very illustrative piece, see Alberch & Alberch 1981). Section 3 of our article is devoted to presenting an application of Alberch's ideas on heterochronies to the special case (not considered by him) of the evolution of the nervous system and cognition.[9]

Another very distinctive aspect of Alberch's model is the idea that systems of interactions underlying developmental processes are rather stable and that changes in these processes are mostly due to modifications in the values of one or another of the morphogenetic parameters of the system (Alberch 1989: 44). Thus, neither changes in the system of interactions nor in the nature of interactors themselves seems to be needed in order to explain certain major achievements in the course of natural evolution.[10] In the study of complex dynamic systems, the concept of 'control parameter' refers to the systemic component whose perturbations correlate with the emergence of new morphologies (a new pattern in the surface of a chemical solution, a new embryological state, a new form of behavior, and so on; see Thelen & Smith: 63–64). A control parameter is not, however, a central agent in causing phenotypic variation, in that the effects of its perturbations do not immediately reflect on morphological outputs, but on the other morphogenetic parameters instead (Kelso 1995: 7; see also Thelen & Smith: 112).[11]

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7 There exists a connection between these two aspects of Alberch's thinking: The genetic determination of morphogenetic parameters presents itself as the only possible reason for excluding environmental factors from systems of development.

8 'Heterochrony' is a concept that originated with Ernst Haeckel (1834–1919), who used it as the conceptual basis of his Biogenetic Law (i.e. the initial stages of embryonic development represent the adult states of ancestral organisms). For Haeckel, the only possible heterochronic formula was the acceleration of the appearance of early stages so as to give place to the introduction of further stages. The idea that heterochronies can also consist of decelerations is due to Gavin de Beer (1899–1972). See Gould (1977: Part I) for a historical view, and Gould (1977: Part II) and Alberch *et al.* (1979) for systematic and formal descriptions of heterochronies.

9 Alberch introduced some clarifications of the concept of 'heterochrony' in papers such as Alberch (1985) and Alberch & Blanco (1996), his intention being to liberate the concept from its strong dependency on the idea of 'time' and to connect it more directly to the internal dynamics of development (see Etxeberria & Nuño de la Rosa, in press).

10 Alberch (1991: 17–18) speculates with the idea that this 'evolutionary mode' could be an outcome of natural macro-evolution, naturally selected by its advantageous combination of robustness and flexibility: A system of development so defined is very resistant to external aggressions, but it also allows the exploration of new designs, eventually needed in relatively or radically unstable environments.

11 In Thelen & Smith's (1994: 112) words, a control parameter "constrain[s] the interacting elements, but [does] not prescribe the outcome in a privileged way". Or in Kelso's (1995: 7) words: "[The] control parameter does not prescribe or contain the code for the emerging

Within this model, development (even the development of novel forms) is always a function of the system as a whole. The idea of 'control parameter' basi-cally introduces the possibility of signaling a single parameter of the system as the starting point of the chain reaction leading to new morphologies.

For our own purposes, however, the most relevant aspect of Alberch's proposals is the contention that developmental systems foreshadow the scope of their attainable phenotypes, as well as the trajectories leading from a certain phenotypic state to another. It is the concept of 'parametric space' that in Alberch's framework is in charge of theoretically representing the finite and discrete set of the possible outcomes of any developmental system (Alberch 1989, 1991).[12] The main properties of parametric spaces are summed up in the following paragraphs (see Figure 5 below as a point of reference):

(A) A parametric space is a finite set of discrete phenotypes. The discontinuous character of phenotypic variation is captured in Figure 5 by the separate spaces named with capital letters;

(B) each phenotype has a characteristic probability of coming into being, represented in Figure 5 by the extension that it occupies ([D] is thus the most probable phenotype, while [B] is the most improbable one);

(C) moreover, each phenotype is also characterized by the relative probability of transforming itself into one or another of the neighboring phenotypes. In Figure 5 this aspect is represented by the extension of the line separating different phenotypes ([A] has a strong probability of turning into [D], a low probability of turning into [B], and no probability at all of turning into any of the remaining phenotypes).

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pattern. It simply leads the system through the variety of possible patterns or states".

12 The notion 'parameter space' is related to a certain point to Waddington's (1957) concept of 'epigenetic landscape'. This concept refers to the strong 'canalization' of certain developmental paths (see fn. 1 of this article), an effect that makes them highly resistant to external perturbations. These pathways are thus firmly established and only very radical perturbations can serve to redirect development to another less canalized but also genetically available path.

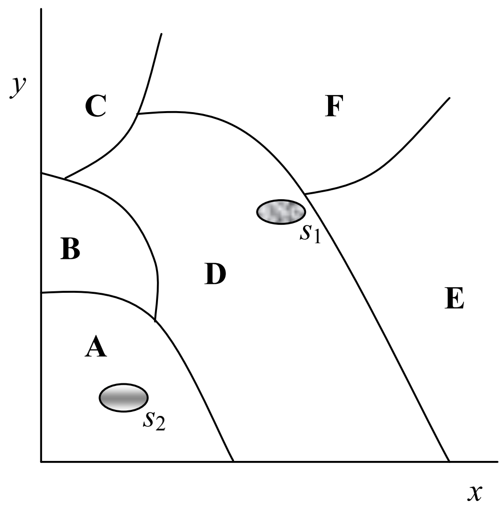


Figure 5: Parametric space

[The figure represents a parametric space ideally defined by means of two morphogenetic parameters (*x* and *y*). The space delimits a finite number of discontinuous phenotypes (**A, B, …, F**). Perturbations of the values of the parameters can transform a phenotype into another. Each phenotype has a characteristic probability of coming into existence (in the figure, the space of the relevant phenotype) and a certain propensity of transforming itself into another phenotype (in the figure, the extension of the lines limiting phenotypes). (Based on Alberch 1989: 51 and Alberch 1991: 16.)]

Moreover, species are represented in Figure 5 by means of the oval items ('$s_1$' and '$s_2$'). From a populational point of view, the model incorporates the following contentions:

(A) Every species falls within one or another phenotype ('$s_1$' belongs to phenotype [D], whereas '$s_2$' fits in phenotype [A]);

(B) the morphological stability of a species is a function of both (i) the probability of its phenotype ('$s_1$' is, in principle, a more stable population than '$s_2$') and (ii) its proximity to a point of bifurcation to other phenotypes ('$s_1$' is thus a rather unstable population within its phenotype, given its vicinity with the bifurcation leading to [E] and [F]);

(C) the proximity to a point of bifurcation as well as the relative propensity of its own phenotype to transform into one or another phenotype puts a certain population at the edge of undergoing a radical morphological reorganization ('$s_1$', for instance, has a high propensity of acquiring the properties of phenotype [E]).

'Bifurcation', a point within a parametric space in which a minimal perturbation is capable of bringing about qualitatively new morphologies, is thus another key concept in Alberch's framework (Oster & Alberch 1982). In Figure 5, for example, species $s_1$ can easily shift to phenotype [E], attending to its proximity to the bifurcation between [F] and [E] from its original phenotype [D], as well as to the higher propensity of phenotype [D] for transforming into [E]. Using the jargon of dynamic systems theory, we can say that population $s_2$ occupies a 'well of attraction' in the situation depicted in Figure 5, whose depth guarantees a high degree of stability to its phenotype (of type [A]). On the contrary, population $s_1$ is at the edge of its well of attraction (i.e. of a 'phase shift') due to its continuous exposure to a certain kind of perturbing factor, a situation that makes it highly susceptible to entering into a different well of attraction. Figure 6 is thus an alternative way of symbolizing the situation previously presented in Figure 5. Figure 7 summarizes a particular application of this model to the study of an entire family of organic structures.

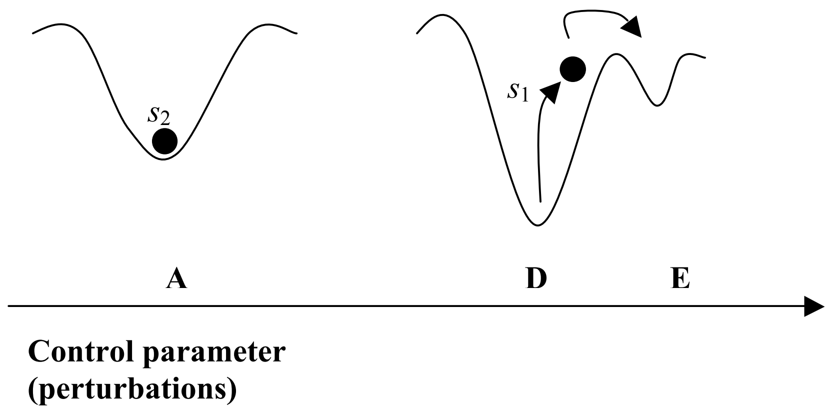


Figure 6: Wells of attraction and phase shifts

[Some morphologies are located in a region of the parametric space that guarantees them a high degree of stability in spite of the perturbations of the morphogenetic parameters. They occupy a 'well of attraction', as it is the case of population $\mathbf{s_2}$ within phenotype **A**. On the contrary, some other morphologies are in positions that render them highly unstable and exposed to radical changes with a minor perturbation of a single parameter. This is the case of population $\mathbf{s_1}$, at the edge of undergoing a 'phase shift' within phenotype **D** to phenotype **E** (based on Thelen & Smith 1994: 64).]

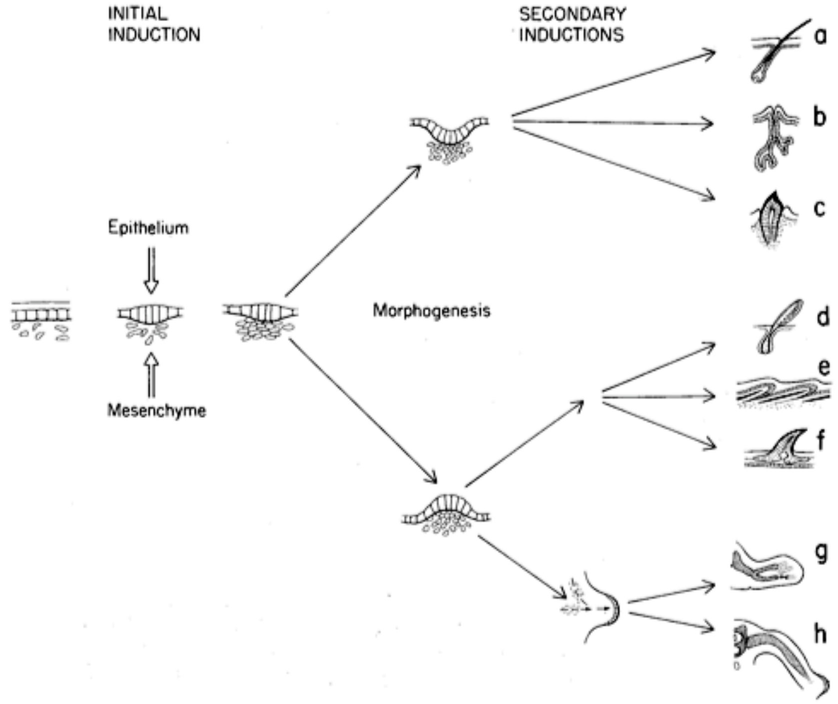


Figure 7: Morphogenesis of dermal organs

[The skin is made of structures (hair, salivary glands, teeth, feathers, scales, limbs and carapaces, among others) that are discontinuous from species to species, in the sense that no structure can be said to be a transition form between any other two structures. However, they all originate in equivalent inductive processes having to do with the thickness of the epithelium and the concentration of the mesenchymal tissue. Depending on the elasticity and the strength obtained in each case, the epithelium can (i) invaginate, giving place to hair, glands and teeth, or (ii) evaginate, giving place to feathers, scales or carapaces. Thus, each structure is only accessible through perturbations acting upon a particular phenotypic stage in the course of development (see Odell *et al.* 1981 and Oster & Alberch 1982; the figure is from Alberch 1989: 47).]

From an evolutionary point of view, an important corollary of this model is that the geometry of parametric spaces, representing forces of an internal sort acting upon individual development, works as a very strong constraining force, capable of counteracting that of natural selection. As explicitly stated by Alberch, although this stance does not discredit the role of natural selection as a filter in the evolution of organic designs, it nevertheless strongly decreases the creative character that this mechanism has to the adherents of MES-oriented frameworks (Alberch 1980: 664, Oster & Alberch 1982: 455, Alberch 1989, 46–48, Alberch 1991: 16; see also Goodwin 1994: 143 and Wagensberg 2004: 125).

In this sense, Alberch's model connects to an old pre-Darwinian tradition known as Transcendental (or Rational) Morphology, with such illustrious repesentative proponents as Johann Wolfgang von Goethe (1749–1832), Étienne Geoffroy Saint–Hilaire (1772–1844), Richard Owen (1802–1892), and Isidore Geoffroy Saint–Hilaire (1805–1861); see Russell (1916). In spite of the unequivocal empiricist aims of this historical trend, interested in the formal study of organisms, its influence on the biology of the twentieth century was truncated because of the accusations of Platonism by MES-inspired historiographers (see Amundson's 2007: Part I analysis of this episode of the history of modern biology). It is our opinion that Alberch's ideas serve as a clear demonstration that the belief in organic types fits perfectly well with a materialistic world view, as far as the different types result from natural constrictions and cannot be said to be ideal or abstract ideas that are alien to a physical order of things (Amundson 2001, Love 2003).

In connection with this, Alberch emphasized the evolutionary interest of the study of developmental monsters (or teratologies), recovering a tradition also embraced by Transcendental Morphologists (see Figure 8 below); see especially Alberch (1991).[13] In Alberch's opinion, teratologies are demonstrative of the strength of development against the pressures of natural selection, in that they are, by definition, maladaptive. However, a teratology can be the basis of organic novelties with a potentially evolutionary import.[14] Furthermore, the range of possible teratologies seems to be very strictly constrained (for example, duplication, but not triplication, of structures is common in abnormal development). This fact points to the existence of strong constraining forces on development not directly coming from natural selection (see Figure 9 below).

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[13] See Geoffroy Saint–Hilaire (1822) and Geoffroy Saint–Hilaire (1832-1837).

[14] Alberch (1989: 28) points out that the evolutionary potential of teratologies can be rather limited. Anyhow, he observes that it would be an error to discard them as a possible source of raw material for the evolutionary process. In his own words (Alberch 1980: 656):

> The argument that most developmental anomalies are harmful, commonly used to 'discredit' the relevance of these 'mutations' to evolution, is fallacious. Most genetic mutations were once thought to be deleterious (presently it is recognized the most of them appear to be selectively neutral, but that does not affect my argument), however nobody doubts that they provide the raw material for evolutionary change.

Alberch's position is thus not far from that of Richard Goldschmidt (1933, 1940: 390–393): "Mutants producing monstrosities may have played a considerable role in macroevolution. A monstrosity appearing in a single genetic step might permit the occupation of a new environmental niche and thus produce a new type in one step" (Goldschmidt 1940: 390).

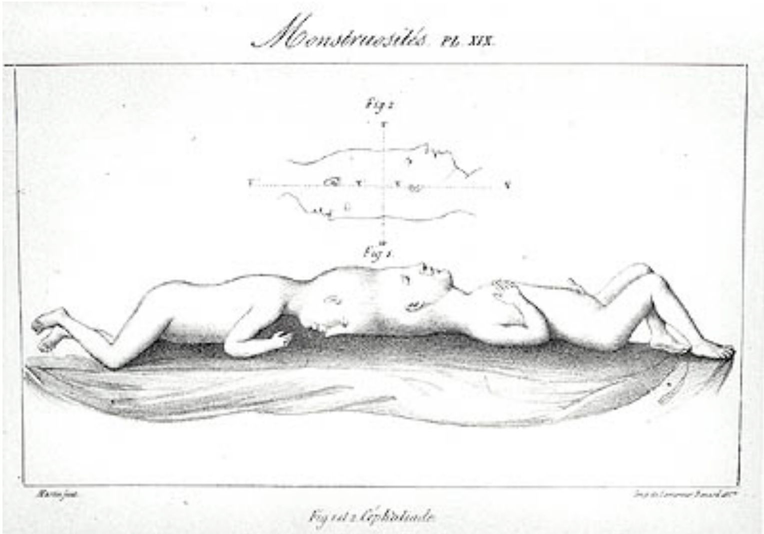


Figure 8: The logic of monsters (I)

[“After having studied anomalies in their special conditions and established the laws and general relations underlying all particular facts, I will demonstrate the way in which these laws and relations are but corollaries of even more general laws of organization; […] the way in which […] many principles established on very weak evidence find in anomalies a complete demonstration. And the way in which teratology, among its many concerns, covers every single condition of the organization of life” (Geoffroy Saint–Hilaire 1832–1837, vol. I: xi; our translation — SB & GL).]

Figure 9: The logic of monsters (II)

[“Monsters are a good system to study the internal properties of generative rules. They represent forms which lack adaptative function while preserving structural order. An analysis of monsters is a study of pure form” (Alberch 1989: 23). “These major deviations from normal development result in forms that are often lethal, and always significantly less well adapted than their progenitors. Therefore, one expects monsters to be consistently eliminated by selection. This is a useful property because if, in spite of very strong negative selection, teratologies are generated in a discrete and recurrent manner, this order has to be a reflection of the internal properties of the developmental system” (Alberch 1989: 28).]

Anyway, Alberch did not contend that natural selection was to be deprived of any creative role in evolution. Actually, he noted that natural selection biases evolution just by filtering out those designs unable to withstand populational and environmental pressures, thus having the effect of directing the process towards the morphologies accessible from the selected designs. Therefore, natural selection is continuously determining the probability of the presence of certain organic designs at future stages of evolution, an unequivocal creative intervention on the evolutionary process (Alberch 1989: 46 and Maynard Smith *et al.* 1985: 270).

## 3. The Evolution of Grammar through the Development of the Mind

"If the ideas that make up Evo–Devo have been so productive in opening up new lines of investigation into morphological evolution, they may be equally productive for psychological evolution". These words by Paul Griffiths (2007: 196) are particularly well suited to mark the transition of our work, so far devoted to an exposition of the theoretical underpinnings of Evolutionary Developmental Biology in the field in which it has become a mature scientific discipline — that is, in the field of the evolution of the formal patterns followed by organisms, to the study of a specific aspect of the evolution of human cognition.

Griffiths reminds us that, inasmuch as cognition is just another dimension of the organic world, there is no principled reason for not extending the core theses of Evo–Devo to this domain. Indeed, to the extent that we eschew any hint of 'mind/body' dualism, the fact is that there exists a perfectly natural path to extend and apply these ideas to the field of the evolution of mind. Taking then what in any case appears to be the most logical assumption, if what we call 'mind' — or 'cognition', as, for the purposes of this article both terms can be taken as synonymous — is nothing else than what "the brain does", to use a well-known expression by Searle (1985), it is obvious that the study of the morphological evolution of the brain is a first step towards the evolutionary study of mental functions. It is also clear therefore that the evolutionary study of the brain qua organic structure and, specifically, the evolutionary study of its formal diversity among the species endowed with complex nervous systems presents itself as a natural field of application for Evo–Devo.

It goes without saying, however, that the evolution of brain morphology is just a part of a broader Evo–Devo agenda, aiming at the investigation of the evolution of mental functions. In this sense, it may be useful to distinguish three different levels of analysis, all involved in the study of such functions, and briefly to consider in these preliminary remarks their positions within the kind of evolutionary explanations specifically devoted to the phenomena of development. The three levels are: (i) brain anatomy, (ii) brain function, and (iii) observable behavior.

As for the first level (brain anatomy), we already pointed out that, for Evo–Devo, it just is a specific area of application within its broader research program centered on the evolution of formal organic patterns. As a matter of fact, there already exists a number of very interesting findings, such as those reported in Parker *et al.* (2000), Falk & Gibson (2001), and Minugh–Purvis & McNamara (2001), for example, just to restrict ourselves to the evolution of the primate brain.

As for the second level (brain function), this is where most problems traditionally associated with the study of mind are concentrated. In this work we will assume, as already pointed out, that when talking about the mind we are talking about the functions related to brain activity. Therefore, this level adds up a new level of analysis whose phenomenology goes way beyond the mere facts of brain morphology. Nevertheless, the fact that, at this level, most theoretical approaches tackle the study of brain function abstracting away from its physical realization does not mean that it is really abstract and with irreducible properties to its material base.[15] Thus, we share point of view of Noam Chomsky, who in his works has argued that framing an explanation on a strictly physicalist language or using an abstract vocabulary to talk about these matters is just a mere question of perspective, and that decisions must be made simply on the basis of practical issues, such as the accessibility to observation of the considered domain or the significance of directly observable data (see, for example, Chomsky 1980). Also, from the Chomskyan point of view, similar considerations dictate that in the study of higher cognitive functions in humans there often prevails a computational and more abstract approach. Whatever the most convenient or just possible approach that one may take, the truth is that the exploration of the putative modifications in brain function regarding such aspects as the timing of the onset and stabilization of brain development, its maturation rhythms, the alteration of intermediate states or of the terminal state, and so on appear to us as topics which fit perfectly well within the Evo–Devo agenda.

Finally, we come to the study of observable behavior, which for a long time was the alibi of twentieth-century behaviorist psychology to attain the kind of scientific respectability imposed by the dominant positivist ideology in the philosophy of science. Curiously enough, Amundson (2006) suggests that transition of theoretical psychology towards a more cognitivist approach is, historically, comparable to the deliverance of Evo–Devo from the narrow-minded perspective imposed by the MES. In both cases, so Amundson argues, we observe a transition from an approach focused on environmental conditioning factors to a perspective where the search for internal constraints prevails. As Amundson himself suggests, such a situation may well be interpreted in the sense that both Evo–Devo and cognitive psychology are the products of the same *zeitgeist*, which should favor the convergence of both scientific approaches, along the lines of what we sketch in the previous paragraph.

All these considerations notwithstanding, we believe that the study of behavior must not be eschewed in order to favor only the study of internalist issues. As a matter of fact, the kinds of explanations based on the interaction among multiple domains which are generally favored within Evo–Devo make us believe (i) that in the application of this discipline to the realm of cognition none of the three aforementioned levels must be privileged over the others, and (ii)

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[15] But see the introduction to Fodor (1975) for an alternative point of view, and Churchland (1981) for the defense of a radically different stance from the one adopted in this paper.

that such an application may foster the formulation of complex interactions in different directions among these levels, such that these very same interactions may be seen as the *locus* of alterations in development with a potentially evolutionary significance (Figure 10). Indeed, the fact that specific environmentally induced alterations in the behavior of an organism during its development may be the basis of further modifications of the anatomic, physiologic and, eventually, genetic determining factors capable of inducing a new behavioral pattern is a non-problematic idea in the context of Evo–Devo and one that has contributed to vindicate certain models of change — such as 'organic selection' or 'genetic assimilation', which apparently stand in direct contradiction with the dominant genocentric logic of the MES, by letting in the Lamarckian ghost of the inheritance of acquired characters.[16]

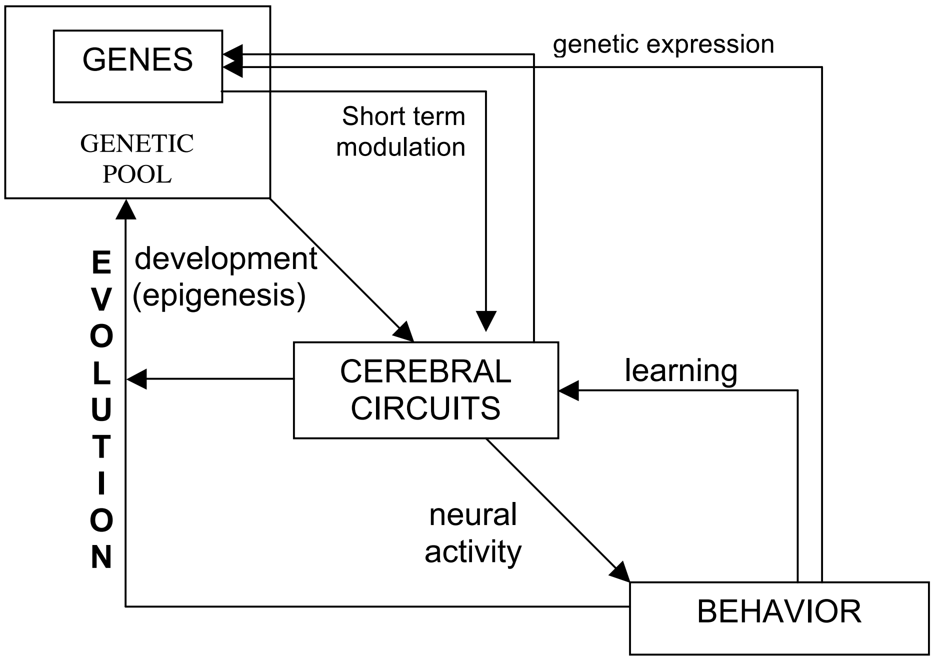


Figure 10: Evolution and behavior

[Interactive model with multiple levels of development and activity in higher organisms (based on Plotkin & Odling–Smee 1981; see also Johnston & Edwards 2002 for a still more complex model in terms of levels and interaction loops, and in terms of the specified morphogenetic parameters). In this model, the intergenerational recurrence of alterations within a specific behavioral pattern may have evolutionary repercussions, either through a novel redistribution of the dominant alleles in the genetic pool of the population ('genetic assimilation') or through the selection of new mutations that occurred in it ('organic selection').]

16 For a synthesis of these ideas and models, see Gottlieb (2003: 14–23). The original references for each of them are Baldwin (1896), Morgan (1896), Osborn (1897), and Waddington (1953), respectively. Longa (2006) is an interesting clarification of the ideas of these authors, often wrongly identified in certain recent applications of the so called 'Baldwin Effect' to the evolution of language. Jablonka & Lamb (1995) is a recent vindication of the 'Lamarckian dimension' in evolutionary biology.

### *3.1. The Evolution of the Brain through Development*

The application of the Evo–Devo assumptions to the particular case of a species' psychological endowment in essence implies trying to find out what alterations in the development of the mind/brain of the organism might be capable of introducing into the population to which the organisms in question belong a heritable variety, eventually capable of making them fitter with respect to the rest of the population. In the light of the ideas exposed in Section 2 of this article, if we are to find any illuminating answers, this task involves taking into account the following four key points:

(A) Identify the morphogenetic parameters which make up the developmental system of a particular feature of the mind/brain;

(B) identify the control parameter or parameters whose perturbation acts as a trigger for change;

(C) specify the constraints imposed by the developmental system itself, defining parametrical spaces of possibilities; and

(D) elucidate the populational and environmental conditions capable of filtering the distribution of phenotypes within the population and favoring the resulting solution of the process of change in question.

That said, we will devote the following paragraphs to briefly discussing a number of important contributions connected to the issue of the study of the evolution of human cognition in any of its dimensions (i.e. structure, function, and behavior). As we already hinted in section 2, all share the assumption that one form or another of 'heterochrony' is the basic mechanism able, just as in the case of morphology,[17] to explain the evolution of the human nervous system and cognition, which would thus be a product of some alteration in timing and/or intensity of development of closely related species (Table 1).

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[17] As stated by Gould (1977: 4):

> Evolution occurs when ontogeny is altered in one of two ways: when new characters are introduced at any stage of development with varying effects upon subsequent stages, or when characters already present undergo changes in developmental timing. Together, these two processes exhaust the formal content of phyletic change; the second process is heterochrony.

*Heterochrony*. Evolution through changes in developmental timing and intensity. Types:

1. Paedomorphosis (underdevelopment or terminal truncation). Subtypes:
   - 1.a. Reduced rate of development (neoteny);
   - 1.b. Earlier offset (progenesis); and
   - 1.c. Delayed onset (postdisplacement).
2. Peramorphosis (overdevelopment or terminal extension). Subtypes:
   - 2.a. Increased rate of development (acceleration);
   - 2.b. Delayed offset (hypermorphosis); and
   - 2.c. Earlier onset (predisplacement).

Table 1: Heterochrony: Definition and types

[Based on McKinney (2000) and Parker & McKinney (1999); other sources: Gould (1977), McKinney & McNamara (1991), and Alba (2002).]

In the study of the evolution of mind, brain structure as the result of transformations that have occurred in the ontogeny of organisms is the most studied area. This is not at all surprising, as it is the domain which stands closest to the typical body of applications of Evo–Devo. As a consequence of that, one may take as an established fact, for example, that both larger brain size and connectivity rate among its components (especially, but not exclusively, at the cellular level) may result from alterations at very early stages of embryonic development. In this connection, Kaskan & Finlay (2001) point out that an increased rate of production of precursor cells or an extension of cytogenesis during embryonic development may result in significant differences in brain size. These authors, in fact, define 'cytogenesis' as the period spanning from a point where production of precursor cells of some structure starts to the point where a maximum of cell division obtains and the resulting cell does not divide but 'migrates' to the forming structure (Kaskan & Finlay 2001: 17). They note moreover that in the development of different brain structures from the neural tube of mammals one observes clear displacements once the highest point of such asymmetric terminal divisions is reached. Finally, they also contend that in each case the resulting neural population grows exponentially with respect to the displacement of this peak (Figure 11).

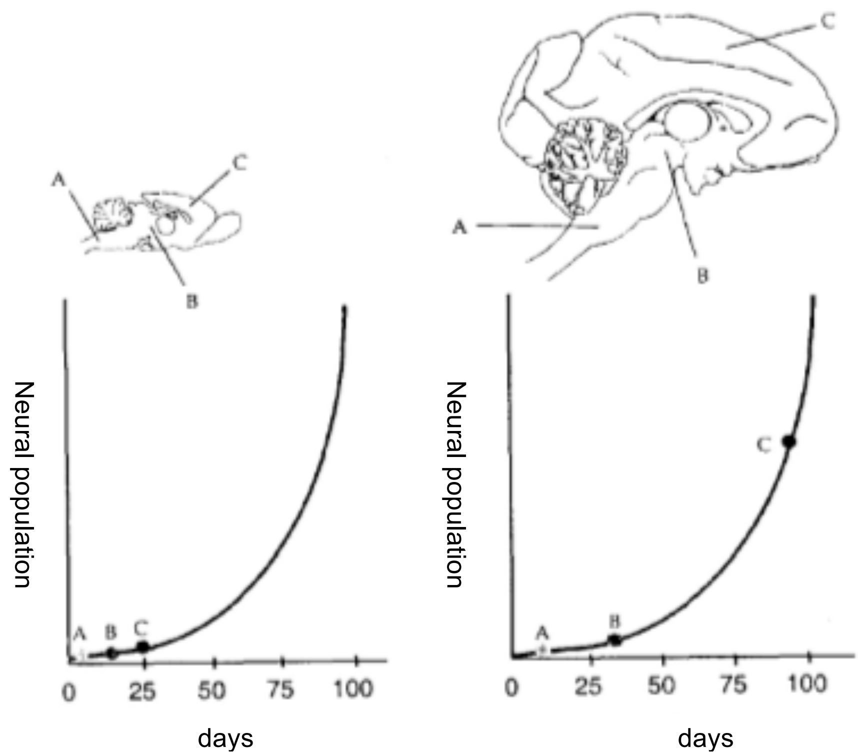


Figure 11: Extension of cytogenesis and exponential growth of brain structures

[The temporal extension of this 'summit' of asymmetric cellular divisions causes an exponential increment in the population of resulting neurons. The charts above show how such displacement is generalized in the cytogenesis of different brain structures in primates (right) compared to the homologous structures in rodents (left). They show moreover that a greater displacement in the formation of the cortex (**C**) results in an increased neural population for this structure, much larger than that for other structures, such as the spinal chord (**A**) or the basal anterior brain (**B**); this increment has an exponential character. (Taken from Kaskan & Finlay 2001: 20.)]

Rakic & Kornack (2000), for their part, offer some interesting quantifications. They point out that the phase of asymmetric cell division yielding to neural cells (that is, the phase to which peak Kaskan & Finlay refer to) starts in monkeys some four weeks later than in mice, which entails an extension of the period of symmetric cell division, where the majority of neural precursors are produced. According to the data presented by these authors, as a result of this displacement on the onset of a characteristic phase of embryonic development, the population of neurons in monkeys doubles that of mice. Moreover, as Rakic & Kornack (2001: 45–46) point out, in the case of humans the onset of asymmetric cell division is displaced only a few days later than that of monkeys, but, given the exponential effect of such displacement on the production of neural precursors, the population of neurons in humans is estimated to be between some eight or sixteen times larger than that of monkeys. In the light of these data, the authors conclude that the mutation of some regulatory gene (or a collection thereof) may have been responsible for the reorganization of the neo-cortex and of the cognitive and behavioral novelties associated with it (Rakic & Kornack

2001: 46).[18]

It must be taken into account, however, that this is not the only known difference in the developmental pattern of the human brain as compared to that of monkeys. Parker & McKinney (1999) and McKinney (2000), for example, contend that (i) fetal growth of the human brain is 25 days longer than that of monkeys, (ii) myelinization of the neo-cortex (and, especially that of the frontal cortex) is extended in humans until the age of 12, whereas in rhesus monkeys it lasts only 3.5 years, or (iii) dendritic growth is extended in humans until the age of 20, well beyond that of any other of its close kindred species. All these cases have to do with factors affecting the neural interconnectivity rate and their combined effect must no doubt have some far reaching consequences, as explicitly argued for by Parker & McKinney (1999) in their comprehensive comparative study of cognitive development in monkeys, apes and humans.

It is also relevant to point out, however, that the alterations of developmental phases with direct repercussions on the proliferation of neural precursors or on the interconnection of the resulting neural populations cannot bring about a totally unconstrained type of growth or of brain reorganization. Metabolic and energetic limitations aside — which are in any case an important factor in limiting brain capacity — Hofman (2001) reasons that certain aspects of the *Bauplan* followed by the primate brain impose limiting constraints on its evolutionary potential. He points out, for example, that the exponential character of the increment of the cortical mass with respect to cerebral mass as a whole is not observed in other brain structures (all playing a relevant role in regulating cortical connectivity, such as the cerebellum, the basal ganglia, etc.), where growth follows a linear pattern. As a consequence of that, the more the brain grows, the more disproportionate is the relation between cortical mass and these structures, which will eventually be inadequate for processing the same kind of complex information the cortex would be able to process (Hofman 2001: 115–119). Besides, Hofman contends that there are limitations on axon length which may cancel out the effects of an increased cortical mass, for the simple and practical reason that it could not be supplied with the appropriate wiring to ensure the connection between areas distant from each other in the cortex. Hofman points out that this may be the underlying cause for the compartmentalization of the cortex into specialized areas or modules within which highly interconnected neurons are concentrated.[19] A disproportionate growth of the brain would, in any case, bring the issue back to the level of inter-modular wiring which is necessary to support the characteristic flexibility of human-like cognition (Hofman 2001: 119–124).

Findings such as these provide additional support to the idea that alterations in brain development may be at the basis of the evolution of human cognition, and that factors such as the significance of genetic regulation as a

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[18] In this connection, we find promising some recent findings pointing at genes which regulate certain phases of brain growth (see Dorus *et al.* 2004). In connection with some of them, like *Microcephalin*, relatively recent mutations specific to modern humans have also been identified (see Evans *et al.* 2004 and Evans *et al.* 2005); for a comprehensive overview of the genes implied in brain development and growth, see Benítez Burraco (2009: chap. 3).

[19] See Griffiths (2007) for a defense of the concept of 'modularity' in the application of Evo–Devo to the study of mind.

particularly relevant control parameter, or the role of allometry as a constraint on possible phenotypic spaces constitute a serious hypothesis and are of major importance in this research. Unfortunately, they are rather uninformative at the time of concocting an explanation at the level of the resulting mental functions and their reflection on specific behavioral patterns. This point is not only relevant for the definition of parametric spaces of brain functionality, but also because, as we already pointed out, the identification of some specific population within one space or another may in turn have repercussions on the evolution of the morphogenetic parameters delimiting these spaces.

In this connection, and deserving of special attention, there already exists an important line of research focusing on the study of mental function as a result of alterations in developmental systems which adopts a comparative perspective similar to that of the works cited above.[20] Thus, for example, Langer (2000) compares physical and logico-mathematical intelligence in humans, chimpanzees and monkeys, whence he derives a series of conclusions about the onset and culmination of the development of such cognitive abilities in these species and, additionally, about the relative intensity of its progress in each case. The most fundamental conclusion derived from this comparison is that, evolutionarily speaking, there appears to have occurred an early onset and an acceleration in the development of these cognitive dimensions (particularly pronounced in the case of logico-mathematical intelligence), as well as a synchronization of the processes involved, favoring a mutual inter-penetrability with clear repercussions on how the surrounding world is conceived and explained. Langer specifically points out that the emergence of physical intelligence takes place early in all cases, but logico-mathematical intelligence in monkeys appears only after maturation of the former has been completed; this happens only a bit earlier in chimpanzees, while in humans both processes are entirely synchronized. Langer moreover contends that certain particularly complex aspects of human cognition, such as the ability to perform specific mental operations in an integrated and simultaneous way (e.g., deal with two sets of objects, compare them and redistribute their elements to make them identical) are an effect of this pattern of premature, accelerated and synchronized development of the aforementioned abilities. Lastly, Langer observes that the development of these abilities in humans goes well beyond that of the species taken as a point of comparison, which takes him to speak about a model of cognitive evolution through "overdevelopment" or "terminal extension" (Langer 2000: 229). As he himself emphasizes, this conclusion is particularly interesting, since it is consistent with the hypothesis of a parallel process of overdevelopment at the level of brain structure, with an equal involvement of different aspects of brain anatomy (extended growth of glial cells, axon myelinization, synaptogenesis, and dendritic growth in the cortex) (Langer 2000: 227). As a whole, this is clearly an important step in the establishment of bridges between the neural and the mental levels.[21]

The next section of this article may be seen as an attempt to put forth some

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[20] See also the work by Parker & McKinney (1999) cited above, as a key reference in this direction.

[21] See Gibson (1990, 2004) as additional representative references in this direction.

concrete proposals within this same line of thought regarding human cognition as an effect of the evolutionary history of its developmental pattern. We try to bridge the gap as much as possible between the physical and functional levels of analysis. We will be concerned with a particular aspect of the human mind, namely the computational system associated with the FL. We will more concretely explore the kinds of morphogenetic perturbations that may be the cause of its origin; also, and from a representational and abstract perspective, we will suggest a localization for such a system within a parametric space of possibilities, and the network of bifurcations leading to the position that it occupies today. Finally, we will also suggest some ideas about the environmental and populational conditions that may have made this system possible, while filtering out other alternatives within the same parametric space.

### *3.2. A Model of the Evolution of the Nervous System and Cognition: Parametric Space and Computational Phenotypes*

As was mentioned at the end of the preceding section, there are not very many proposals where an explicit connection is established between the development of new neural structures and novel cognitive abilities. In this section, we will try to tackle this problem by sketching a proposal built on the principles of Evo–Devo, where we apply Alberch's notion of parametric space to different possible phenotypes of the nervous system and where we associate each possible phenotype with specific computational properties.

To the extent that "development of form cannot be straightforwardly related to genetic change, because the relation of function to new morphologic structures is not simple or direct" (Edelman 1988: 156), any proposal along these lines should, on the one hand, seek to determine what morphogenetic parameters make up the developmental system and, on the other hand, define the space of possible phenotypes on the basis of each and every parameter of the system. For expository reasons we will, however, limit ourselves to representing parametric spaces on a bi-dimensional plane, adopting Alberch's convention of considering only two 'abstract' parameters — $x$ and $y$ — from which one should be able to define the phenotypes in question. Additionally, and despite the considerable progress in the study of the different factors involved in embryonic development in general and in the development of the nervous system in particular, it would at present be practically impossible for us to precisely pinpoint all these factors: The fact is that many important aspects of the process still remain a mystery. We can nevertheless assert that these factors are not only and exclusively of a genetic nature and, as is made explicitly clear in the proposed models (see, for example, Edelman 1988, for development in general, and Edelman 1987 and Ebesson 1980 for the development of the nervous system), such factors cover a wide spectrum of elements. For example, in Edelman's topobiologic model, the main developmental processes are cell division, cell movement, and cell death, the driving forces of the whole process, with cell adhesion and differentiation acting as regulatory processes. It is important to point out, however, that these processes, which are responsible for building and giving form to an organism, are the result of a complex balance between genetics and epigenetics. Indeed, as emphasized by Edelman (1987: chap. 4), the development of the brain is one of those cases where special attention must be paid to epigenetic mechanisms, since, although the anatomical structures in specific brain areas in individuals belonging to a particular species are very similar and therefore their development must obey some specific genetic constraints, it is also true that an extremely high degree of variation is observed at the levels of neuronal morphology and neural patterning, especially at the level of axonal and dendritic branching. It goes without saying that such a degree of variation can only be the result of epigenetic factors acting during development on what Edelman calls the "primary repertoire" with a genetic basis (but also variable). As the process moves forward, new elements of variation are introduced, particularly at the synaptic level, in the form of changes in the biochemical structure and the appearance of an increasing number of different neurotransmitters. It is obvious, then, that a process of this kind possesses enormous potential for the introduction of morphological novelties, and even more so if we factor in the possibility that it be affected by some kind of heterochrony altering developmental timing and rate of growth.[22]

A developmental system of this kind, with a high creative potential, is not, however, totally unconstrained. Remember, in this respect, Hofman's (2001) considerations about the structural and connectivity restrictions that constrain the space of possible phenotypes for the nervous system of an organism, and which are, moreover, the kinds of constraints over form alluded to by several scholars since the beginning of the nineteenth century as the basic ingredient to explain the phenomenon of form.[23] In accordance with this stance, we understand that it is perfectly licit to assume that the developmental system of the nervous system defines a finite set of possible phenotypes, in such a way that a specific organism might have access to any of them, should the necessary perturbations to remodel it or relocate it in a new position occur. In addition, and coming back to our discussion concerning the different levels of analysis in the study of cognition, we propose to extend such distinctions to the realm of parametric spaces, such that, in parallel to the morphological parametric space, another parametric space would exist with what we may call a collection of 'cognitive phenotypes', where to every phenotype in the space of forms there corresponds a unique cognitive phenotype.[24]

Even though an exhaustive and detailed characterization of all the defining properties of a specific morphological phenotype is impossible, at least we have

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22 See Edelman (1987: chap. 6) for a concrete proposal along these lines.

23 The most significant — and radical — representative of this line of thought is, without doubt, D'Arcy Thompson (1860–1948), who presented his ideas in his monumental *On Growth and Form* (D'Arcy Thompson 1917). It is possible, however, to identify similar ideas in the thought of earlier authors, such as Richard Owen (see Owen 1848: 102–106) and, in particular, William Bateson (1861–1926) (see Bateson 1894). Webster & Goodwin (1996) is perhaps the most comprehensive an detailed exposition of the theoretical and empirical underpinnings of contemporary 'Generative Biology'; see also Goodwin (1994).

24 Note that it is not strictly necessary that the number of morphological phenotypes be equal to the number of cognitive phenotypes. In fact, we deem it perfectly reasonable to assume that this is not the case and that a single cognitive phenotype may be associated with more than one morphological phenotype or, in other words, that different morphologies may be susceptible of an identical abstract characterization.

at our disposal a powerful theoretical tool that makes it possible to identify its main features at the computational level, and to elaborate concrete proposals about what structures and neural organizations could be associated with these properties. In concrete terms, we will assume that the parametric space of cognitive phenotypes contains a minimum of four phenotypes, in direct correspondence with the four levels of computational complexity of the Chomsky Hierarchy (Figure 12). We speak of a minimum of four phenotypes, because we will for the moment stick to the original version of the Hierarchy (Chomsky 1959). We are perfectly aware, however, that at a later date new levels were added to it. For example, Chomsky himself argued soon after for the necessity of distinguishing between strict context-sensitive systems and systems capable of generating any recursive system (Chomsky 1963); Aho (1968) described indexed systems within the complexity space originally reserved to type 1 systems (see also Hopcroft & Ullman 1979: chap. 14), whereas Aravind Joshi and collaborators (see Joshi 1985, Joshi *et al.* 1991, and Vijay–Shanker & Weir 1994) added new compartments to this space with the extended context-free systems or, alternatively, mild context-sensitive systems. What follows, however, does not particularly hinge on the exact number of phenotypes (although we will come back to these new developments) and, for ease of exposition, we will stick to a parametric space with four phenotypes, without forgetting that human language would be somewhere within the 'lower' area of type 1 systems.

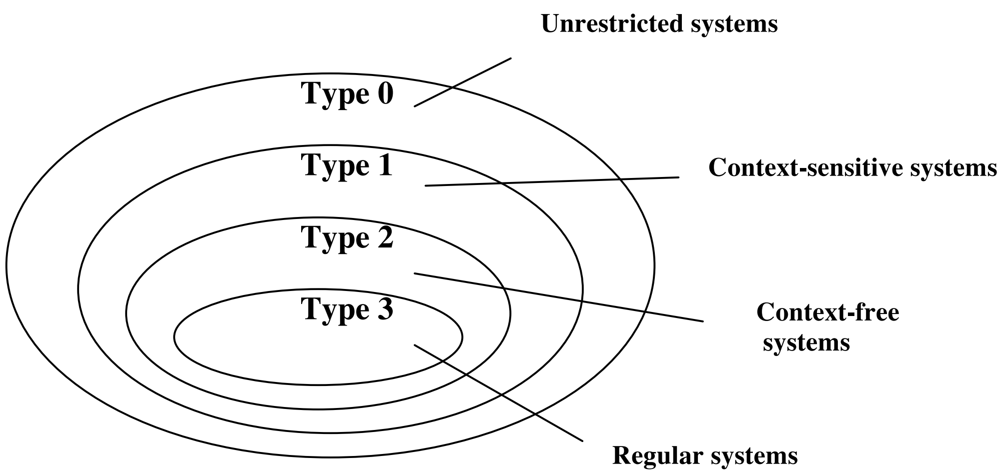


Figure 12: The Chomsky Hierarchy (Chomsky 1959)

[The Chomsky Hierarchy defines a scale of systems with an increasing generative power, and capable of generating different types of recursive sets whose elements show increasing levels of complexity (type 3 to type 1), and, eventually, any type of recursively enumerable set (type 0).]

In Figure 13 we sketch our proposed parametric space of cognitive — or, better perhaps, computational — phenotypes. In the figure, we label the phenotypes as $FC_3$, $FC_2$, $FC_1$, and $FC_0$ in correspondence with the four levels of the

Chomsky Hierarchy; with arrows we indicate the possible transitions between one space and another.

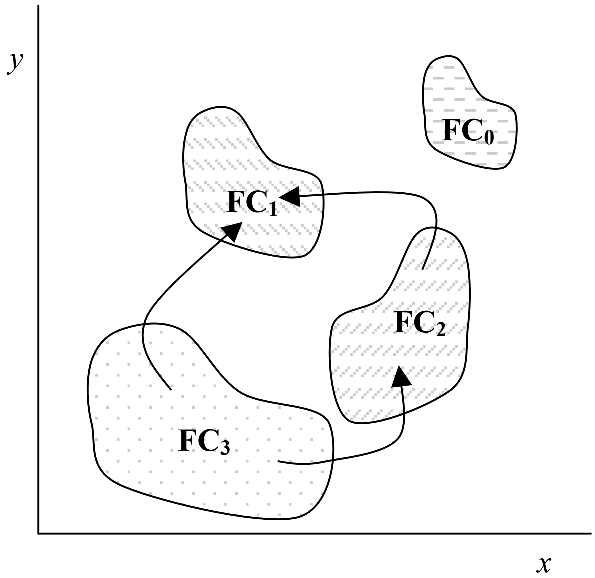


Figure 13: The parametric space of Chomskyan computational phenotypes

[Each computational phenotype occupies a specific area within the space brought about by the interaction of the morphogenetic parameters involved in cognitive development (here just two idealized parameters *x* and *y*). Once one or more parameters — the control parameter(s) — attain some critical value, the conditions are met for a 'jump' (arrows) within the space of possibilities. This kind of figure (which Alberch also used in his writings) differs slightly from the one we used before for representing parametric spaces (Figure 5), because it remains silent as to the relative probability of a jump from one phenotype to another. The larger or smaller surface of a phenotype still represents, however, its greater or smaller relative stability and, consequently, the probability of its occurrence. Finally, as for the absence of arrows pointing toward the $FC_0$ phenotype, this is our way to capture the idea that there may not exist any possible developmental path leading to it. Contrary to what it may seem at first blush, this is not at all problematic. As pointed out by Rasskin–Gutman (2005: 214–215), we must distinguish between (i) a 'theoretical morphospace', including possible (both actual and potential) and impossible phenotypes, and (ii) an 'empirical morphospace', excluding the latter. Its exclusion is justified by natural limitations on the parametric factors involved.]

Note, then, that, despite the fact of not having a precise characterization of phenotypes at the morphological level (but see section 3.3 below for some proposals in this direction), we do have a precise computational characterization of our computational phenotypes. In fact, whatever the specific morphological properties of a phenotype, we know that, if it is associated with phenotype $FC_3$, its computational power will be equivalent to a finite-state automaton; if it is associated with $FC_2$, it will be equivalent to a push-down automaton; if it is associated with $FC_1$, it will be equivalent to a linear-bounded automaton; and, finally, if it is associated with $FC_0$, it will be equivalent to a Turing machine.

Observe, moreover, that the parametric space of cognitive computational phenotypes possesses the very same properties as the morphospaces proposed by Alberch in his works. That is, it shows a non-continuous distribution of phenotypic variation, with discrete and easily identifiable states. Thus, the transition from one state to another is, in fact, a 'jump' that is only made possible once a specific critical point is attained (possibly as a result of the accumulation of small gradual changes). As a consequence, just as in Alberch's model there is no sense to be made of the phrase "being between phenotype A and phenotype B", in ours one cannot describe a system whose computational regime is somewhere in between, say, a finite-state system ($FC_3$) and a context-free system ($FC_2$). It is nevertheless important to take into account that, as already pointed out by Bateson (1894), the fact that there is discontinuity in *variation* is not in contradiction with the idea of gradual change at the level of *processes*; this is, in fact, a fundamental feature of the concept of 'critical-point emergence' to which we shall appeal here (see Reid 2007: chap. 8, for details). According to our proposal, then, the evolution of what we call 'the computational mind' would have consisted in a historical process where complex interactions among genetic and epigenetic factors during the individual developmental process of the nervous system would have given rise to qualitatively differentiated phenotypes via a sequence of 'critical-point' emergent processes. Such phenotypes would have been able to act as the material support for richer computational regimes, and one of these 'jumps', the one leading to a computational regime of type 1 ($FC_1$), would have been the one that made possible the emergence of human language.

Before sketching our proposal for an evolutionary scenario, to which we turn presently, we would like to devote some space to describe some of the most important properties of the different systems and of the different computational regimes capable of generating them. We shall begin with the less relevant ones (type 0 systems), which will additionally serve as a justification for our having relegated them to the realm of impossible phenotypes within theoretical morphospace. Next, we turn to the remaining cognitive phenotypes, much more relevant for our purposes.

Chomsky (1959: 126–127) has already pointed out that we could not learn much about language if its elements are specified in the form of "such 'unstructured' devices as general Turing machines". In order to grasp the essence of this assertion, it may be relevant to recall some basic aspects of the mathematical theory of recursive functions, to which the theory of computation is intimately related.[25]

Remember, first, that, as stated in the caption of Figure 12, a Turing machine has the power of generating *any* recursively enumerable set, that is, any finite or infinite set that may be put in a one-to-one relation with the set N of natural numbers. This implies that, if some set A is enumerable, then a bijective function *f* exists assigning to every element of A an element of the set N, and, consequently, that the cardinal of A is, at most, $\aleph_0$, that is, equal to the cardinal of

[25] To be precise, we should have written 'μ-recursive functions'. The thesis that μ-recursive functions and Turing machines are equivalent is traditionally known as Church's Thesis or the Church–Turing Thesis; on this topic and for a large part of what is discussed in the text, see Lewis & Papadimitriou (1981).

the set of natural numbers. The property of being recursively enumerable implies, therefore, the theoretical possibility of counting the elements in the set, such that all finite sets plus all infinite sets (or also the finite ones) sharing with N the (crucial) property of being *recursive* are recursively enumerable. This point is crucial, because, from the enumerability of a set one may not necessarily deduce its recursivity (although the reverse is always true: Any recursive set is enumerable). In this case, one must resort to a different class of recursive functions, namely those which, given an arbitrary element $x$, are capable of returning a result of 1 or 0 (or True or False), such that, if the result is 1, then $x \in$ A, and, if the result is 0, then $x \notin$ A. Note that, now, it is not a matter of *counting* the elements of a set, but of *deciding* (or *generating*) what are the elements of that set and, indirectly, those of its complement.[26] From this it follows that the set N of natural numbers is an ideal model to deal with these issues, since we know that it is enumerable (we can put it in a relation to itself to count it) and that there is a finitely definable procedure capable of generating it (for example, Peano's axioms).[27] The notion of a finitely definable procedure brings us to the point at which we can add a third fundamental element to Church's Thesis, which asserts that any μ-recursive function may be imitated by a Turing machine (and vice-versa). This third element asserts that a Turing machine may be imitated by a rewriting system or *grammar*, understood as a system of rules the recursive application of which allows us to put a string of symbols in a relation with another string of symbols.[28]

And thus we come to the point where Chomsky's words quoted above can be clearly interpreted. The family of rewriting systems equivalent to a Turing machine is the family of unrestricted rewriting systems, that is, those systems whose rules can put strings of an arbitrary length into a relationship with strings of an arbitrary length. With no constraints on the nature of rules, any rewriting system may be able to generate any set of strings of symbols, such that, for example, the set of all strings made up by the iteration of any instance of the symbol *a*, which we can abbreviate as $a^*$ (any sequence of zero or more *a*s), might be generated by a system of rules like $S \rightarrow \varepsilon$, $S \rightarrow a$, $S \rightarrow aa$, $S \rightarrow aaa$, and so forth, where ε represents the empty string. Clearly, *this is not a finitely definable procedure* to generate the set $a^*$. This is perfectly natural for a computational device like a Turing machine, since it has, after all, an unlimited amount of time and space available to carry out its work; but it is not so for us, because what we want is a finite device for generating the elements of a set (such as, for example, the set of sentences of a natural language), and the simple fact that a set is recursively enumerable is not a guarantee of the existence of such a procedure. The only family of sets for which we know for certain that such a procedure exists is recursive

26 Which moreover implies that, if set A is recursive, both A and its complement are recursively enumerable, but not, of course, that the complement is also recursive.

27 Or, to be precise, Peano's ninth axiom of his *Arithmetices principia* (1889) or Principle of Induction, which defines the successor function σ, such that σ(n) = n+1 for all n ∈ N, and which is one of the primitive recursive functions from which it is possible to define all other recursive functions, including the μ-recursive functions.

28 What Church's Thesis states, therefore, is that the three ways of understanding the idea of computation — recursive functions, automata, and grammars — are strictly equivalent.

sets. This is not, however, the only problem posed by unrestricted rewriting systems, as — and returning to the case of the set $a^*$ — we see that what we have just demonstrated is that a Turing machine is able to give an extensional definition of the set, that is, that it is able to enumerate each and every element of the set, but nothing else. This is what Chomsky refers to when he says that these systems are 'unstructured': A mere list of its elements tells us nothing about the properties of the elements of the set, when, in the case of natural language, for example, we are not only interested in knowing whether some sequence of words belongs to the language or not, but also, and above all, we want to know its internal structure. Again, the only systems capable of providing us with this information are those capable of generating recursive sets, to which we turn below.

The simplest computational devices are type 3 or regular systems, whose equivalent in the theory of abstract machines is the finite-state automaton. These systems have the power of generating sets of structurally very simple strings (or languages).[29] Thus, for example, the language $a^*$, which we encountered above, is regular, as are languages like $a^*b^*$ (a possibly null sequence of *a*s followed by a possibly null sequence of *b*s), $a^nb^m$ (a non-null sequence of *a*s followed by a non-null sequence of *b*s), and the language $\{a, b\}^*$ (that is, the one constituted by sequences of *a*s and *b*s of any length and in any order). A quick glance at these languages is sufficient to see that linear order is not a problem for this kind of system, as is perfectly possible to build grammars (or automata) capable of generating sequences where symbols follow a strict order; but this is not a sufficient guarantee of adequacy and for regular grammars to capture the complexities of natural languages. In fact, Chomsky, in a brief note (Chomsky 1956b), has already demonstrated that a language like $a^nb^n$ (a sequence of *a*s followed by a sequence with the *same* number of *b*s) is beyond the generative power of a finite-state system. The datum to which we need to pay attention here is that the complexity of this new language has nothing to do with the relative order of both substrings, but rather with the fact that both substrings must be of the same length, which is equivalent to saying that there exists a dependency relation between both substructures. In other words, in order to be sure that both substrings will be of the same length, we need some device to keep track of the number of symbols used during the process of construction of the first substring, such that we can access this information while we are building the second one. In a nutshell, we need *memory*, a resource which is not available in a finite-state automaton. As Chomsky points out in the reference cited above, natural languages have plenty of this kind of dependency relation,[30] which automatically invali-

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29 It is important not to confuse the term 'language' as we use it here in the context of formal language and automata theory, with the way we use it in the rest of this paper, which corresponds to its traditional meaning in the field of generative linguistics. In the former case, 'language' refers to a set of strings generated by a grammar; in the latter case, to the capacity of humans to produce and comprehend sentences. The equivalence is not precise, but assuming there is one, the 'grammar' of a mathematician is like the 'language' of a linguist, who pays little or no attention at all to lists of sentences and is more interested in the construction of grammars.

30 Such as, for example, constructions of the *if… then*-type, or relative clauses, to name just two

dates the ability of finite-state systems to capture some of the most basic properties of human language.

In the light of these results — well known, as we have seen, since the 1950s — research in the field of the formal complexity of natural language turned its attention to type 2 and type 1 systems. This research was motivated, on the one hand, by what for some linguists was a premature quantum leap towards the development of transformational models, after Chomsky's rejection of context-free systems as adequate models for natural language grammars (see Chomsky 1957: chap. 5) and, on the other hand, because of some formal results that appeared to indicate that transformational models were equivalent to unrestricted rewriting systems (Peters & Ritchie 1973). Thus, around the mid-1980s, an important body of data was available, justifying, on the one hand, Chomsky's dismissal of type 2 grammars (see, in particular, Bresnan *et al.* 1982 and Shieber 1985) but, on the other hand, showing that the necessary expressive power was only slightly above type 2 systems, within a complexity space not identified by Chomsky when he defined his Hierarchy, and which Joshi (1985) named mild context-sensitivity.[31] Whatever the definitive position of natural language within the complexity scale, however, our main concern here is to characterize the differences between the different systems within the space between type 3 grammars and type 0 grammars. For this purpose, formal languages are a useful tool, since they help us to focus on specific structural properties and on the necessary computational resources to deal with them. Let's go back, then, to the language $a^n b^n$, which, as we saw, is not a type 3 language, but a type 2 language. Depending on the kind of structural description we want for, say, the string *aaabbb*, a context-free grammar offers us a number of alternatives, of which we only contemplate the following,

(1) $[a_i\ [a_j\ [a_k\ b_k]\ b_j]\ b_i]$,

where sub-indices indicate the presence of some dependency between the elements sharing the same sub-index. As we will see presently, the source of complexity is not in the number of dependencies, but in the relations among them. Note that in (1) dependencies are strictly nested, and a type 2 system is perfectly capable of dealing with constructions with multiple nested dependencies (or, for that matter, with sets of independent nested dependencies, that is, $a^n b^n c^m d^m$, $n \neq m$, is also a type 2 language). As we pointed out earlier, the key is

of them. Note, by the way, that what is important is not that both strings be of the same length, but that some dependency relation holds between two elements separated by an arbitrary long sequence of symbols. As we will see presently, the number and nature of these dependencies are critical factors at the time of assessing the degree of complexity of a language.

31 Which, moreover, means that such expressive power would sit below that of indexed grammars. So far, only one objection with respect to these results has been presented in connection with the sub-system of Mandarin names for cardinal numbers, whose properties appear to be beyond the power of mild context-sensitive grammars (Radzinski 1991). It is significant, however, that this result is connected to number names, which, for some authors, may be indicative of some extra-linguistic factor not directly related to the real complexity of natural language; see Pullum (1986).

in memory, a resource that is available to a push-down automaton (our $FC_2$), but not to a finite-state machine (our $FC_3$). The push-down stack in a push-down automaton supplies the additional workspace where we can store those symbols we have generated (for example, three *a*s) and which we pop out as we add *b*s to the string: For each *b* we add to the string, we pop an *a* out of the stack, such that, when the stack is empty, the process is over. Given the structure of the stack, which follows a first-in/last-out regime, we can see that nested dependencies fall within the power of type 2 grammars since, when we write the first *b*, we pop out the last *a* that went into the stack, and so on, until the point at which we write the last *b* and pop out the first *a* we stored in memory. Suppose now that dependencies are organized as in (2):[32]

(2) $a_i a_j a_k b_i b_j b_k$

Note that in this case the dependencies are crossed, such that the first *a* is related to the first *b*, the second *a* with the second *b*, and so on. This kind of structure is beyond the processing power of a push-down automaton, as are more complex languages like $a^n b^n c^n$. Without for the moment going into great detail (but see Weir 1994 and Joshi & Schabes 1997: sect. 7), what we need here is a more powerful automaton, one we can get by just improving the capabilities of the memory system, extending and restructuring it such that it will be able to create additional stacks to store data any time that this is required by the computation.[33]

Thus, and on the basis of the preceding discussion, it is clear that the progression up the scale of complexity is a function of the changes introduced in the memory system, *with no other modification of any fundamental property of the computational system being necessary*. This observation puts us in a position not only of being able to characterize the phenotypes represented in Figure 13 in computational terms, but also of being able to determine the kinds of alterations of the developmental system which are necessary to 'jump' from one phenotype to the other. Therefore, assuming that we have a simple (but recursive)

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32 This is the case of the cross-serial dependencies found in Dutch and in some varieties of Southern German, where the English construction *…that John saw Peter help Mary swim* may be expressed in Dutch as *…dat Jan Piet Marie zag helpen zwemmen*.

33 This is an extremely intuitive characterization of the embedded push-down automaton, which is equivalent to a mild context-sensitive system. We are using this example here, instead of that of the linear bounded automaton, because as Weir (1992, 1994) has shown, push-down automata constitute a general model of automata, of which the classical push-down automaton (with one stack) and the embedded push-down automaton are only two particular cases, the simplest ones within a scale of increasing complexity definable exclusively in terms of improvements introduced in the storage system.

Weir's results in fact go well beyond that, since they can be generalized to the whole family of languages made up by type 2 and type 1 languages in the Chomsky Hierarchy and which constitute a natural class within the Hierarchy, sharing a number of interesting computational properties (e.g., recognition in polynomial time and decidability, among others). Weir's work defines a sub-hierarchy within the old hierarchy, in which we observe a progression towards higher degrees of complexity definable just in terms of the levels of embedding of the memory stacks (i.e. stacks of stacks, stacks of stacks of stacks, and so on), a particularly relevant mathematical result for the proposals we develop in this article.

computational system, we only need first to add some memory, and thereafter progressively to sophisticate this system of memory, in order to make the transition from one phenotype to the other possible ones, going up the scale of complexity and concomitantly acquiring the ability to execute computational operations of an increasing complexity (Uriagereka 2008). This is, in essence, the sketch of our proposal for the origins of the FL, which would be the product of an evolutionary process with the effect of increasing and ameliorating the system of memory available to an original computational system, until the point at which sufficient capacity was reached to give way to what today we know as language.[34]

### 3.3. *Steps towards the Identification of the Control Parameter and the Reconstruction of the Evolutionary Process*

The steps, certainly rather tentative, towards the definition of the control parameter identifiable as the main trigger of the developmental changes capable of causing the evolutionary process described in the previous section force us to picture a minimally realistic image of the computational system underlying language. With a precision number we will introduce below, we will assume a proposal quite similar to Lieberman's (2006) model of the 'basal ganglia grammar' (BGG).

Philip Lieberman's model is based on a fundamental distinction between:

(A) a *cognitive pattern generator*, whose inhibition/excitation mechanism is localized in the basal ganglia; and

(B) a *working memory space*, which is located in Broca's area.

(Lieberman 2006: 207–209)

These are, respectively, the sub-cortical (A) and cortical (B) components of a circuit which Lieberman defines in functional terms as an *iterative sequencing machine*, which is at work when we walk, talk or understand a sentence (Lieberman 2006). Assuming this global picture, we wish, however, to introduce a couple a qualifications about the adoption of this neuro-anatomical structure as the basis of our model for the computational system underlying language.

Firstly, basal ganglia comprise a complex anatomical structure which

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34 As the reader may have already guessed, our hypothesis rests on two basic premises: (i) that Church's Thesis is true and (2) that the set of sentences of a natural language is recursive. Of course, there is no shortage of proposals denying either (or both) of the two premises above. For example, Penrose (1994) rejects (i) in favor of what he calls 'quantum computation' which, according to him, sits beyond classical computational models. As for (ii), Langendoen & Postal (1984) argue that the set of sentences in a natural language is not recursively enumerable (and, therefore, neither recursive nor enumerable), and that its cardinal is a transfinite number, that is, greater or equal to the cardinal of the set R of real numbers. In this case, any attempt to provide a computational characterization of the FL is doomed, because a non-enumerable set is, by definition, non-computable. Indeed, then, should any of these proposals turn out to be true, we would be out of the game — as would a large part of those who strive to unveil the mysteries of human cognition.

appears to participate in several cortico–subcortico–cortical circuits associated with the regulation of different aspects of mobility, cognition, and emotivity (Figure 14). Following Cummings (1993), Lieberman (2006: 163–167) estimates that the so-called ‘prefrontal dorsolateral circuit’ is the one involved in the programming of the motor control of speech, in sentence comprehension, and in other aspects of cognition. This circuit projects from this cortical area towards the dorso-lateral area of the caudate nucleus, the lateral dorso-medial area of the globus pallidus, and the thalamus which, in turn, projects back to the prefrontal cortex (Figure 15). We will assume, with Lieberman, that this is in effect the circuit that language uses as a computational system.

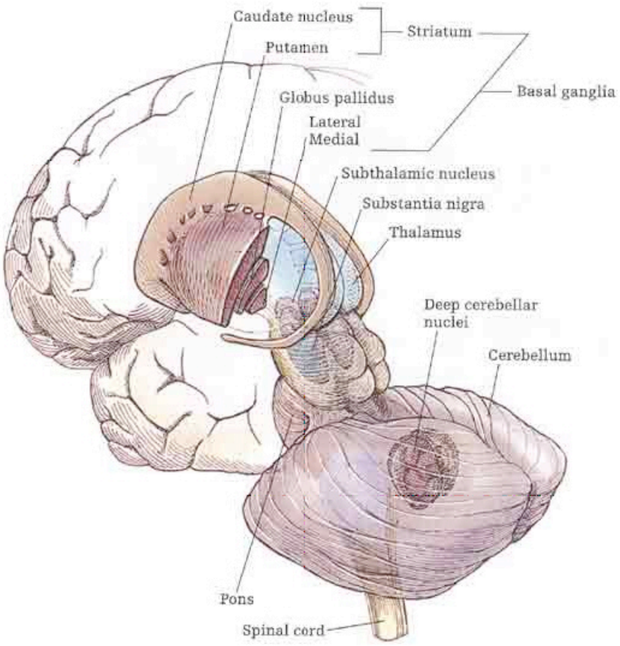
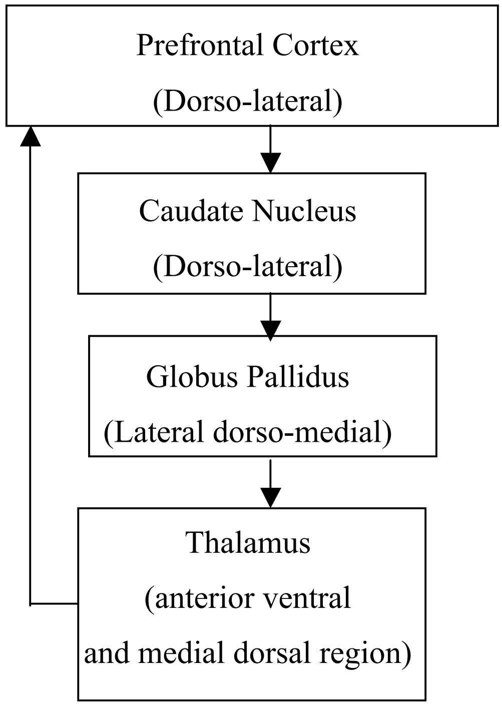



Figure 14: The basal ganglia

[The basal ganglia comprise a complex anatomical structure located in the inner brain, with numerous afferent and efferent projections among their own components and including the frontal cortex and the thalamus.]




Figue 15: Prefrontal dorso-lateral circuit
[The prefrontal dorso-lateral circuit is involved in the motor programming of speech, in sentence comprehension, and in other aspects of cognition. Other similar, and partially overlapping, cortico-subcortico-cortical circuits participate in other aspects of cognition and behavior. For example, a circuit projecting from (1) the prefrontal cortex towards (2) the striatum (caudate nucleus + putamen), (3) the globus pallidus and the substantia nigra, and (4) the thalamus which, in turn, projects back to (1) the prefrontal cortex, appears to be involved in affective modulation. (Based on Cummings 1993.)]

Now, and this constitutes our second qualification of the BGG model, we contend that limiting to Broca's area the localization of the working memory space is an assumption that oversimplifies. The involvement of Broca's area in the system of computations of the FL is unquestionable.[35] However, it is quite plausible that this structure is part of a larger cortical circuit with bidirectional projections between the frontal and parieto-temporal areas, within a system of working memory networks such as the one postulated by Aboitiz *et al.* (2006).[36] The matter is not entirely clear. However, for the purposes of this article, it is important for us to localize the working memory of the computational system within the cortical component, more or less large, of the anatomical structure making up such a system.[37]

[35] See, among others, Embick *et al.* (2000), Grodzinsky (2000), Moro *et al.* (2001), and Musso *et al.* (2003).

[36] See also Aboitiz & García (1997). On the role of the frontal lobes in language processing, see the review by Friederici (2002), as well as the studies by Shtyrov *et al.* (2003) and Pulvermüller & Assadollahi (2007).

[37] For a partially divergent model, see Ullman (2004), where the assumption is made that the computational aspect of language utilizes a system of 'procedural memory' distributed

Our main thesis in this section is, as we already put forward at the end of section 3.2, that those perturbations which motivated the bifurcation of the computational system in the direction of context-sensitivity ($FC_1$) during human evolution have a direct connection with the development of the cortical structure supporting working memory. This idea is entirely justified if we take into account that the different levels of computational complexity making up morphogenetic space as defined in Figure 13 correlate with the capacity and the organization of the system of memory associated with the pattern generator. In purely formal terms, one goes up the hierarchy as memory gains capacity and, in terms of our evolutionary scenario, bifurcations occur within the morphogenetic space as a more developed cortical structure is available to support this. Within the framework of our proposal, however, no greater inter-specific differences are expected at the level of the system's pattern-generation procedure nor in the sub-cortical structure acting as its material support. As a matter of fact, from an evolutionary perspective the basal ganglia, as opposed to the cortex, have been described as highly conservative structures among amniotes (reptiles, birds, and mammals) (see Reiner *et al.* 1984).[38]

This thesis has a number of interesting consequences, to which we would like to devote some space. Firstly, it is our contention that the developmental event giving rise to the cortical structure serving as the physical support for the working memory space of the computational system is not an evolutionary event directly related to language. As we already noted above, the kind of brain growth which characterizes human evolution appears to be connected with the overproduction of precursor cells during cytogenesis, meaning that it is not a process oriented towards the production of one or another specific type of cell. Kaskan & Finlay (2201: 27) point out that, from the point of view of development, the brain is one of the few organs whose the development appears to follow a set rules affecting the organ as a whole. This observation is, in our opinion, particularly interesting, since it supports the idea that the perturbations giving rise to the higher level of complexity of the computational system subserving language do not seem to be, in any sense of the term, *adaptations* for this linguistic function, but rather that such a level of complexity is a mere side-effect of a more general process of brain reorganization which took place without specific motivation.[39]

That said, and along the lines of Rakic & Kornack (2001: 46), we are persuaded that the control parameter whose perturbations gave rise to the level of complexity necessary for language may well have been a regulatory gene (or a

across the frontal and parietal cortical areas, the basal ganglia, and the cerebellum. In Ullman's model, however, no computational specializations between a sequencer and a working memory are assumed, and the basal ganglia are seen as responsible for the stimulation and inhibition of the memorized patterns across the whole brain circuit.

38 Our proposal, therefore, has an important difference from that of Hauser *et al.* (2002) in the sense that, for us, the evolutionary novelty that made possible the emergence of language would not be, strictly speaking, recursion, but rather the ability to deal with recursive patterns via a higher level of complexity.

39 For some considerations about this point, see Chomsky (1968: 124), Chomsky (1975: 74), or, more recently, Hauser *et al.* (2002: 1578).

collection thereof) responsible for the proliferation of neuronal precursors. A mutation in this gene (or genes) in humans would have had an effect of 'peramorphosis' or 'terminal extension' (or 'hypermorphosis') in cortical development which, even if minimal in terms of chronological timing, may have had far-reaching anatomical repercussions, given the exponential relation between the extension of cytogenesis and the development of brain structure (Figure 11).[40] With respect to this idea, however, it is important to make a couple of points clearer:

First, by pointing to a regulatory gene as the control parameter responsible for the evolutionary transformation we have just suggested, we are not denying that other morphogenetic parameters involved in the very same developmental system remained unchanged. On the contrary, the perturbation of the control parameter in question may well have triggered a whole chain of perturbations affecting other parameters, with obvious repercussions on the structure and function of the resulting phenotype. In this particular case, we can plausibly speak of a chain-effect affecting such factors as cortex myelinization or dendritic growth, for which also late termination effects have been observed (Gibson 1991). As was made clear in section 2.2, a control parameter is not a unique causal agent, but just the initial trigger in the chain of reactions giving rise to a new morphology.

Second, although a direct consequence of the late termination of the development of cortical structure, the mere gross increment in the population of neurons cannot be taken as an indication that the complexity level attained by the system is a direct function of the said increment. In this connection, it is important to take into account the general thesis of dynamic systems according to which there is no linear and simple relation between the morphogenetic parameters and the properties of the phenotype. Quite to the contrary, then, the most direct effect of a perturbation (in our case, the gross increment in the population of neurons) may trigger novel phenotypic effects which nevertheless need not imply the incorporation into the developmental system of new morphogenetic parameters. Particularly relevant for the case in point is Terrence Deacon's observation that larger brains will necessarily possess more laminated, more highly nucleated, more parcellated structures, and so on, just as a side-effect of the growth of the neuronal population with respect to brain size (Deacon 2000: 61). We understand that very plausibly these are factors capable of having a great impact on the kind and complexity of the operations the brain is able to execute.

There is another important question deserving further comment. According to our proposal, the level of complexity of the computational system of language would be explained in evolutionary terms as a particular aspect of a global perturbation in the developmental system of the brain, and not, therefore, as an episode in brain development directly selected for its advantages related to linguistic function. This is not to deny that, next to the global reorganization of the brain, other developmental events may have taken place directly related to the functionality of resulting structures of this global process of reorganization.

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40 Along similar lines, see Edelman (1987), Finlay & Darlington (1995), and Deacon (2000).

As pointed out by Hofman (2001: 122–123), in fact, brain sub-structuring into anatomically and functionally specialized modular units is one of the effects derivable from the expansion pattern typical of its evolution, and, as Rakic & Kornack (2001: 49) add, this causes the introduction of novel organizational units and interaction pathways among these units which may later be targeted by natural selection.

In this regard, the human mutations of the *FOXP2* gene,[41] which have been the focus of various studies and interpretations in the last few years, may well find its way into our proposals. The function of this gene in the regulation and functioning of the brain structures which, following the BGG model, we identified with the computational system of language is a well established fact.[42] Remember, however, that our proposal singles out as the event responsible for the evolutionary change of this system the increment in the capacity of its working memory, which, in turn, we identified with the cortical component of the BGG. Thus, our suggestion puts special emphasis on the overdevelopment of the cortex, but less so — in evolutionary terms — on the basal ganglia. The idea is, therefore, compatible with our observations above concerning the fact that in brain growth a disproportion is observed between the growth of the cortical component and that of sub-cortical structures, the basal ganglia among them (Hofman 2001: 117–118). Now, this is not incompatible with the fact that in this context some specific mutations capable of adding robustness to development and of modulating the global activity of this system may have been selected. This is in fact what, as we see it, appears to be the more realistic interpretation for the positive selection of the human variety of *FOXP2*, which would thus not be responsible for the evolution of the computational system of language per se, but rather a response to the kind of functionality independently attained by this system during its recent evolutionary history.[43]

One must not forget that language is not only a system with specific computational properties. It is also a symbolic system which, precisely because it possesses such computational properties, also possesses the property of discrete infinity.[44] This is important because, as pointed out by Lorenzo (2006), it is perfectly plausible to assume that in the animal kingdom certain behaviors are

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41 See Lai *et al.* (2001). The exact dating of said mutations is still a much debated issue: Enard *et al.* (2002) originally attributed to them an antiquity of some 125,000 years, but Krause *et al.* (2007), after the identification of some fossil DNA from Neanderthal individuals found in the cave of Sidrón (Asturias), extend this date to 300,000 years and argue for the existence of the human variety of the gene already in Archaic sapiens. Coop *et al.* (2008) argue, however, that the application of their statistical method for phylogenetic dating yields a result below 50,000 years; for a critical overview of the Neanderthal *FOXP2* and its possible implications for the linguistic capacities of these hominids, see Benítez Burraco *et al.* (2008) and Balari *et al.* (2008).

42 See Ferland *et al.* (2003), Lai *et al.* (2003), Liégeois *et al.* (2003), Takahashi *et al.* (2003), and Benítez Burraco (2009) for a comprehensive state of the art.

43 In this connection, it is interesting to note the recent identification of a gene (*CNTNAP2*) involved in cortical development and which integrates the *FOXP2* regulatory chain. Some varieties of this gene may be the source of diseases more or less selectively affecting language; see Vernes *et al.* (2008).

44 For a characterization of this property, see, for example, Hauser *et al.* (2002: 1571).

observed which suggest the presence of a symbolic system (or the rudiments thereof) — as is perhaps the case of the systems of calls and gestures of some primates (see Cheney & Seyfarth 1990, 2005; also Tomasello & Call 1997: chap. 8), or the use of very complex recursive patterns, as may be the case with some birds (Marler 1998), *without this implying the simultaneous presence of both complex recursion and symbolism*. To extend and to perfect the working memory space is, therefore, a necessary condition for the emergence of language, but not a sufficient one. This dissolves a potential objection to our notion of computational phenotype, namely that it may not necessarily be the case that this phenotype (and its corresponding morphological phenotype) correlates with the presence of FL. In fact, this observation is correct and our $FC_1$ might well correspond to non-linguistic 'minds' which would nevertheless be capable of producing complex recursive patterns within other areas of cognition, such as, for example, motor sequences or melodic sequences in birdsong. Remember that, according to Lieberman's (2006) model, the basal ganglia comprise a sequencer of cognitive patterns, but cognitive patterns may be of many different sorts and the sequencer, just because of connectivity and working space limitations, might have access to only a single type of pattern or to a limited collection of them (motor patterns and melodic patterns, for instance), but not to others (symbolic patterns, assuming these are even available). In the wake of this line of reasoning, a whole mosaic of evolutionary possibilities opens itself, where symbolic capacities and complex recursion may have appeared independently from each other or even coexist without meeting, with the single exception, perhaps, of human language.[45] Thus, for example, and focusing on birds and primates — which are, perhaps, the most interesting species in this connection, apart from also being the most studied ones — we see that complex recursion may have evolved associated with some abilities observed in birds, specifically with bird-song and nest building (Figure 16), whereas evidence for symbolic behavior is meager and, in any case, with no hint of complex recursion.[46] As for primates, hardly any evidence exists suggesting the possession of abilities with a subjacent complex recursion apart from, perhaps, motor control,[47] whereas some observations suggest the presence of vestigial symbolic or proto-symbolic capacities.[48]

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45 For example, Balari (2005, 2006) argues for such a scenario of the casual 'meeting' between a symbolic capacity grounded on social cognition and complex recursion through a heterochronic process in the development of the nervous system as the basis for the origins of human linguistic capacities; see also Lorenzo (2008).

46 Griesser (2008) notes, for example, that the Siberian jay (*Perisoreus infaustus*) makes use of a series of alarm calls whereby it discriminates whether a hawk, its main predator, is (1) inactive sitting on a tree branch, (2) looking for prey, or (3) in attack attitude. These signals nevertheless lack the structured and discrete character of other forms of birdsong with which, without any hint of symbolic content, many birds attract the attention of potential sexual partners.

47 See Tomasello & Call (1997: chap. 3) for an overview of manual dexterity and use of tools in different species of monkeys and primates.

48 The presence of alarm call systems in inferior primates has been known for years, usually based on specific associations with one or another type of predator. See the pioneering work by Struhsaker (1967) on the alarm calls of vervet monkeys, as well as Hauser (2000) and Cheney & Seyfarth (2005) for up-to-date overviews of this issue. Recently, Arnold &

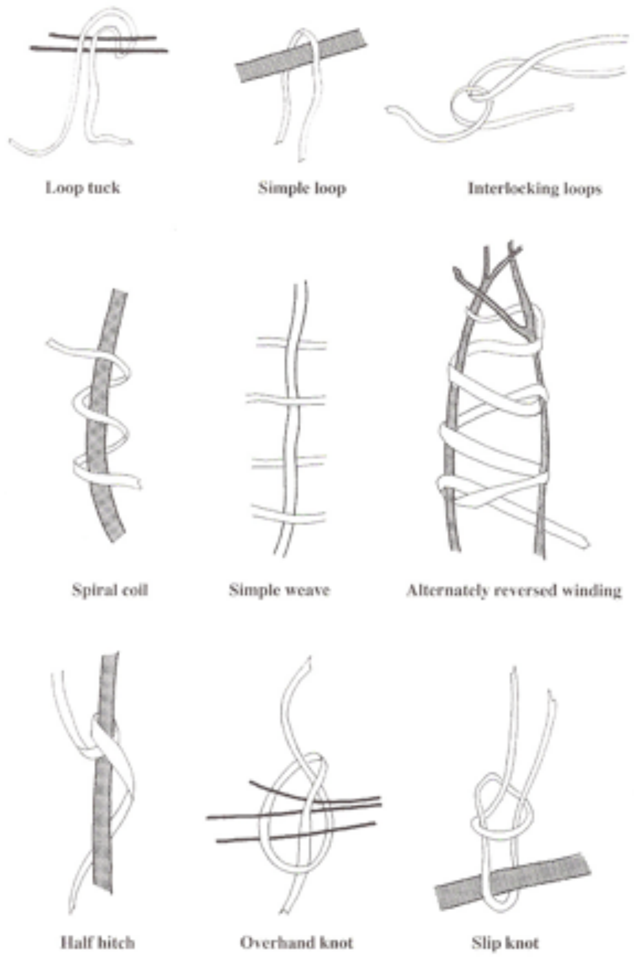


Figure 16: Computational complexity without symbolic representation & communication [Making a knot requires the application of an operation over a part of the constructed figure, and keeping it in active memory until the moment at which the operation completing the figure is executed. It requires, therefore, the participation of a working memory capable of an active bookkeeping of the operations executed. Thus one must not exclude the fact that the level of computational complexity required for building a hanging nest, such as those constructed by many species of weaver birds, occupies a relatively high position within the Chomsky Hierarchy, even perhaps within our $FC_1$. For the building behavior of birds and other animals, see Hansell (2000 and 2005). On the relevance for knots to infer complex computations, see Camps & Uriagereka (2006). In the image, some knotting and weaving techniques used by weaver birds. (Taken from Hansell 2000: 85.)]

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Zuberbühler (2006a, 2006b) have argued that some Old World monkeys (specifically *Cercopithecus nictitans* or white nose monkeys) can combine two different kinds of calls they also emit independently. The truth is that this type of capacity does not seem to require more than our $FC_3$; it will then be very far from the computational complexity of the phenotype corresponding to human linguistic capacities. The 'parametric distance' between one phenotype and the other foreseen in this work automatically invalidates Arnold & Zuberbühler's thesis that human language syntax may be derived from the kinds of combinatorial capacities observed in these monkeys through a simple process of gradual and continuous evolutions. This issue is discussed further in Lorenzo (2008) and in Longa (2008).

We believe that these considerations have important methodological implications, especially with respect to the application of the comparative method when seeking precursors of the FL or of some of its constituent properties like complex recursion. Indeed, one of the most direct consequences of what we have said so far is that formal grammar and automata theory may prove to be an extremely useful tool at the time of assessing the abilities and capabilities shown by other animal species, a point also argued for by, for example, O'Donnel *et al.* (2005). However, one must also be careful when using these theoretical tools in experimental design, for example, in order to avoid an excessive trivialization of the results. There already exist, as a matter of fact, some works along these lines which have been the target of very strong criticisms. For example, Gentner *et al.* (2006) have experimentally tested the abilities of some specimens of European starling (*Sturnus vulgaris*) to learn to discriminate songs with different degrees of complexity, correlating with different types of formal grammars. The birds in this study were instructed, using reinforced conditioning, to identify sequences following a pattern of *n* repetitions of a succession of two characteristic sounds ($ab^n$) of the species' birdsong, formally characterizable in terms of a regular grammar (type 3 in the Chomsky Hierarchy). These birds were moreover instructed using the same method to identify songs following a pattern of *n* repetitions of a sound followed by an identical number of repetitions of other characteristic sounds of the species (the language $a^nb^n$, we have already come across before in this article; see the end of section 3.2). Unlike the former case, the grammar associated with this pattern is a type 2 context-free grammar, with a principle of recursive embedding of each new pair (*[ab]*) within another identical pair (*[a[a[...]b]b]*). Not without difficulties, but with a level of success above chance (nine in every ten individuals), starlings were capable of identifying the songs following this complex pattern. Similar experiments carried out by Fitch & Hauser (2004) with tamarin monkeys (*Saguinus aedipus*, a species of New World monkey) offered quite different results. According to the authors, these monkeys find almost no difficulties in discriminating sequences of several repetitions of an $ab^n$ pattern, made up by a syllable picked from an initial inventory of eight syllables (*a*), followed by another syllable picked from a second inventory with eight different syllables (*b*), from other sequences not following this pattern. They find, however, insurmountable difficulties when it comes to discriminating sequences made up by some specific number of syllables from the first inventory followed by the same number of syllables from the second inventory (again, $a^nb^n$) from deviant sequences that do not follow this pattern. These monkeys, then, appear to be able to classify sequences unproblematically within the range of a regular grammar, but appear to be incapable of doing so with only slightly more complex sequences but which are already within the context-free space. One interpretation of these works is, then, that tamarin monkeys possess a computational regime located within our $FC_3$ phenotype, whereas starlings would have reached at least our $FC_2$ phenotype. In their reply to Fitch & Hauser (2004), however, Perruchet & Rey (2005) question the validity of these results on the basis of the fact, so they argue, that humans probably do not process patterns like $a^nb^n$ making use of a center-embedding context-free grammar, which might be an indication that other species, like starlings, also process them by means of a different strategy.[49] Besides, and as pointed out by Pullum & Rogers (2006) and Rogers & Pullum (forthcoming), another problem with the Fitch & Hauser (2004) experiments is that these focus their attention essentially on the auditory processsing capabilities of tamarins and, along the lines of the preceding discussion, there is no guarantee that animal species different from ours show the very same abilities within the very same areas of cognition. In fact, let us suppose that, say, tamarins (unlike starlings, for example) might not be able to auditorely discriminate very complex sequences, but might possess abilities in other areas which could effectively be formally characterized by means of more complex systems. Despite this slightly skeptical note, we still believe that research aimed at the identification of the computational phenotypes instantiated by different species may play a crucial role at the time of validating the hypotheses presented above.

Returning then to the case of humans, in short, it is clear that the evolutionary process that gave rise to the FL, and which we characterized as an extension of the working space in the cortical area, would also have allowed the general sequencer access to cognitive types of several modalities, essentially of the symbolic type. This need not, however, be taken as an inevitable consequence of the growth of the cortex,[50] which may eventually explain the observed differences among other hominid species regarding their symbolic capacities.

Also in this connection, and without going into extensive detail, our opinion is that the evolutionary change that took place in connection with the emergence of the FL occurred only recently, after the branching point eventually leading to *Homo neanderthalensis*, meaning that this species never benefited from these changes. In the archaeological record associated with Neanderthals, no evidence is found that suggests the presence of a computational endowment either equivalent or even similar to what is needed for processing human language. For example, Camps & Uriagereka (2006) observed that the absence of objects implying the elaboration of knots — like necklace beads, fishing implements, small projectiles, and so on, all common enough in the sapiens record since some 80,000 years ago — may be illustrative of this fact (see also Piattelli–Palmarini & Uriagereka 2005).[51] As before, we must be careful and not forget the methodological cautions mentioned above and be prepared to discover that Neanderthals might have possessed such computational capacities in other

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[49] It is a well known fact that center-embedding structures are not easily processed by humans — at least since the observations made by Noam Chomsky and George Miller in the early 1960s (Chomsky & Miller 1963 and Miller & Chomsky 1963) — and this must be taken into account at the time of assessing some complexity results. Center-embedding structures are, in some sense, simple, since they fall within the computational capabilities of a type 2 system, but, at the same time — for humans, at least — they are complex, since center-embedding appears not to be one of the structural options selected by our species for the construction of linguistic objects. We use, however, other, formally more complex structures in addition to showing a clear preference for right-recursive objects. Note, however, that these considerations apply only to constituent structure, since, as pointed out in section 3.2, other kinds of dependency relations do follow a center-embedding pattern. Of course, as we already underline in the text, this may not necessarily be the case with other species.

[50] For some considerations about the functions of a hyper-developed cortex in humans, see Gibson (1993), Calvin (1993, 1996), and McKinney (2000).

[51] See, again, what was pointed out in the caption to Figure 16.

domains.[52] Whatever that ability may have been, we are pretty sure that it was not symbolic communication, in the light of the otherwise symbolic poverty of Neanderthals and of the strong asymmetry between their archaeological record and that of anatomically modern humans (see Balari *et al.* 2008 for an in-depth and recent analysis of these matters).[53]

## 4. Final Considerations

A complete fulfillment of the research agenda sketched in section 2 of this article and made explicit at the beginning of section 3.1 would require a detailed clarification of the environmental and populational conditions which were capable of filtering the distribution of computational phenotypes among the first humans in order to favor the result whose evolutionary model we developed throughout the preceding sections. However, we are not in a position to tackle this issues rigorously for reasons we hope will not be taken as the mere justification of patently visible gaps in our explanation (which we openly assume), but simply as the compromise that we will devote special attention to these topics in the very near future in what must be taken as the second part of this article.

Here we specifically focused on the evolutionary explanation of the computational system underlying the human faculty of language (or, following Hauser *et al.* 2002, the Faculty of Language in the Narrow sense, FLN). This means we only dealt with a particular aspect of language and, therefore, that we

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52 That said, it may be convenient to recall that Frederick Coolidge and Thomas Wynn have long defended the idea that the deep asymmetry between the fossil records associated with Neanderthals and anatomically modern humans might be due to the lack on the part of the former of the kind of 'extended' working memory which the latter eventually accessed. According to these authors, the singularity of the Neanderthal behavior would not really be a matter of lack of connections between a complex computational system and the kinds of abilities subjacent to the material culture associated with this species, but rather directly to the lack of a computational system as complex as the one evolved by the modern mind. See Wynn & Coolidge (2004) and Coolidge & Wynn (2005, 2007) for their analysis of technical intelligence, and Wynn & Coolidge (2008a) for their analysis of collective activity; for an overview, see Wynn & Coolidge (2008b).

A word of caution is advisable here. Both Coolidge and Wynn and the present authors appeal to evolutionary changes affecting the working memory space as an explanation of modern 'mentality'. However, it is important to see that in our case the term 'memory' is used from a computational perspective only, with no compromise in regard to some specific psychological model of memory. This is not the case of Coolidge and Wynn, who assume the model developed by the British psychologist Alan Baddeley since the mid-1980s (see Baddeley 1986, for a classical formulation, and Baddeley 2000 for the most recent version of the model). Of course, our proposal and that of Coolidge and Wynn are compatible, but we still want to emphasize the fact that, so far, ours does not commit us to any specific psychological model.

53 For additional evidence supporting this view, the analysis of the patterns of dentition in Neanderthals appear to indicate that their developmental program proceeded much faster than that of humans (see Smith *et al.* 2007), which may be taken as an additional clue in favor of the fact that the alteration in the developmental pattern that gave rise to the reorganization of the human brain may not have occurred before the speciation event leading to modern sapiens.

have so far only provided a partial explanation of this uniquely human capacity. True, this is a particularly crucial aspect of the evolution of the FL, especially if we accept the thesis of Hauser *et al.* (2002) that this aspect might concentrate the most distinctive (even exclusive) aspects of language within the broader context of animal cognition. We only slightly qualified this assertion, pointing at evidence which may indicate the presence of computational systems with degrees of complexity similar to the FL but associated with cognitive capacities that give rise to such disparate behavior as nest building.[54] In our opinion, the most distinctive element of language, seen from an evolutionary perspective, is the kind of associations this computational system has been able to establish through the recent evolutionary history of the human species via other capacities (sensory, motor, conceptual and intentional ones), bringing about a unified cognitive system dedicated to the internal representation and external transmission of complex contents. Our ongoing work is already oriented towards an attempt at explaining the evolutionary convergence of all these 'precursors' in a unified mental system as a result of alterations in the developmental pattern of the human brain. To the extent that no clear answer to this matter is available to us, we will also not be able to provide a rigorous answer to the questions posed at the beginning of this last section. The reason is simple: We believe that the populational processes which made possible the proliferation and generalization of the $FC_1$ associated with the FL had little to do with the emergence of this particular cognitive phenotype, but they are instead related to the association of FL with the kind of mental capacities which made it a system apt for the computation of externalizable mental representations (or, again following Hauser *et al.* 2002, the Faculty of Language in the Broad sense or FLB).

We can, however, offer a brief preliminary sketch of our ideas in this respect. We believe that the first humans who showed the capacity to externalize the complex expressions which their computational phenotype was able to process did not obtain great benefits from it. On the contrary, in the context of an evolutionary scenario dominated by forms of expressivity completely alien to the degree of sophistication of the sequences emitted by the members of this mutant population, it may well have been the case that it was taken as an element of stigmatization and exclusion for these individuals, who would have effectively been seen by the rest of the population as true "social monsters".[55] From a wider

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54 Hauser *et al.* (2002) also contemplate this possibility when they note that the most formally complex features of language may also characterize other species' representational systems like social intelligence or spatial orientation. The thesis they present as the "most probable option" is, however, that those systems are exclusively present in the linguistic computational system.

55 And here we can only borrow Lewontin's (1998: 113) words:

> Thus a species that possesses linguistic competence may indeed take over the earth as a consequence of the technological and managerial capabilities that are the result of language, but in a species lacking linguistic competence, the rudimentary ability to form linguistic elements by a few individuals may be taken as a sign of difference that causes them to be expelled or even killed.

We want to make clear, however, that this notion of "social monster" is not the same notion of 'monster' as it was used in classical teratology nor, more concretely, the same as the perspective, then, they would be 'misfits' to the social conditions in which their emergence took place. We believe, however, that social stigmatization against these individuals is the key factor that eventually favored the conditions for isolations (not necessarily allopatric) which made possible the preservation of the phenotype within the population (originally restricted to this stigmatized group), first, and eventually their proliferation and generalization within the whole population at some critical point in the early history of the human species. We tend to believe that such a critical point may coincide with the demographical crisis which, according to some authors like Behar *et al.* (2008), took place immediately before the intercontinental dispersion of modern humans. This situation, described as very close to extinction in a context of extreme environmental adversity, could effectively have fostered populational replacement in favor of a cognitive phenotype which may have been extremely advantageous for its possessors. According to this idea, then, the special cognitive endowment of this 'founding' population (Mayr 1963) would not have originally been an adaptation, given the dominant social conditions at the moment of its emergence, but it would have possessed from the very beginning some adaptive potential (or 'adaptability' in the sense of Reid 2007) which openly manifested itself at a critical point of human evolution.

What we have just sketched is prone to further elaboration and sophistication and, to this end, we need first a concrete model of the evolution of the FLB through development. In this article we have taken a first step in that direction, proposing a model for the evolution of its core component or FLN, based on the alteration of the developmental pattern of the cortical component serving as working memory for the computational system with a sequence machine located in the basal ganglia. The increment in memory capacity thus attained would have permitted the human brain to 'jump' towards a cognitive phenotype with qualitatively different computational capabilities compared with other existing phenotypes within a discontinuous space of computational regimes. To this end, we have followed rather closely some recent proposals within Evolutionary Developmental Biology and, in particular, those of Pere Alberch, a number of whose key ideas originally inspired this work. We are convinced that this inspiration will also take the lead in our future attempt to explain how the developmental pattern of the first humans may have been altered in order for this computational phenotype to give rise eventually to the linguistic phenotype that at present singles us out as a species.

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"logical monsters" of Alberch (see Figures 8 and 9). It is true, however, that underlying the "social monster" which we allude to in the text is a "biological monstrosity", which is not in any way the case, of course, in all cases of social stigmatization.

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# Ceaseless, Unpredictable Creativity: Language as Technology*

Jan Koster

Notions like 'biolinguistics' have a trivial and a non-trivial interpretation. According to the trivial version, a cultural phenomenon like language is only *based* on our innate biological capacities. Language, in this view, is not a matter of biology *per se* but of applied biology, i.e. a form of technology. Under this interpretation, 'biolinguistics' is uncontroversial and trivial because all our cultural activities are grounded in our biology. According to the non-trivial interpretation, the concept of language can be sufficiently narrowly construed so that we can define a core capacity that is comparable to a biological organ (like the heart or the liver). Recently, it has become common to see this 'faculty of language in the narrow sense' (FLN) as some abstract form of syntax characterized by recursive Merge. According to this article, only the trivial interpretation of 'biolinguistics' is correct. It does not make sense to define language in such a way that it excludes words. Words are human inventions and the necessary tools to give linguistic functionality to whatever biological capacities for recursive syntax we may have. Ultimately, this means that only 'lexicalist' versions of generative grammar can be correct. The agentive function assignment involved in the invention of words distinguishes language from bodily organs, which do not derive their functionality from human agency. More generally, cultural transparency of biological structures is rejected as an ideological form of Panglossian determinism and a denial of the "ceaseless creativity" and freedom coming with human agency.

*Keywords:* agentive functionality; culture; evolutionary psychology; externalism; lexicalism

## 1. Introduction

In this article, I will present a skeptical view of biolinguistics and linguistic internalism as currently conceived and advocate a return to the traditional idea that language is primarily a cultural phenomenon, even if firmly rooted in our biology. During the first half of the 20th century, according to the most common theories of language of those days, it was essentially a system of signs or symbols and sometimes also of rules. Such objects were seen as external to the individual human mind and as belonging to our socio-cultural reality. Language, in this

* The title of this article was inspired by Kauffman (2007).



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view, is a set of invented tools that enables us to give public expression to our inner feelings and thoughts. The classical expression of these ideas is Ferdinand de Saussure's conception of systems of signs in which signs have an external aspect (*signifiant*) and a conceptual aspect (*signifié*). Crucially, such signs could not be reduced to individual psychology, which was in accordance with Émile Durkheim's insight that methodological individualism would not do for certain social and cultural facts (Durkheim 1982 [1885]).[1] Related ideas are Ernst Cassirer's view of language as a system of symbolic forms, the insistence of Ludwig Wittgenstein on the public nature of rules and Karl Popper's claim that language is part of his supra-individual World 3.[2] The near consensus of those days, then, could be characterized, in current terminology, as 'externalist'.

Most of the time, adherents of this externalist view in no way denied the internal aspects of language or its being rooted in human biology. In fact, it is a truism that cultural objects are external to individual minds but are only what they are thanks to the *combination* of external object and mind-internal interpretation. As Rose *et al.* (1984, 282) put it: "The biological and the social are neither separable, nor antithetical, nor alternatives, but complementary".

Another aspect of the externalist consensus was the idea, going back to Herder, Von Humboldt and other early Romantics, that language plays a dominant role in determining the common culture of those who speak the language. Saussure's famous dictum that "*le signe linguistique est arbitraire*" not only has the uninteresting meaning that the same concept is expressed by English *tree* and French *arbre* but also the much more interesting implication that different languages differ in the way they divide our — possibly innate — conceptual 'space'. Thus, English has separate words for 'blue' and 'green', while other languages have only one word here. In other words, linguistic signs are conventional in two ways: in the arbitrary selection of their sound form (or visually-based equivalent) and in the way they organize our conceptual reality. Everyone who has ever translated a text is aware of this fact, even upon minimal reflection. The problems involved increase with cultural distance between languages.

The fact that language is thoroughly conventional (and often diverse), firmly places it in human culture, no matter on which individual-psychological and biological foundations it might also rest. Language, then, was seen as crucially involving external tools and as something cultural. In the United States, the cultural view of language, traceable to the same German-Romantic roots, was advocated by Franz Boas and his School. Of Boas's many well-known students, Edward Sapir was by far the most prominent linguist.[3]

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1 See Searle (1995) for modern elaborations of such ideas.

2 See Cassirer (1953), Popper (1972), and Wittgenstein (1953). A more recent variant of externalism makes use of the notion of 'memes' (Dawkins 1976; see also Dennett 1995 and Deacon 1997). Meant as a cultural counterpart to genes, it has not been developed into a notion of similar concreteness. On the contrary, 'memes' are so diverse and vaguely delineated that the concept is of little value. Moreover, like the elements of Popper's World 3, external elements are nothing except in relation to very poorly understood interpretation by our capacities for mental processing and understanding.

3 For the Boas School and the often subtle and complex ideas about the relation between biology and culture, see Degler (1991).

For many linguists these days, the Boas School is ignored because of its alleged tendency towards cultural relativism, particularly as found in the more extreme interpretations of the Sapir-Whorf hypothesis. Somehow, the cultural view of language is often seen as at variance with the idea of linguistic universals, which is seen as better served by a non-cultural, biologically-based view of language. However, there is no contradiction between a culturally-based view of language and the idea of universals. The issue of universalism vs. relativism is independent of the question whether language is primarily a cultural or a biological phenomenon. Cultural conventionalism can be based on an 'anything goes' philosophy, like behaviorism, or on the idea that cultural conventions are chosen from a narrowly constrained 'biological' hypothesis space. The latter view is equivalent to the 'universal toolkit' view discussed by Fitch, Hauser & Chomsky (2005: 203–204). The example they give is vowel systems: By convention, different languages have different vowel systems, but individual systems are selections from a limited, universal set of possibilities. Clearly, cultural conventions are constrained by our biology. To what degree is an empirical issue and a matter of debate. Also within the Boas School itself, a number of different views were expressed over time (see Degler 1991).

An interesting version of the cultural view is the one that was formulated by Sapir (1921): "[…] walking is an inherent, biological function of man" (p. 3), but "[…] speech is a non-instinctive, acquired, 'cultural' function" (p. 4). Clearly, however, this does not exclude biology for Sapir:

> Physiologically, speech is an overlaid function, or to be more precise, a group of overlaid functions. It gets what service it can out of organs and functions, nervous and muscular, that have come into being and are maintained for very different ends than its own.

Although "speech" is not the most suitable aspect of language to illustrate things further, it seems to me that Sapir's view is basically correct: Nothing biological is intrinsically linguistic. This goes against the currently popular forms of 'biolinguistics', according to which the faculty of language can be construed in a narrow enough sense so that it falls entirely within biology. What I have in mind is the faculty of language in the narrow sense (FLN) as described by Hauser, Chomsky & Fitch (2002).[4]

Not only Sapir, but also his European colleague Otto Jespersen saw language primarily as a cultural phenomenon. Jespersen is an interesting example because he has been mentioned by Chomsky as a precursor of the 'internalist' view of language that is so intimately connected with current biolinguistics (see, for instance, Chomsky 1986: 32). According to Jespersen, languages are manmade, cultural phenomena and artificial rather than natural in most respects. Jespersen took constructed international auxiliary languages, like Esperanto, very

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[4] The biolinguistics aspect was part of generative grammar since the beginning, as in Lenneberg (1967). It has been re-emphasized since Jenkins (2000) and Hauser, Chomsky & Fitch (2002). These writings also inspired my own renewed interest in the questions raised by 'biolinguistics'. My thesis of radical autonomy (see Koster 1987, particularly the preface and the last chapter) was an early rejection of the idea that syntax has an intrinsic linguistic function, independent of its application in a shared culture. This cultural perspective was further developed in Koster (1988, 1989).

seriously and considered them on a par with natural languages. He even went so far as inventing such a language himself. In the presentation of his language Novial, he makes the following comments (Jespersen 1928):

> People who hear about constructed languages will often say that such a language must be as lifeless as a dead herring, and that we may just as well think of setting up an homunculus made in a chemical retort and claiming for it the qualities of a living human being. Languages are not organisms, and their 'life' is not to be compared with that of animals or plants. Forty years ago Schuchardt was able to make short work of this objection by showing how much in the so-called natural languages was really artificial, that is, due to conscious endeavours and conscious selection, and yet was just as capable of 'living' as anything else.

What the examples of Sapir and Jespersen show is that it is possible to have a primarily cultural view of language without denying its biological or mind-internal foundations. In the cultural view, language is not the product of a language organ comparable to the heart but is the fruit of human agency. Accordingly, I would like to argue in this article that 'biolinguistics' as currently conceived is a problematic notion and that language is better characterized as *applied* biology, i.e. as a technology to create cultural products that serve as a bridge between our inner life and an external, shared symbolic world. Language, no matter how narrowly construed, only deserves that name if it has *both* internal-individual and external, supra-individual aspects. Eventually, I will conclude that not only the narrow, biological reconstruction of the notion 'language' is untenable, but also the strong 'internalist' paradigm on which it is based. I will further conclude that only the lexicalist conception of generative grammar (as developed roughly during the period 1970–1990) is compatible with what I see as the correct, cultural conception of language. Recent minimalist deviations from lexicalism are a step in the wrong direction and reminiscent of the flawed pre-lexicalist forms of generative grammar popular in the period 1955–1970.

## 2. Language: External or Internal?

It has been a prominent aspect of the forms of generative grammar emerging in the 1950s that the traditional word-based grammar and externalist paradigm was replaced by a syntax-based, internalist paradigm. According to Chomsky (1986, 19), the Saussurian views of language relegated syntax to '*parole*', where it was often left in limbo. It is true that Saussurian linguistics, although it was at the origin of modern phonology, contributed little to syntax. But from this fact, it does not immediately follow that the relative stagnation was due to a word-based view instead of a syntax-based view. At least since the Stoics, syntax has been seen as the realization of the properties of words.[5] So, nothing in a word-centered view of language precludes development of a syntactic component. It

[5] See Egli & Egli-Gerber (1992). Throughout this article, I use the term 'words' for ease of exposition. Strictly speaking, I often mean 'morphemes' when I refer to words. Words in the sense intended here also include functional elements, like Tense, and 'silent' words interpreted by context (see Kayne 2005).

can even be argued, as I will do later on, that the kind of syntax introduced by *Syntactic Structures* (Chomsky 1957) was mistaken precisely insofar as it deviated from the word-centered tradition. In this view, as I will argue later on in this article, *Aspects* (Chomsky 1965) was not the next revolutionary step forward but a partial return to the Stoic tradition of word-centered syntax. This would culminate in the lexicalist version of generative grammar that reigned between 1970 and the early 1990s.

Together with the technical elaborations of generative grammar, a general view of language was developed known as 'internalism' (see e.g. Chomsky 2000), as opposed to the traditional views that see language primarily in terms of collections of mind-external cultural objects. It must be emphasized from the outset that this traditional externalism should not be confused with standard meaning externalism as proposed by philosophers like Putnam (1975) or Burge (1997). What I am concerned with is the traditional insight that the mental is not limited to the brain. In no way am I committed to Putnam/Burge-style externalism or any other form of semantics that seeks to develop mental content in (partial) referential terms. Implicit in what follows is that what I call 'externalism' is an extension of what in said philosophical tradition is called 'internalism' and 'narrow mental content' (see Clark & Chalmers 1998 for discussion).

The more traditional linguistic views are said (by Chomsky) to be concerned with 'E-language', while Chomsky's own concerns are with the internalist reconstruction of the concept of language referred to as 'I-language'. Since language, construed in a sufficiently narrow sense, is seen as a property of the human mind, a further point of interest is Chomsky's idea that such properties of the mind are in fact properties of the brain described at a certain level of abstraction. This is supposed to be not unlike 19th-century talk about 'chemical valence', which was eventually leading to theories about the underlying physical mechanisms. This partial equation of mind and brain about language, expressed by the neologism mind/brain, is characteristic for the current internalist paradigm. Since theories about I-language are ultimately about the brain — a biological object — at least part of linguistics can be seen in this view as a form of theoretical biology. Hence the program referred to as bio-linguistics.

Let us be somewhat more explicit about I-language. Chomsky (1986: 21) refers to Jespersen, who claimed that there is "some notion of structure" in the mind of the speaker.[6] According to Chomsky, I-language is a distinct system of the mind/brain that grows in the individual from an initial state $S_0$ to a stable state $S_s$. This growth, comparable to the growth of an organ, only involves minimal external factors, such as those that help set the parameters that distinguish the grammars of different languages. In order to counter the obvious objection that language also involves external elements, Chomsky makes a distinction between the 'faculty of language in the narrow sense' (FLN) and the 'faculty of lang-

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[6] Chomsky (1986: 32) suggests a rift between 'Saussurian linguistics' and Otto Jespersen's emphasis on our capacity for 'free expressions'. I do not think this opposition makes sense. On the one hand, nothing in Saussure's *parole* excludes free expressions and, on the other hand, Otto Jespersen firmly believed in the non-natural, artificial character of languages. That Jespersen saw languages as cultural creations also appears from his interest in designed international auxiliary languages (see, for instance, Jespersen 1928).

uage in the broad sense' (FLB). The notion of I-language particularly applies to FLN, which has recursion as its core property (Hauser, Chomsky & Fitch 2002).

I believe this idealization to FLN is problematic and that, therefore, the objection to I-language still stands. More generally, I believe that no coherent notion of I-language is possible and that the partial equation of mind and brain is highly problematic. Just to avoid misunderstanding, I am not at all denying the existence of brain-internal computational structures used in language or even their innate character. Poverty-of-the-stimulus arguments sufficiently show that language involves innate structures of some kind. Given the varied pace and fashions with which our various skills and forms of knowledge are acquired, these innate structures are not general, as the behaviorists liked to see it, but specific to various degrees. As a matter of fact, these observations are rather trivial, since *all* our mental capacities are based on innate structure. My argument is not against innateness but against the idea that biological structures are transparent with respect to their cultural functions, including their role in language.

In the next section, I will argue that the point can be generalized to all form–function relations, at all levels of biology: Since there is no intrinsic form–function relationship, successful correlations can only be maintained by some kind of memory. For biological structures like our organs, form–function relations are mainly, but not exclusively, preserved by DNA. For cultural phenomena like language, form–function correlations are mostly maintained by our cultural record. A cultural record is not a property of any individual in particular but can be seen as a shared, external memory.

At this point, it is absolutely crucial for my argument to appreciate that words are man-made, public cultural objects and that nothing biological is properly called 'linguistic' in abstraction from words. Since the hypothesized initial state $S_0$ of I-language is wordless, it has nothing to do with language, no matter how innate it is and no matter whether it will be exclusively used later on for language or not. As Otto Jespersen, quoted at the beginning of this article, recognized, human languages are called 'natural' but are in fact artificial. They can be created by communities over long periods of time, like English and Dutch, or they can be designed by one or more people in a relatively short time, like Esperanto, Volapük, or Novial.

Reading Hauser, Chomsky & Fitch (2002), one gets the impression of an argument about humans that sounds like the equivalent of a discourse about fish without ever mentioning the fact that they swim in water. Humans are different from animals in that they live in a world not just of culture (like apes to some very minimal extent) but of *symbolic* culture. This symbolic culture functions as a supra-individual, external memory. As Donald (1991) put it, humans live in *symbiosis* with this shared, external memory. The shared, external memory is not only man-made but also preserves the functions that we, as agents, have assigned to certain structures of our brain. I agree with Donald (2000) that cognitive science is bound to remain sterile if it continues the solipsistic assumption of "the myth of the isolated mind" and by being in denial about the symbiotic nature of human cognition.[7]

[7] See also Clark & Chalmers (1998) for similar ideas and Muysken (2002) for a critique of I-

There is perhaps even reason for fundamental skepticism in general when talking about external vs. internal with respect to the human mind. It is far from clear how these terms should be interpreted. Like most intelligent systems, the human mind relies on structures for processing or interpretation on the one hand and on data structures stored in memory on the other hand. If we disregard the mind-external processing done by our computers, we can say that processing is done 'internally', in individual brains, but data structures in memories are not private in the same sense. This is the core of the problem because nobody limits the notion 'mind' to processing and interpretation. *Both* processing and stored data structures are necessary conditions for us to coherently speak about 'mind'. But, as Clark & Chalmers (1998, 7) put it, "[w]here does the mind stop and the rest of the world begin"? Thus, if you want the text of the Dutch national anthem, you can ask me or look it up on the internet. I predict more successful retrieval via the internet, but that can change from one moment to the next. Our cultural memory is stored in, and distributed over brains, including my own, and over libraries and other collections of media. The same is true for words and other linguistic expressions. It would be absurd to say that I remain within the confinements of I-language when I produce or understand a sentence exclusively with words from my own memory, but that I embark on a short excursion to E-language if I use a dictionary for one word or another in the middle of a sentence.

More generally, one can say of course that if words or the text of the Dutch national anthem are stored in my brain they are 'internal' and when they are found in a dictionary or stored on some hard disk of a web server they are 'external', but such a distinction would be insignificant. The more interesting distinction is between private processing and interpretation, leading to conscious understanding among other things, and the data structures interchangeably distributed over individual brains and other media. If we like to insist on the terms 'internal' and 'external', it would make sense perhaps to call individual processing and interpretation 'internal' and memory content, including content stored in my own brain, 'external'. But the distinction thus construed completely undermines the partial equation of 'mind' and 'brain'. My brain is contained within my skull, but luckily my mind does not stop at the bony borders of the brain's confinement. Unlike what makes sense for the brain, the distinction between "within the skull" and "outside the skull" is completely meaningless for the mind. The boundaries between my onboard memory content and the content found in other brains and media are fluid and ever changing. Since the mind is not limited the same way the brain is, it is questionable whether talk about the mind is talk about the brain at some level of abstraction. I therefore reject the notion mind/brain.[8]

If my reconstruction of the notions 'internal' and 'external' is correct, the distinction between I-language and E-language also loses its significance. Under any meaningful definition, a language minimally contains words. Words are man-made cultural objects (no matter how many biological constraints there are

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language.

8 The equation of brain and mind is also problematic in the other direction, as many brain functions, like regulating respiration, etc., have nothing to do with what we call mind.

on possible words) and they do not belong to any particular individual but are external in the sense that they are stored in media, in the memory parts of brains, in dictionaries and other books, etc. In that sense, words are E-elements that belong to a speech community. If I die, the Dutch language will in all likelihood survive.

Of course, a language can only exist thanks to individual capacities for processing and interpretation, but that is true for all cultural phenomena. A painting by Rembrandt is only a painting for humans, not for cats and dogs, and it involves innate and unconscious forms of processing, like the ability to interpret two-dimensional images as representations of things three-dimensional. But that should not lead us into making a distinction between E-paintings and I-paintings or paintings in the broad sense and paintings in the narrow sense. All cultural objects exploit our abstract cognitive capacities, but at the neuro-biological level, in abstraction of our common culture, these capacities do not assign labels like 'painting' or 'linguistic expression'. There certainly are biological aspects to what makes painting or language possible, but both painting and language only exist if another necessary condition is fulfilled, namely the presence of a man-made, external and supra-individual cultural environment.

It is crucial for my argument that words are supra-individual, external elements of our shared cultural record. If words would have a strictly individual, internal counterpart, the internalist view of language could perhaps be saved in some weak sense. Although there have been attempts to see words as labels for a fixed and universal repertoire of concepts or feature complexes, such attempts are ill-advised.[9] As we have seen in our brief discussion of Saussure, words do not form a nomenclature for fixed concepts but divide up conceptual reality in many different ways, no doubt following biological constraints but ultimately with conventions that differ from language to language. Thus, not only in their sound form but also in their modes of significance most words thoroughly involve conventions. Conventions do not belong to our biology, but, once more, to our external, shared cultural record. I will return to meaning externalism (again, to be distinguished from Putnam/Burge-style externalism) in section 4.

## 3. Form, Function, and Reductionism

Other than what is found in physics and chemistry, biology and human culture

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[9] Chomsky (2007: 4) says the following in this regard: "In addition to Merge applicable without bounds, UG must at least provide atomic elements, lexical items LI, each a structured array of properties (features) to which Merge and other operations apply to form expressions". Since UG is the initial state of the language organ, it is claimed here that children are born with abstract lexical elements, perhaps as precursors of normal lexical items.

I cannot make sense of this claim, as normal lexical items are based on various public conventions that establish complex, partially language-specific and poorly understood relations with our innate conceptual potential (whatever that may be). Even if there are precursors of lexical items, due to the conventional choices made by languages, there can be no one-to-one relation between normal, public lexical items and the postulated innate elements (neither in their atomic form nor in complex form as the result of Merge). In fact, there is no evidence at all that something even remotely like lexical items is part of the initial state of some language faculty.

are characterized by rich patterns of form–function correlations. In considering form–function patterns, it is important to realize that there is no such a thing as an intrinsic function of a physical structure. Functionality is a relational concept: A functional structure is always functional with respect to something *external* to that structure. The emergence of form–function relationships in nature is an opportunistic process, as it is heavily constrained but not determined by physical law. In the terminology of Jacob (1982), it is a form of "tinkering". As was realized by Darwin, evolution assigns functions not only to available material without previous function, but also to structures that were originally adapted to some other function. In the latter case, biologists used to speak about 'pre-adaptation', now more commonly referred to as 'exaptation' (after Gould & Vrba 1982). A famous example are the wings of birds, currently adapted to flight but originally evolved as flaps for thermoregulation.

Evolutionary adaptation creates the false, Panglossian illusion that structures are functionally transparent. The many exaptations in the history of life dramatically illustrate that there is never an intrinsic relation between form and function. Even if structures are magnificently adapted to function A, the very same structures can be used for function B under an appropriate change of external conditions. Consider a clear and spectacular example of adaptation: the various teeth forms found in mammals. Carnivores often have huge canines compared to the modest counterparts of herbivores. A biologist can tell from the shape of teeth in which kind of environment an animal lives. This falsely suggests intrinsic functional transparency. There only is transparency in relation to certain external environments and the biologist knows those environments, hence the illusion of transparency. For a physicist from Mars, with no knowledge about habitats and life styles, the various dental shapes of mammals would be a complete mystery.

From the vantage point of physics, then, the relation between form and function follows physical constraints, but is otherwise as arbitrary as the relation between *signifiant* and *signifié* in linguistics. The set of functions that can be fulfilled by a given structure is potentially as infinite as the set of possible environments and Kauffman (2007: 911) rightly observes "that the biosphere and human culture are ceaselessly creative in ways that are fundamentally unpredictable and presumably non-algorithmic or machinelike". Since the relation between form and function cannot be predicted by physical law, biology is essentially historic, in spite of the fact that it is narrowly constrained by the laws of physics. In practice, this means that successful form–function relations can only be preserved thanks to memories. There are various kinds of memory in the living world (see Jablonka & Lamb 2005), but for the biosphere as a whole, DNA is by far the most important memory type. There is a direct link between the emergence of memory molecules and the non-deterministic, 'historical' nature of form–function relationships in living organisms.

As the set of possible functions of physical structures cannot be predicted by physical law, the idea that biology can be reduced to physics or chemistry is an illusion. This insight goes back to Aristotle and in the long tradition of opposition against his concept of a *causa finalis* next to other forms of causation, it has been stated numerous times that description of biological functions can be translated into normal physical talk about cause and effect.[10] Kauffman (2007: 911) convincingly articulates the view that this misses the point:

> Asked what the function of the heart is, Darwin would have replied, "To pump blood". That is, the causal consequence of the heart, by virtue of which it was selected by natural selection, is pumping blood. But the heart also makes heart sounds. These are not the function of the heart. Thus, the function of the heart is a subset of its causal consequences and must be analyzed in the context of the whole organism in its selective environment. Again, this says that biology cannot be reduced to physics, for while the string theorist might (actually could not) deduce all the properties of a given heart, he/she would have no way to pick out as the relevant property that of pumping blood. But it is that property that accounts for the existence of hearts in the biosphere.

The "ceaseless, unpredictable creativity" of the biosphere, with its fruits preserved via DNA, is a very slow process, with non-agentive natural selection as the driving force. In spite of millennia of primitivist propaganda against the obvious, the emergence of the human mind in the biosphere is an event in magnitude comparable to the emergence of life itself. It involved the introduction in the biosphere of agentive creation of form–function relations and a new type of memory to preserve the fruits of its creativity. This memory, of course, is the shared, supra-individual symbolic memory implied by our culture, as discussed in section 1. Animals certainly show forms of agentive function assignment, such as when chimpanzees use stones to crack nuts. However, to the extent this leads to traditions, these traditions are passed on by imitative behavior, not by symbolic means (cf. Jablonka & Lamb 2002: chap. 5). Humans are unique in that their agentive function assignments take place in relation to a shared symbol-based memory.

The distinction between agentive and non-agentive functionality has been extensively discussed by Searle (1995: 20). It is directly relevant for an understanding of cultural creativity as intended in this article. Cultural functionality involves human decisions and is therefore agentive, for instance when we use a stone as a paperweight. This illustrates once more the fact that functionality can be assigned to arbitrary objects that meet certain physical constraints (a boulder would not have the right size and weight). The functionality of the heart and other organs, in contrast, is assigned in a very different way, namely completely independent of human interference. It is essentially a genetically driven, fully automatized biological process. Interestingly, one and the same organ can be functional in an agentive and in a non-agentive way. The lungs, for instance, function non-agentively in respiration and agentively when we are making music on wind instruments. In normal usage, respiration is seen as part of our biology, while the playing of trumpets or clarinets is seen as part of our culture.

Is the use of our capacity for recursion, as manifest in language, more like the lungs in respiration or like the lungs in the creation of wind music? Since words are man-made instruments, the use of our capacity for recursion in language sides with lung function in wind music: in both cases, unlike what we see in respiration, a biological structure is made functional the agentive way, i.e.

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[10] See Ruse (2003: 17–19).

by human invention. Referring to the capacity for recursion as "the faculty of language in the narrow sense" is just as odd as to refer to lung capacity as "the faculty of trumpet playing in the narrow sense". Such usage would be Panglossian, erroneously suggesting that physical structures can have an intrinsic cultural function. As the example of the lungs shows, innateness of structure is not the issue here: the same genetically determined biological structure is used in a non-agentive way (respiration) in one case and in an agentive way (trumpet playing) in another case.

This is, of course, not to say that language is acquired in the same way as how we acquire musical skills. All our cultural activities utilize human inventions to exploit our innate capacities and all differ among each other in the degree to which genetic factors are involved. Thus, riding a bike and driving a car both use invented mechanisms to give new functions to some of our biological capacities. Nevertheless, riding bikes is already done by kids, often without much instruction. Driver's education, in contrast, is most fruitfully started later in life, often with long trajectories of instruction and with varying degrees of success. Given our biological capacities, some cultural activities are more accessible and attainable than others.

Language might be on one extreme of this spectrum, things like theoretical physics on the other extreme. Given the enormous advantage of language, it is even likely that its invention has stimulated a form of co-evolution of culture and brain structure, making the use of language the most readily accessible of all cultural activities.[11] But this possibly extreme genetic facilitation in no way alters the basic logic of the situation: the structures involved in language thank their functionality to a human invention, unlike the functionality of organs like the kidneys or the heart.

As to their variety, the properties of agentive function assignment are very much like what we find in organic evolution. Thus, we can agentively give a function to a functionless object, as when we use a stone as a paperweight. Human culture is also full of (the agentive equivalent of) exaptations, namely when we give a new function to an existing functional object. Human tinkering is full of exaptations, for instance, when we use beer cans as car wheels for model cars or wooden shoes as sailing dinghies. The creative fruits of agentive function assignment are just as unpredictable and potentially infinite as the possible exaptations in non-agentive evolution. There is no law-like, deterministic relationship between our biologically given properties and the environments we can invent to make our potential functional. Therefore, just as biology cannot be reduced to physics, human culture cannot be reduced to biology (or physics). In both cases,

[11] See Deacon (1997) and Jablonka & Lamb (2005) for the idea of brain–language co-evolution. Many innateness issues have been discussed in relation to the sounds of speech (for instance, as in Mehler *et al*. 1988) or even similar elements of sign language (Petitto 2005). It is very well possible, and even likely, that rapid and smooth access to such elements evolved to facilitate its use in language. But note that there is nothing inherently linguistic about speech sound. Speech is not even a necessary condition for language, as in many cases the public aspect of language is not represented by speech sounds (or signs) but by written words or print. What is necessary for language is an external, publicly accessible medium. Speech happens to be such a medium, but not a necessary one. Writing, an everyday form of language use for many, is not biologically facilitated the way speaking is.

function assignment is arbitrary as seen from 'below'. It always is the higher level (in the order culture > biology > physics) that determines the functionality of structures at a lower level.

## 4. Agentive Functionality and Word Meaning

A less lofty term for 'agentive functionality' is the notion 'application' we use in daily life, particularly in technological contexts. The logic of application might seem trivial, it is a key factor in human creativity. In order to understand the role of 'application' in word semantics, it is useful to highlight some features of applications in general. As an example, consider electric motors. Electric motors are themselves applications of physical principles producing something as abstract as an automated rotating movement. Such rotating movements are like mathematical structures in that they are not intrinsically functional. Therefore, human technology has developed numerous secondary applications, in which electric motors are made functional 'from outside', by inventing new contexts for use. These applications range from coffee grinders to electrical toothbrushes to the engines of locomotives. Like in all cases of agentive function assignment, the possible relations between an electrical motor and its applications are:

(1) a. infinite
b. unpredictable
c. contextual
d. constrained
e. partially conventional, partially innovative

The infinity and unpredictability of applications (1a–b) follow from their contextuality (1c). Essentially, novel applications involve new functional contexts. Thus, in order to use an electric motor for a coffee grinder, there must be a context in which coffee exists together with invented techniques to brew it, etc. It is clear that all future contexts and technologies cannot be predicted. Furthermore, there are numerous physical constraints on the application of devices in real life situations (1d). Electric motors, for instance, cannot be used under water without protective insulation. Size is a very important factor in agentive function assignment to technical devices. Earlier on, I gave the example that boulders are not suitable as paperweights. For comparable reasons, windmills are excellent power sources for water pumps, but less so for toothbrushes. Last but not least, applications can be innovative or conventional (1e). Successful applications become part of the cultural record, so that not each generation has to reinvent the wheel. The properties of agentive function assignment as listed in (1) are the key to understanding the semantic functioning of words. But first I will make a few critical remarks about the naming paradigm, which has been the curse of lexical semantics.

Although both Saussure and Wittgenstein have taught us that words do not form a nomenclature for things in the world or in the mind, the naming paradigm seems almost insurmountable in common sense thinking about language. It is implicit in 'Fregean', referential approaches to semantics ('externalism' in the other, philosophical tradition), and from Descartes and Locke to Katz and Fodor (1963), it has been thought that words stand for 'ideas', 'concepts' or 'meanings'. These ideas are situated either in some abstract realm (Platonism) or in the mind/brain (Rationalism and Empiricism).

According to the approach in question, what the French word *arbre* and the English word *tree* have in common is that they name the same concept, namely TREE. Sometimes the concepts named are not specified as words in capital letters but as feature complexes. All such approaches beg the question because everything that is mysterious and in need of clarification about the word *tree* is mysterious and in need of clarification about TREE and in exactly the same way. Capital letters and feature notations only disguise the fact that words are explicated this way in terms of other words. Such verbal explications do not give meanings but hints about how to use words. Paraphrases are only useful if the periphrastic elements are already known to those for whom the paraphrase is intended to clarify things. Thus, if we explicate 'pork' as MEAT FROM PIGS, something is clarified only for those who already know what pigs are. Personally, I learned the word 'pork' before I learned the word 'pig', namely by direct acquaintance with a kind of meat with a kind of color and taste (see Koster 1990). Paraphrases do not specify THE meaning of a word but are one way, among many, to learn something (but by no means all) about the usage of a word.

The strongest refutation of the naming paradigm comes from the traditional notion of polysemy.[12] Consider the varied use of an ordinary noun like *book* (some of them traditional observations):

(2) a. The book weighs a pound.
b. The book is exciting.
c. The book fits on a 256MB memory stick.
d. The book only exists in her head.
e. The book is his main income.

In all these cases, the word *book* corresponds with a different concept, leading to corresponding variation in possible reference. This suggests that what corresponds to words like *book* is not one concept BOOK, but an infinite set of concepts, depending on context. In (2a), for instance, *book* is used to refer to a physical object, while in (2e) it refers to generated revenue. Even proper names, which might be considered the 'flagships' of the naming paradigm, behave in this fashion:

(3) a. Schubert is difficult.
b. Schubert will take 30 pages.
c. Schubert is for sale.
d. Schubert will be reburied next year.
e. Schubert can be downloaded everywhere.
f. Schubert will be burned on request.

12 Polysemy was emphasized in a Dutch tradition represented by Reichling (1935) and Uhlenbeck (1973). I first got acquainted with examples like those given in (1) and (2) in Reichling's class lectures in the early 1960s. Very similar examples are used in Chomsky (2000: chap. 5).

Suppose these sentences are about Schubert the composer. Even then it is impossible to say without further context whether (3a) is about playing his music or about his character. And in (3e) and (3f), *Schubert* refers to what it can refer to only thanks to the context provided by recent inventions. These sentences (and what 'Schubert' refers to) are perfectly intelligible in 2009, but would have been a riddle only 10 years ago. In short, as has often been observed, names cannot only refer to persons but, in the right context, also to anything related to those persons. This comes down to an infinite set of concepts and an infinite set of potential referents. Like exaptations in evolution and the technological applications of hardware, the interpretation of words is a case of "ceaseless, unpredictable creativity".[13]

Word interpretation has the properties listed in (1), which suggests that we are talking about forms of agentive functionality: Limiting ourselves here to common nouns and proper names, what is stored in association with such words in our brain is not meanings or concepts but something that only becomes meaningful from outside, i.e. by the agency of a human interpreter operating in a given context. *Concepts (and extensions) are not properties of words but properties of interpretations of words*. But if what the various uses of words like *book* or *Schubert* have in common cannot be a concept or a meaning, what else could it be? What can be stored in the brain in association with words must be physical, i.e. something representable by neural circuitry or other material properties making up the memory banks of the brain. In short, words must be associated with coded information in the brain, not with meaning, because all known physical structures are without inherent meaning.

It is the hallmark of coded information that it only is 'something' in relation to an interpreting, external environment. DNA, for instance, only is what it is thanks to its functioning in the chemical environment provided by living cells. The information stored on CDs is not inherently audio or video, but only in combination with the right electronic output devices. Something similar is true, I suppose, for the coded information associated with words in the brain. Following this line of thought, we can furthermore assume that what the various uses of *book* or *Schubert* have in common is not some kind of core meaning, but coded information about *books* and *Schubert*, respectively. In that sense, words are addresses of information clusters of unknown but presumably considerable size. It is impossible and unnecessary that these information clusters are the same for each person. Some people have stored enough information to distinguish an elm from a beech, others have not, as was pointed out by Putnam (1975). A certain "linguistic division of labor" is an obvious fact of life and successful communication is possible because our information banks and interpretive skills overlap to various degrees.

Note, incidentally, that as before, data structures are not individual-psychological or internal in any strict sense. The information associated with the words of a language is distributed over all the speakers of a language plus what

[13] The kind of (non-rule-governed) creativity shown by polysemy is related to the Cartesian creative aspect of language use discussed by Chomsky (1966). See also Chomsky (2000: 128) and the references to Pustejovsky (1993), Moravcsik (1990), and the Aristotelian origin of polysemy and related forms of creativity.

we find in books and in other media. When we produce or understand sentences we can use our 'onboard' data banks, but we do not step out of language when we let ourselves occasionally be assisted by a dictionary. The informational basis of meaning is essentially external in this sense. What is mostly internal is our capacity for processing and interpretation.

The information associated with words must include coded instructions for what must be seen as standard applications. The meaningful use of words is not permanently innovative but largely conventional, where, as mentioned above, different languages do not apply exactly the same conventions. As inter-personal agreements, conventions are not individual-psychological or biological, but part of the culture of a community. This is another reason why an exclusively internalist approach to meaning does not work. There is no meaning without a living individual's capacities for processing and interpretation, but there is no meaning either without supra-individually distributed information and conventions. That the use of words is partially conventional corresponds with property (1e) of what we see for agentive function assignment in general. As in the case of electric motors, partial conventionality by no means refutes the potential open-endedness of application. The distinction was appropriately described by the great 19th-century linguist Hermann Paul (1880 [1975: 103]), who was speaking of the "usual" and "occasional" application of words.

It is unlikely that the information involved in the interpretation of words is completely specified for each word individually. If we look at coded information and its decoding devices in technological contexts, it is always the case that the decoding devices add information of their own. The speed of music, for instance, is not coded on records or CDs but depends on the speed of the rotating parts of the decoding devices, such as turntables or the corresponding parts of CD players. In the case of the information stored for words, we do not know how much information is stored for the word itself and how much information is added by the interpretive mechanisms. But the high plausibility of the division of labor in question further undermines the idea that meanings or concepts are directly found in the brain as stored properties of words. Concepts only exist as the results of agentive function assignment, as created elements involving three factors: the information stored for the word itself, the contributions made by our (possibly innate) capacities of processing and interpretation, and, last but not least, the context of use.

An example of what might be a general aspect of the meaning of words dependent on interpretive capacities is what I would like to call 'the Platonic residue' of words. No matter what one thinks of Platonic universals in general, a minimal notion of universals seems unavoidable, namely the types of the type-token distinction made since Charles Sanders Peirce. It is just a fact that we think in terms of types, not tokens. Thus, when we find a concrete book (the token) somewhere we see it as an instantiation of the general notion of a 'book' (the type). What is fascinating is that the type–token distinction applies to all cases where we use words for things, even things just invented, like iPods. Each individual iPod is seen as an instance (token) of the general notion 'iPod' (type). Given its generality, it is unlikely that the type–token distinction is part of the stored informational make-up of each individual word. That would mean that even essential parts of the meaning of certain words (like being a type) are not represented as individual properties of those words. As a matter of fact, universals (minimally: types) are completely beyond the scope of naturalistic inquiry, as the physical world is populated by particulars (tokens) and not by universals (types); see Koster (2005a).

Another general addition of the process that interprets coded word information is awareness, which eventually will contribute to subjectively experienced understanding. Presumably, the traditional idea of naming-based word meaning has been so irresistible over the centuries because meaning is what we seem to be aware of when we think of words via introspection. In other words, introspection creates the 'optical' illusion that meanings are properties of words rather than of interpretations of words. When we think of words, the associated 'dead' information is brought to life by the interpretive process. It implicitly adds the usual ingredients of interpretation, such as virtual contexts, the Platonic residue and other general elements, thereby obscuring the fact that what is actually stored for individual words is coded information. This coded information is not directly accessible at all. Interpreting it by introspection is a form of use, it adds interpretive information as in other forms of use and it creates the actual meaning in the process.

Word meaning, then, at least for the common nouns and proper names we discussed, is another example of "ceaseless, unpredictable creativity" and as such entirely comparable to what we see in the applications of technical devices like electric motors. This is the case because in both domains the creativity is based on agentive function assignment and has the properties listed in (1). The fruits of successful agentive function assignment are usually stored in our cultural memory, in this case accounting for the conventional aspects of word interpretations. As in the case of technological applications, this will not prevent us occasionally from applying the information associated with words in innovative ways in new contexts. That the word *Schubert* can, in recent times, refer to something that can be downloaded depends on novel contexts that were completely unpredictable in the composer's days.

The way we create concepts, by interpreting the coded information associated with words, is as far as I can tell unique for humans and at least as revolutionary and essential for language as the use of recursive syntax. As we saw earlier on, chimpanzees and other animals know certain forms of agentive function assignment which are mainly preserved as 'cultural traditions' by imitative behavior. What is unknown in the worlds of animals is the agentive function assignment to the information complexes associated with words and other signs. As taught to us by Saussure, the life of signs goes way beyond the individual, is conventional in many essential respects and therefore irreducibly social-cultural. As before, this social-cultural view does not exclude the biologically-based interpretive capacities of the individual, but simply says that these capacities only lead to meaning by the *combination* of internal mechanisms and external data structures. Neither meaning nor mind would exist without supra-individual, shareable information structures.

The conventional aspect of words lies not only in the arbitrary choice a culture makes of outer forms (*tree* vs. *arbre* etc.). As was mentioned before, it also shows up in the way words help us to conceptualize reality. Thus, it is partially a matter of convention how languages divide the color spectrum. According to Saussure, a sign always gets its value in relation to how it contrasts with other signs of the same system. It is therefore misleading to say that apes have a primitive form of the concept of 'ownership' (as suggested by Fitch, Hauser & Chomsky 2005; see Koster 2005b for a critique). Showing more or less consistent possessive behavior with respect to objects across a variety of contexts is something very different from having a primitive form of the human concept of 'ownership'. What is shown in the animal behavior is about some necessary conditions at best. As discussed, human concepts are the result of an interpretive process which involves agentive function assignment to information complexes. These information complexes are distributed over a culture and the creative interpretive processes associated with a word like *ownership* may lead to an, in principle, infinite variety of concepts (polysemy). Application of the word information to forms of possessiveness is a possible choice governed by implicit conventions. These conventions constitute a semantic field, among other things, in which *ownership* is contrasted with other words that also apply to possessive behavior, such as *borrowing*, *leasing*, and *renting* etc. There is zero evidence among chimpanzees for either the endless polysemy of interpretation or for the necessary conventionality involved in semantic fields.

Another essential aspect of human word meanings so far not attested among animals is what I called 'the Platonic residue', i.e. the fact that the human mind takes 'ownership' as a universal (a type), of which observed possessiveness can be a token. All in all, it seems to me that it is entirely misleading to say that animals (particularly chimpanzees) have anything coming even close to human concepts. Whatever biological capacities enabled humans to invent and interpret words and preserve them in their cultural, supra-individual memory is truly revolutionary. Invented words and the associated astonishing capacity for conceptual creativity in forming bridges between our inner life and the external realm shared by a community is the basis of language. I therefore strongly disagree with the idea that human language is primarily based on our capacity for recursion. The essence of linguistic functionality is the giving of an outer form to our inner ('conceptual-intentional') life. Signs have this capacity independently of syntax, as is shown for instance by traffic signs. Recursive syntax, in contrast, has no linguistic functionality whatsoever independent of linguistic signs (morphemes, words). Recursive syntax is no doubt an extremely powerful addition to what we do with words, but we should remember that it only makes complex signs of simpler signs. Language construed in the narrowest way possible is about words (or morphemes) and their creative use. Recursive syntax gives a tremendous boost to this creativity, but clearly is a secondary broadening of the primary role played by words given in our culture.

## 5. A Revisionist Sketch of the Recent History of Linguistics

My conclusion so far is that linguistics is only indirectly about biology and that it primarily is the study of simple cultural objects (morphemes and words) and complex cultural objects (sentences). These great human inventions *apply* our biological capacities, meaning that language is a technology, with functionality closer to how the lungs function, say, in cultural activities like trumpet playing rather than in purely biological contexts as in respiration. Linguistics seen this way entails a partial return to a Saussurian, sign-based view in lieu of a syntax-based view, without denying the enormous importance of our capacity for recursion. The latter is no doubt biologically based and in that sense the Chomskyan view was a much needed correction in the direction of a more balanced view. However, generative grammar of the last 50 years disturbed the balance in the opposite direction, developing large-scale denial about the essentially cultural nature of language. Recent conceptions of 'biolinguistics', together with a minimalist practice more and more degenerating into the latest descriptive technology, make it necessary in my view to rethink the foundations of linguistics and to try to achieve a synthesis between the traditional Saussurian ideas and the more recent Chomskyan perspective.

From this vantage point, unfortunately, the history of generative linguistics is not a continuing story of success after success. In fact, we cannot entirely avoid criticizing the inflated, sometimes somewhat self-congratulatory rhetorical style of our field. Once upon a time, *Aspects* (Chomsky 1965) was hailed by some as 'the New Testament', which was supposed to supersede 'the Old Testament' of *Syntactic Structures* (Chomsky 1957). This allegedly led to the innovative lexicalism of *Remarks on Nominalizations* (Chomsky 1970), which was the beginning of the second great revolution in linguistics, the Principles-and-Parameters framework of *Lectures on Government and Binding* (Chomsky 1981), which was sometimes said to be the first 'construction-free' theory in the more than 2,000-year history of the field.[14] With equal revolutionary pathos and enthusiasm, many adopt Minimalism since the late 1980s, dismissing X-bar theory and fruitful notions like government and binding almost without discussion.

I reject this narrow view of what happened and think we can still learn much from the more than 2,000-year history of the field. Before going into that, I would first like to emphasize that linguistics is a much livelier and richer field than 50 years ago. The changes since the 1950s led to more or less uniform terminology, representational techniques and methodology involving many more languages and linguists than before and from all over the world. In terms of descriptive richness and partial insights, the field has truly exploded in recent times. The connection to theories about human nature added much to the appeal of modern linguistics, and the victory over behaviorism was definitive, not only in the US but also in Europe and Asia, where behaviorists were a rare species to begin with. In spite of all these positive developments, however, I believe that conceptual-theoretical progress has been minimal in the last few decades. Why is this the case?

Seen from my (partial) Saussurian vantage point, the history of linguistics of the last 50 years is not a continuing revolutionary success story, but a story of the rise and fall of the appreciation of what I see as the correct perspective, namely lexicalism. The synthesis of Saussurean and Chomskyan ideas that I

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[14] This is not true, as pre-Chomskyan X-bar theory is construction-independent.

advocate is only compatible with the lexicalist versions that dominated generative grammar roughly between 1970 and 1990. This means that I reject both the non-lexicalist (or less lexicalist) versions of early generative grammar (c. 1955-1965) and the partial return to those in minimalist practice (after 1990). But even of the lexicalist period, I reject the almost exclusive internalist emphasis and the non-lexicalist residue of earlier theories, namely 'Move α' (not to speak about the even more dubious covert movement supposedly leading to Logical Form).[15]

In retrospect, then, *Syntactic Structures* was not in all respects a revolutionary improvement over what was historically seen as syntax, but an ill-advised denial of what was grammatical wisdom since the Stoics, namely that syntactic structures are properties of words. The problem was caused by adopting models for natural languages that were derived from studying the artificial languages of logic and mathematics. Looking back, the use of mathematics and of elements from recursive function theory was more successful from a propagandistic point of view than from an empirical and theoretical point of view. It invested linguistics with the false prestige and illusion that it was a very sophisticated discipline, a beacon for the humanities in its transition to mathematically based science. As every linguist must be aware of in 2009, sophisticated mathematics does not play any role whatsoever in syntactic theorizing.

In *Syntactic Structures*, grammars are introduced that replace the terminal symbols of context-free artificial languages with words from English. This kind of move is based on superficial similarities between natural languages and the artificial languages in question and it did not lead to much new insight. The reason is that the words of natural language are signs with rich internal properties, while the terminal symbols of the artificial languages had no intrinsic properties whatsoever. At best, one could say that their combinatorial properties were implicitly defined by the phrase-structure rules in which they figured.

The most important characteristic of words of real human languages is that their potential syntactic environments are among their properties in abstraction from any rules or other word-independent computational devices. Thus, somebody who knows English knows that the word *book* can be preceded by an article: *the book*. This is public knowledge: One can find it in reference grammars and if English were part of a culture with an oral tradition only, every native speaker could confirm it. Similarly, every native speaker of English knows that the verb *like* can have a subject and an object: *John likes Mary*. In fact, then, the basic structure of a sentence can be seen as a property of the verb. That a verb 'projects' arguments is such an obvious fact that it has been known since Antiquity: it was known to the Stoics, to the medieval modists and to the structuralists, who called it 'valency'.[16] *Syntactic Structures* must be unique in a grammatical tradition of over 2,000 years in that it introduces syntactic structure mostly in abstraction of the projecting properties of words. It was a fundamental error, directly traceable to the idea that natural language grammars must be modeled after certain formal languages.

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15 See Koster (1987) for a critique of 'Move α' and levels such as Logical Form.

16 See Tesnière (1959). A related approach can be found in the German notion *Rektion* ('government'), which has a history of several centuries.

Fortunately, the error was corrected in *Aspects* by the introduction of a lexicon. This lexicon contained subcategorization frames to account for the valency of verbs, among other things. In all honesty, it must be said that, in this respect, *Aspects* was not the next revolutionary step forward but the return to a traditional insight. In other words, the development from *Syntactic Structures* to *Aspects* is, apart from some new ideas and improved explicitness, more accurately characterized as a return to the tradition than as something revolutionary. It was soon understood that when syntactic structures are projected from lexical items, separate rules to generate syntactic structure are redundant. Hence, the return in *Remarks on Nominalization* (Chomsky 1970) to X-bar theory, a concept derived from Harris (1951).

By that time, the redundancy problem was well understood. Chomsky (1981: 31) formulated it as follows:

> Thus information concerning the class of subcategorization frames is in effect given twice in the grammar: once — implicitly — in the lexicon, as a property of the class of lexical items in its totality; and once — this time directly — by the rules of the categorical component.

What had been achieved by that time was greater explicitness than in the tradition, based on better and more uniformly applied representational techniques (trees, labeled bracketings, etc.). Naturally, that led to new empirical discoveries, like the pervasive locality of syntactic relations (island phenomena etc.). Conceptually, however, generative grammar since the 1970s had become a more or less explicit version of the traditional word-based conception of grammar, rather remote from the idea that *Syntactic Structure* had introduced something revolutionary by applying the formal methods of recursive function theory to natural language. Indirectly, this is confirmed by the fact that mathematical linguistics gradually disappeared into the background.[17]

Thanks to Emonds's idea of structure-preservingness (1970), the revolutionary nature of generative grammar came even more under fire: If the outputs of transformations have exactly the same form as the outputs of phrase structure rules, why would one need transformations in the first place? If the kind of structure generated by phrase structure rules is all there is, everything could be reduced to X-bar theory (a theory of lexical properties) and therefore to a form of grammar completely compatible with the tradition. In the 1970s, many syntacticians came to that kind of conclusion in one way or another, leading to more or less transformation-free variants of generative grammar (Brame 1978, Bresnan 2001, HPSG, Koster 1978, 1987). Mainstream generative grammar, however, insisted on the transformational residue 'Move α', which eventually disappeared but lives on in current minimalist theories as 'internal Merge'. I have argued elsewhere why I do not find this ongoing derivational tradition convincing (see, for instance, Koster 2007). Although I do not see X-bar theory as formulated in the 1970s and 1980s as the last word, I find the leading idea basically correct, namely that syntactic structures are projected from lexical items. Given a word, its possible syntactic environments are predictable, which can be seen as a reflection of the public knowledge about a language.

17 See Pullum (1989) and Tomalin (2002, 2006) for discussion.

What used to be called trace theory (since Chomsky 1973) was another aspect of structure-preservingness. Whereas Emonds's theory had the ultimate consequence that a core class of transformations does not create new structures other than the lexical template structures given by X-bar theory, trace theory had the consequence that no information of earlier levels was lost. This was the beginning of the end of the idea of levels of representation (like D-structure, S-structure, and Logical Form). Level theory was still fiercely defended by Chomsky (1981), but rejected in my own work (Koster 1978, 1987). In Chomsky's more recent work, level theory is rejected as well and even presented as one of the fruits of Minimalism. However, the disappearance of level theory was a consequence of appreciating structure-preservingness.

In contrast to how it was often perceived, then, transformational grammar was conceptually dead by the 1970s. Lexical-independent phrase structure rules had been replaced by lexical template theory (X-bar theory), which was in many ways a return to the pre-generative tradition. Thanks to structure-preservingness, major transformations (particularly the core 'families' of NP- and Wh-movements) were superfluous. Nevertheless, it was never sufficiently appreciated that, thanks to structure-preservingness, for every structure generated with 'Move α' the very same structures could be generated without 'Move α'. It is odd, therefore, that theories that eliminate redundant concepts as crucial to earlier theories as 'movement' and multiple levels of representation were often seen as notational variants of theories that maintained these superfluous elaborations.

It should be noted that structure-preservingness led to theories that, in fact, eliminated structure-preservingness itself. In theories like those in Koster (1978, 1987) traces are no longer residues of movement but incompletely lexicalized templates, to be completed by the syntactic environment of the unlexicalized element. For this, no new mechanisms had to be stipulated (which would have led to notational variants of movement-based theories). On the contrary, completion in filler-gap constructions ('movements') could be seen as an instance of a very general property of template structures, namely that their nodes can share their properties with sister nodes and mother nodes. This can be done in a strictly variable-free (= local) way, eliminating the need for 'constraints on variables' in the sense of Ross (1967) . Island conditions could be redefined as upper bounds on the vertical spread of a feature (see Koster 2003, 2007 for details).

Thanks to the idea that basic syntactic structure is nothing other than the (sometimes partial) realization of the template structure associated with lexical items, full historical continuity with the more than 2,000-year old tradition of the field could be restored. This does not mean that no progress was made since pre-generative times. I already mentioned the increased explicitness of representation and the use of more or less uniform techniques and methods world-wide. Perhaps the greatest achievement of generative grammar, up until the present day, is the mass of insights developed about 'displacement' (the phenomenon originally described as 'movement'). The recognition of the more ubiquitous presence of empty elements of partially lexicalized templates (formerly: trace, pro, PRO, etc.) has led to enormously increased understanding of the abstractness of syntactic structure, with classical discoveries as the establishment of the local nature of all secondary computation (i.e. computation based on the primary structures provided by X-bar theory). Furthermore, traditional X-bar structures were expanded with numerous functional projections (based on C, D, Infl, Agr, etc.) that had the same general structure as lexical projections.

But all these fruitful developments should not obscure the fact that modern abstract syntax is nothing other than an elaboration of the traditional idea that syntactic structures are realizations of the properties of words. Almost everything that made appear generative grammar revolutionary in the 1950s and 1960s has turned out to be wrong. I am thinking about the application of formal methods derived from recursive-function theory, phrase structure rules, transformations (including the residue 'Move $\alpha$'), levels of representation, etc.

Minimalism (not as a program but in practice) is another non-revolution and a partial return to the failed pre-lexicalist theories of the 1950s and 1960s. Lexicon-independent theories of sentence generation inevitably lead to the redundancy problem, indicating that something is wrong. Unfortunately, this insight, that was so clearly formulated by Chomsky (1981: 31), was forgotten by the time the minimalist framework was developed. In most minimalist theories, lexicon-independent sentence generation has made an unexpected comeback in the form of the operation Merge. Merge differs from phrase structure rules but partially runs into the same problems: It combines lexical elements while ignoring the fact that they are not dummies. Lexical items, even before Merge has applied, already have full-fledged combinatorial properties that fully specify the hierarchical configurations that are redundantly generated once more by Merge. This redundancy includes the property of recursion. In agreement with the tradition, X-bar theory correctly accounted for the fact that, for instance, verbs can have complements that contain verbs (clauses). When in some minimalist numeration a verb is impatiently waiting to be merged, it already has recursion among its projectable properties, *before* the operation Merge applies. Merge is just as redundant as phrase structure rules because it mimics parts of the lexical properties of the verb, particularly its property that it can be combined into a hierarchical structure with recursion.

In spite of this obvious problem, it is widely believed that X-bar theory is superseded by Merge and its generation of bare phrase structure. However, it seems to me that this belief is based on confusion between our (possibly innate) background capacity for recursion and the application of this capacity to words. Merge could be a correct description of the former, while X-bar theory could be a correct characterization of the application. In fact, I believe the redundancy problem is inherent to this confusion between unapplied background capacity and actual application. Assuming that the capacity for combining things recursively has a biological basis, this capacity only has something to do with language in that it has been used to give complex properties to certain cultural objects, namely our invented words. Thanks to our biologically-based capacities, we are able to assign to our little inventions not only information about the extra-linguistic world but also information about how to combine words with other words. Associated with each word is a template structure that is tentatively described by X-bar theory. Sentences are generated by partially lexicalizing the template structure of some word (perhaps beginning with the verb in most cases). Since the templates allow for recursion, there is no upper bound to the potential syntactic environments of words.

Only if sentences are generated by lexicalizing the templates associated with words, we can avoid the redundancy problem. There is no evidence that sentences must be generated by an operation Merge that treats the morphemes in some numeration as dummies, without accessing their internal properties before the end of some phase at which 'the interfaces' are reached. Each individual word is a complete interface element connecting three kinds of information: public form (for instance sound form), information for conceptual-intentional interpretations, and information about possible syntactic environments.

Originally, bare phrase structure was used as an argument against Kayne's derivation of his LCA (Kayne 1994; see Chomsky 1995: chap. 4). Whether the critique of Kayne's derivation was justified or not, bare phrase structure (via Merge) seems to have thrown away the baby with the bath water. With truly minimalistic zeal, bar levels, labels and indices were dismissed. However, bar levels, as in the three-bar level representation $[_{N''}\ [_{N'}\ [_{N}$ book]]], are not empirically vacuous, as the bar levels more or less correctly indicate that the word *book* has the potential to be expanded two levels 'up' — it can be followed by a PP and preceded, at the next level up, by an article:

(4) $[_{N''}$ the $[_{N'}\ [_{N}$ book] about linguistics]]

Whether this representation is correct or not, it certainly has empirical content, as it seeks to account for the (potential) combinatorial properties of the word *book*. If bar levels are given up, the question arises which alternative way there is to account for the 'ranked' combinatorial properties of the noun. Reference to 'the interfaces' will not do, as those are only reached post-Merge, while the word has the properties in question pre-Merge. Similar considerations can be held against the ban on labels and indices: These are elements of a meta-theory accounting for real properties of the world. All representational devices are fine in science, as long as they have empirical content. Thus, the indices in (5), interpreted in the linguist's practice, correctly account for the fact that *John* is coreferential with *himself*:

(5) $\text{John}_i$ saw $\text{himself}_i$

Indices are empirically motivated notational devices that account for a real fact *about* sentence (5). It is not very fruitful to criticize notational devices independently of the empirical content they stand for. It would be like devising critique against theories of optics in physics for their non-minimalist practice of representing rays of light by arrows.

In many cases, minimalist theories show a regrettable lack of explicitness, so that they are often hard to evaluate. It is not clear, for instance, how the conceptual-intentional interface deals with the massive mis-generation resulting from the mismatch between s-selection (properly seen as conceptual intentional) and purely syntactic c-selection (see Chomsky 1986 for the terminology and, for instance, Odijk 1997 for discussion). The same can be said for things like subcategorization. In the rare cases that the minimalist counterpart of subcategorization is made explicit (Adger 2003: 86), a verb like *kiss* is given the following c-selection property:

(6) *kiss* [V, *u*N]

According to Adger, this means that *kiss* is a verb V with an uninterpretable feature *u*N. Whenever *kiss* undergoes Merge, the derivation will eventually 'crash' unless *kiss* has the good luck to be merged with an N, which would 'check' the offending feature and eliminate it. Apart from the ever more exotic terminology of crashing and checking, I see no progress here over the way subcategorization was accounted for in the style of Chomsky (1965):

(7) *kiss* [+V, __NP]

What is disturbing about this reformulation is that the basic idea of X-bar structure implicitly remains intact (VP structure as a property of the V), but that it is falsely pretended that we have an alternative to X-bar theory. In fact, Adger deserves credit for having made this explicit, because very often reference to Merge is some kind of lip service rather than part of an explicit account of sentence structure. More often than not, *how* exactly Merge cooperates with the interfaces to specify real structures is left in the dark. There is nothing against Minimalism as a program, but its practice often shows lower standards of explicitness than what the field was used to in earlier periods.

But there is a deeper reason why sentence generation by Merge will not work, at least not along 'internalist' lines. The point is that possible syntactic environments are conventional ('c-selection') and therefore part of our external, cultural memory. In other words, they are not 'biological' in any sense (particularly not as following from some innate conceptual-intentional system). Suppose that our conceptual system (innate or not) is such that we know that a verb corresponds to something selecting two arguments. Then nothing biological determines whether these arguments are expressed as DPs, PPs, CPs or even silent categories (as in pro-drop languages and languages relying more on discourse completion, like Chinese). The way arguments are expressed is, even when possible choices are 'biologically' constrained, ultimately conventional, and therefore determined by our culture.

The idea that language is a cultural phenomenon, partially based on biology but ultimately a technology (i.e. *applied* biology), is confirmed by the fact that a similar capacity is applied in other cultural creations, such as arithmetic and computer programs. Recursion in computer programs can only be created by us thanks to our biologically given capacity to deal with recursion. But nobody will conclude from that that we need a new scientific discipline called 'bio-computing' that concerns itself with recursion as the core of our faculty of computer programming 'in the narrow sense'. The difference with language is not a matter of principle, but possibly the fact that the application in language is genetically facilitated in ways computer programming is not. But genetic facilitation is something different from genetically-based function assignment. The former still leaves room for human agency, while the latter does not involve human interference at all. As we discussed before, no matter how much language is facilitated by evolution, its ultimate functionality depends on the human creations — words — preserved in our culture. The functionality of the heart as a pump does not crucially depend on the fruits of human agency this way.

Interestingly, both Hauser, Chomsky & Fitch (2002) and Chomsky (2007) suggest ways of looking at things not too remote from what I am advocating myself. In Chomsky (2007, 7) we read the following:

> The conclusion that Merge falls within UG holds whether such recursive generation is unique to FL or is appropriated from other systems. If the latter, there still must be a genetic instruction to use Merge to form structured linguistic expressions satisfying the interface conditions. Nonetheless, it is interesting to ask whether this operation is language-specific. We know that it is not. The classic illustration is 'the mathematical capacity', which troubled Alfred Russel Wallace 125 years ago because it "is wholly unexplained by the theory of natural selection, and must be due to some altogether distinct cause", if only because it remained unused. One possibility is that it is derivative from language.

This passage is worth reading twice because it considerably weakens the biolinguistics thesis: Neither in its origins nor in its ultimate applications is Merge necessarily language-specific. However, as we just discussed, there is no evidence at all that Merge is directly used "to form structured linguistic expressions", so that it does not make sense either to say that there is "a genetic instruction" to do so. At best, there has been some kind of culture-brain co-evolution, so that there are genetic factors that facilitate to some degree the process of investing certain cultural objects — words — with the complex properties they happen to have.

## 6. Conclusion: Against 'Fatalism Light'

Let me now summarize some of the main points of this article and conclude with the deeper reasons why I am skeptical about biolinguistics. To begin with, there is a trivial interpretation of the biolinguistics thesis that is no doubt true, particularly the idea that human language is possible thanks to a genetically based capacity to deal with recursion. The reason that this is true is trivial because *all* human capacities are genetically based. Something in our genetic make-up makes us dramatically different from apes and must allow us to create recursive grammars. However, what the biolinguistics thesis seems to be about is the non-trivial idea that there is a language faculty (in some narrow sense) comparable to organs like the kidneys and the heart. This non-trivial thesis, it seems to me, is false and denies the role of human agency and culture in the creation of language, no matter how narrowly construed.

No physical structure is intrinsically functional, including the physical structures that allow us to deal with recursion. Physical structures are only functional in some context and thanks to some historical process. In biological evolution, structures become functional thanks to long historical processes involving natural selection. There never is an intrinsic and deterministic relation between form and function, even after long optimization through adaptation, as shown by the numerous and unpredictable exaptations. Since functional dedication of structure is a historical, non-deterministic process, successful cases can only be preserved by some kind of memory. During most of evolution, the memory was provided by big molecules, like DNA.

Whatever other types of memory there are, humans distinguish themselves by a symbiotic relationship with a shared, external and symbolic memory in which the fruits of successful agentive function assignment are preserved. Our culture conceived this way is a man-made environment providing numerous new contexts to give a function to our biological capacities. These biological capacities themselves are culture-neutral, that is, they only have something to do with our culture when seen 'from above', from the vantage point of our man-made environment. The example of dyslexia may illustrate this. According to current insights, dyslexia is caused by a brain disorder. But at a purely neurobiological level, in abstraction of our cultural context, there is nothing in the brain that has anything to do with reading. Writing and reading are very recent inventions (of, say, 6,000 years ago) and the neurobiological condition causing dyslexia must have occurred long before reading was invented.

This is the pattern we see throughout the worlds of biology and culture: No structure is intrinsically functional (or dysfunctional) in isolation but only with respect to some environment. Language is like dyslexia in that it only is functionally related to certain brain structures in relation to a context of our own making and external to the brain. This makes linguistic functioning different from organic functioning, or to put it differently: There is no language faculty at a purely biological level. Like all our culture-based activities, language is *applied* biology and therefore a form of technology.

There are also reasons to resist biolinguistics — in the non-trivial sense — on more general grounds. Conceptually, it is related to sociobiology, certain forms of evolutionary psychology and, ultimately, to social Darwinism. Most biolinguists, if there is such a species, are not ultra-Darwinists and are considerable more skeptical about the application of socio-biological ideas to humans than, say, Edward O. Wilson or Steven Pinker.[18] Nevertheless, biolinguistics seems to

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[18] For Chomsky's attitudes towards sociobiology, see Segerstråle (2000: 203–206). Ultra-Darwinism is the idea that adaptation to an environment is more important for the understanding of the forms of the organic world than physical, developmental and other intrinsic properties of organisms. It is the pan-adaptationism criticized by Gould & Lewontin (1972). Ultra-Darwinism can be seen as the diachronic version of Skinner's behaviorism: Both are selection theories that emphasize the shaping role of adaptation to an environment. From this perspective, Pinker (2002) is a unique *tour de force*: It combines ideas akin to those of Skinner's (ultra-Darwinist environmentalism) about our evolutionary past with theories like those of Chomsky's (nativism) about our biological present. Pinker could have avoided these mutually inconsistent foundations of his framework if he had learned from modern linguistics that selection theories (like Skinner's or Darwin's) are explanatory to the extent that their hypotheses are drawn from a limited hypothesis space. Behaviorism was not rejected because it was a selection theory (about behavior) but because it was close to empty as to a priori limitations on the set of possible hypotheses. Ultra-Darwinism of the kind of Pinker or Dennett (1995) is not generally accepted anymore by evolutionary biologists these days, as it has the same shortcomings as behaviorism. Evolutionary biology can only rise above the level of 'just so' stories by combining the idea of natural selection with a theory of possible form. This is admirably formulated by biologists like Müller & Newman (2003),

share with those frameworks the downplaying of human agentive creativity and the role of culture in preserving the fruits of that creativity. Humans are fundamentally different from animals in the (cultural) ways they depend on their biology and can overcome biological constraints. We are certainly constrained by our biology but we do not live a life dictated by our genes. Or to put it more succinctly, we were never biologically programmed to fly but we do it nevertheless.

As this fact cannot be missed, it is sometimes recognized by Wilson and Pinker, but they replace the rejected biological determinism and fatalism by some form of 'fatalism light'. This means that we have the liberty to deviate from the dictate of our biologically given nature, but only at a certain cost. Pinker (2002: 237ff.) even goes so far as to suggest that resisting what Thomas Sowell calls "the Tragic Vision" (of social conservatives from Edmund Burke to Milton Friedman) is possible but will be an uphill battle.[19] I am not denying that, due to our biological nature, social reform involves uphill battles sometimes, but there is no way to make reliable general statements about that, as Pinker wants to have it. The idea that our genes make conservatism less costly than the progressive advancement of social justice should be approached with the utmost suspicion.

Somehow, an elementary Panglossian error is lurking behind Pinker's assumptions (as has been observed for practically all sociobiology and evolutionary psychology), namely the idea that if our biological nature is the result of adaptations to certain environments, those environments are optimal to our nature in some absolute sense. More concretely, evolutionary psychologists like to say that progressives fight uphill battles in modern times due to the fact that our nature was an adaptation to the contingencies of hunter-gatherer societies over long periods of time. This idea has close to zero credibility for all kinds of reasons, but the most important point is that even if our nature was optimized by natural selection for hunter-gatherer societies, that would say absolutely nothing about how our nature would fare in other types of societies.[20]

As shown by the numerous exaptations in nature, certain structures can be optimized by natural selection for one function and be used later on in other contexts with equal or even more success. Stuart Kauffman, quoted above, rightly observed that future exaptations are a mattter of "ceaseless, unpredictable creativity". For any available structure, the most optimal context of application might still be something of a world to come. This is even true for the famous

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who claim that "neo-Darwinism has no theory of the generative" (p. 7). It is hoped for that the emerging discipline of evo–devo will contribute to such a theory. See Amundsen (2005) for a lucid account of some relevant issues.

19 The myth of the uphill battle is discussed (not under that name) by Edward Wilson (1975: 275), who is, like the later evolutionary psychologists, talking about adaptations to hunter–gatherer societies and "the early hominids still within us", with absurdities like a "genetically accurate" code of ethics. According to Wilson, we do not know yet which behaviors can be altered "without emotional damage or loss in creativity". "Uncertainty in this matter", Wilson continues, "means that Skinner's dream of a culture predesigned for happiness will surely have to wait for the new neurobiology". This kind of view, criticized in the text of this article, would turn human biology into an authoritarian nightmare. See also Rose, Kamin & Lewontin (1984) for a similar critique.

20 See Malik (2000: 244 ff.) for a critique on the ideas about the role of hunter–gatherer societies in human evolution.

Swiss army knife used as a metaphor by evolutionary psychologists to illustrate the modularity of the mind. A Swiss army knife is not one general tool (like an ordinary knife) but has various 'modules' optimized for specialized tasks. Its corkscrew, for instance, is optimized for pulling corks out of bottles. As always, this function (of the corkscrew) is contextual and not intrinsic. In a world without cork — not an unrealistic perspective at some not too distant future moment — it does not make sense anymore to talk about corkscrew functionality. But thanks to our capacity for agentive function assignment, we can give a new function to the corkscrew. We can use it even now to clean our finger nails, but we can also invent entirely new functional contexts. For any structure — mathematical, physical, or biological — there is an infinite set of functional contexts. It is not possible to predict which context will be the most optimal. It is a matter of invention and experimentation. Similarly, it is a matter of invention and experimentation (next to our commitment to ethical principles) which socio-cultural environment is the best for our inherited biological nature.

The biolinguistics thesis — in the non-trivial sense — has no direct relation with 'fatalism light' and Chomsky would be the last to embrace it. Nevertheless, throughout his work, a thinker like Steven Pinker seems to be inspired by the example of the flawed form of biolinguistics criticized in this article. It sets a bad example and it is paradigmatically undesirable that the most characteristic human attribute — language — is falsely reduced to the level of organic evolution rather than seen as the combined fruit of our innate biological structures and our own "ceaseless, unpredictable creativity" in assigning functions to these structures. Unlike what we see in the evolution of other organisms, the human version of the universe's creativity is agentive, free and sustained by a shared symbolic world of our own making.

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★ BRIEFS ★

# Externalization and Emergence: On the Status of Parameters in the Minimalist Program

Jason Kandybowicz

## 1. The Tension Raised by Parameters in a Minimalist Setting

It is widely assumed that the Minimalist Program imposes a boundary condition on theories of language, namely, that the human language faculty (FL) has a Principles–and–Parameters (P&P)-like architecture (Chomsky 1981, 1993). Hornstein *et al.* (2005: 5), for example, maintain that this perspective represents the "consensus view of the overall structure of the language faculty". Accompanying this core tenet is the Strong Minimalist Thesis (SMT), which holds that FL is perfectly/optimally designed to meet the interactive needs of the language-external (but organism-internal) cognitive sub-systems with which it interfaces, namely, the conceptual–intentional (C–I) and sensorimotor (SM) systems. Taken together, these two hypotheses raise a fundamental tension that is rarely considered. One goal of this brief is to bring this issue to light in hopes of stimulating sustained productive discussion and thus begin chipping away at admittedly broad and challenging related inquiries.

The basic question that the SMT raises in a P&P-model of grammar is this: To what extent would a parameter-free FL represent a departure from optimal design? One might put it another way. Why did FL evolve with flexible (i.e. parameterizable) principles over a more minimal/streamlined format consisting solely of fixed principles? We might reason that the net effect of a rigid FL would be to minimize surface language variation. From the perspective of communicative economy, this would appear to be a non-trivial boon. Thus, in light of the SMT, the P&P hypothesis raises challenging questions about the evolution of FL and the origin of parameters (i.e. the roots of language variation). Given the success of the model in resolving tensions of descriptive and explanatory adequacy, it is unlikely that the P&P lens will be abandoned anytime soon. Pursuing the SMT alongside the parameterized FL hypothesis thus forces one to confront a formidable question. In what way can the flexibility of parameterization be squared with the optimal design of FL?

Before addressing this issue, it is important to acknowledge that although

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rarely discussed, the problem of language variation (*qua* the existence of parameters) arises independently of the SMT in the P&P tradition. Baker (2001: 207) provides a good overview, remarking: "[…] the silence on this point is striking. Rarely is the question even framed". He then goes on to consider a number of cultural/sociological and evolutionary/biological proposals, ultimately rejecting each and concluding that the existence of parameters/variation is for the moment an unexplained mystery. Here, Chomsky's (1975) distinction between *mysteries* and *problems* is relevant. Unlike problems, which resemble questions that have been addressed by way of scientific progression along established channels and thus have a solvable quality, mysteries are open questions that by their very nature seem to limit progress and/or derive from human cognitive limitations. If the existence of parameters is a true mystery in this sense, as Baker suggests, one could argue that the pursuit of the roots of language variation is, for the moment, pre-mature or possibly even ill-conceived. In this squib, I'd like to suggest that the minimalist perspective concerning the asymmetrical relationship between language and its cognitive interfaces, familiar from mid-1970s biolinguistic theorizing, provides a lens through which the 'mystery' of language variation can be reinterpreted as a 'problem' (albeit a formidable one).

## 2. Some Evolutionary–Developmental Approaches to Parameterization

One well known socio-evolutionary approach to the origins of linguistic variation is Dyson's (1979) idea that parameterization affords the advantage of enabling the formation of separate and distinct social groups, thus promoting accelerated evolution and ensuring the survival of the species by means of genomic diversification. Ultimately, though, this proposal can be rejected on the grounds that parameterization would offer no significant contemporaneous selective advantage, only a long-term one, which is antagonistic to the principles of natural selection. Furthermore, as Baker (2001) points out, parameters are over-engineered for the purpose of group formation and identification, given that variability of pronunciation alone would suffice to distinguish one group from another.

On the bio-evolutionary side, a greater number of proposals exist. Pinker & Bloom (1990) suggest that at the point of FL's evolution, the existing cognitive mechanisms were powerful enough to facilitate parameter setting so that there was simply no adaptive pressure to specify/crystallize those parameters. Simply put, there was little risk that language learning would be compromised under a parameterized FL. Pinker (1994) speculates that parameterization arose to offset changes to FL induced by inevitable random genetic variation, thus keeping mutual intelligibility in check by means of providing the resources for interpersonal grammar synchronization. See Baker (2001) for a critique of these proposals. Another speculation, first discussed years ago in class lectures by Noam Chomsky and then later independently proposed by Massimo Piattelli–Palmarini (Noam Chomsky, p.c.), is that variation/parameterization involves a 'mini-max' problem: Leaving principles open/unspecified reduces genetic information, but increases the cost of acquisition. One conceivable solution is that the existing parameters are an optimal trade-off. See Chomsky (2004: 166) for discussion on this point.

## 3. Biolinguistic Approaches to Parameterization

Returning to the issue at hand, that is, coming to grips with the existence of a parameterized FL under the SMT, two ways of proceeding immediately suggest themselves. The first possibility is to maintain the minimalist null hypothesis: FL evolved with a P&P-style architecture. The difficulty with maintaining this perspective is that it isn't clear on what grounds the existence of parameters renders FL perfectly engineered to meet the needs of the C–I and SM interfaces. On the face of things, the flexibility afforded by parameterization seems irrelevant to the rigid demands imposed on language by the cognitive interfaces. To make matters worse, existing research on variation/parameterization from the mathematical perspective shows that given that a parameterized FL affords no environmental advantage, there is selective pressure to reduce/eliminate parameterization from the language faculty (Nowak *et al.* 2001). How might a parameterized architecture be justified on minimalist terms, then?

One approach would be to maintain that the evolution of a system with both principles and parameters might be justified on naturalistic grounds (Chomsky 1980), thus sidestepping the issue raised by the SMT. Along these lines, one might speculate that parameterization is not inherently unique to FL, but rather emerges as a recurring principle of design/organization in the organic world. Certainly, this view would be harmonic with Jacob's (1976) analysis of biological speciation. If true, the question of the evolution of parameterization in FL would be subsumed under the much larger question concerning the emergence of parameter-like organization in the biological world. In other words, on this approach, speculation concerning the compatibility of parameterization and optimal language design would lie beyond the scope of linguistic inquiry, falling instead within the domain of the biological sciences. We might view this as another instance of minimalism making non-trivial inroads in the natural sciences. See Boeckx (2006) for other examples. This possibility, though certainly not implausible, does not seem particularly promising.

Another option would be to maintain the existence of parameters, yet hypothesize that FL evolved without them, optimally meeting the needs of the C–I system along the way. On this approach, parameters would be viewed as emergent properties of the language system, as opposed to defining core components of FL architecture as in previous GB and minimalist conceptions. This is precisely Chomsky's (2008) current position. Accordingly, the problem of reconciling the existence of parameters with optimal design (as well as the more basic question concerning the existence of parameters/variation) is largely misconceived. Chomsky argues for the primacy of the relationship between FL and C–I on a number of grounds. As such, the externalization of language (the latter, a set of mind-internal conceptual/symbolic representations) via Spell-Out to the SM system is viewed as a secondary phenomenon. Under this perspective, language externalization involves mapping a newly evolved computational system optimally designed to interface with C–I to an independent and unrelated

SM module that has been intact in the species for hundreds of thousands of years. Regardless of whether or not the recruitment of earlier hominid SM systems was accompanied by special adaptation, a mapping of this sort (a sort of work-around solution) poses a cognitive problem of sorts. The problem arises in virtue of the fact that systems designed without each other in mind must now interface, creating tensions of compatibility that limit the generative capacity of at least one of the interacting systems. Within this set of assumptions, it becomes possible to maintain that parameterization reflects the different, but limited ways of solving this cognitive problem. Distinct linguistic communities may have solved it in different ways and at different times, thus yielding the observable surface diversity of languages we observe today. Because the mapping from syntax to semantics would pose no such cognitive problem, given the optimal design of the computational system, the existence of an inherently parameterized syntax would be surprising. Thus, under this conception of language, diversity or parameterization would not enter into the evolution of FL, a line of thought that is reminiscent of work carried out in the embodied cognition tradition (see Clark 1998, among others). Baker (2001: 215) alludes to the possibility of a biological explanation of parameters along similar lines: "Parameters might have been a biological accident rather than an adaptation". He also considers the possibility that "parameters might exist because of physical necessity". This view, perhaps the most interesting of the views considered thus far (and perhaps the most compelling as well, given Occam's Razor-style considerations), has important implications for the architecture of FL. If correct, parameters would reduce to emergent properties of the language system imposed by the mapping to SM. They would thus have no independent status in the architecture of the language faculty, as conceived of in the GB era.[1] This sort of position is pursued by Boeckx (forthcoming), who maintains that variation emerges as a natural consequence of a genetically underspecified FL that is shaped largely by language-external principles such as computational efficiency (i.e. Chomsky's 2005 'third factor' in language design). A consequence of this epiphenomenal take on variation, then, would be to limit the domain of parameterization to the PF side of grammar. In

[1] An anonymous reviewer points out that treating parameters as epiphenomena raises several important questions. One, if parameterization is related to externalization, a secondary phenomenon, could parameters be random? Two, does this imply that parameterization can tell us little about the architecture of the language faculty? Space reasons preclude a detailed response to these questions. I refer the interested reader to Boeckx (to appear), who suggests that parameterization is a consequence of underspecified computational possibilities, which although underspecified, impose certain limitations on possible structures. For example, symmetric Merge allows for either head-initial or head-final structures. This would suggest that rather than being random, parameters are actually constrained by the interplay between the possibilities made available by the computational system and the requirements imposed by the SM system. With respect to the implications for the architecture of the language faculty, note that the position offered here, if true, provides corroborating evidence for the primacy of the mapping from syntax to C–I, a non-trivial architectural claim. Furthermore, if parameterization is an epiphenomenon of externalization, then it becomes possible to reanalyze phenomena once thought to be strictly syntactic in terms of morphophonology, thus expanding the analytical channels and pathways exploitable by linguistic theory. See Boeckx (to appear) for further discussion.

other words, a corollary of the emergence/externalization hypothesis is the conclusion that parameterization/variation does not exist on the LF wing. Clearly, this position is at odds with work on semantic parameterization, for instance, Chierchia's (1998a, 1998b) Nominal Mapping Parameter, a purported semantic parameter regulating the ability of languages to use bare nominal expressions in argument or predicate positions, a dimension along which languages vary in limited ways. The position also conflicts with claims of pragmatic variation sometimes cited in the literature (cf. Matthewson 2006 on presuppositional cross-linguistic variation) and attempts to unify syntactic and semantic parameters (cf. Svenonius & Ramchand 2008 on the specification of meaning in the narrow syntax versus negotiation of meaning by C–I). The role and status of semantic parameters within FL architecture would thus need to be rethought if the emergence/externalization hypothesis is on the right track. At the very least, it is clear that this view of parameterization leads to interesting questions and conceptual consequences that are well worth exploring.

## 4. Conclusion

To the extent that the minimalist perspective on parameters makes predictions and is falsifiable (e.g. all linguistic variation is morpho-phonological in nature), inquiry into the nature and origin of linguistic variation can in principle proceed along the familiar channels of scientific methodology. In this way, what once appeared to be a 'mystery', can now be regarded as a 'problem' with a theoretically discoverable solution.

In closing, the problem of variation raised in this squib highlights the sort of issue that both arises under a biolinguistic perspective and sharpens under minimalist scrutiny. It is hardly worth stating that further work is needed on this topic as many of the details of FL remain perched on the horizon of scientific inquiry. By considering the issue of parameterization raised by the SMT, however, I hope to have initiated a baby step in the right direction.

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★ FORUM ★

# A Question of Irresponsibility: Postal, Chomsky, and Gödel

John Collins

As part of a highly critical discussion of much recent work in linguistics, Paul Postal identifies "the most *irresponsible* passage written by a professional linguist in the entire history of linguistics" (Postal 2004: 296).[1] The brief of the present piece is to correct an error in Postal's reasoning. Space precludes a discussion of Postal's broader assault against what he perceives to be 'junk linguistics', although some brief remarks will be offered that pertain to the essence of his criticism.[2] The "most *irresponsible* passage" is from Noam Chomsky and includes the following remarks:

> [Expressions, i.e. the output of the language faculty — JC] are not entities with some ontological status; they are introduced to simplify talk about properties of [the language faculty — JC], and they can be eliminated in favor of internalist notions. One of the properties of Peano's axioms PA is that PA generates the proof P of "2 + 2 = 4" but not the proof P′ of "2 + 2 = 7" (in suitable notation). We can speak freely of the property "generable by PA", holding of P but not P′, and derivatively of lines of generable proofs (theorems) and the set of theorems without postulating any entities beyond PA and its properties. (Chomsky 2001: 41f.)

Chomsky's general point in this passage, I take it, is that the empirical coverage of any theoretical discourse can be rendered as a commitment to a set of the relevant entities (e.g., the set of possible trajectories of Halley's Comet, the set of possible electrons, the set of possible tigers, or, indeed, the set of expressions as the idealized output of the language faculty). Such ontological commitment to the sets of the relevant entities, however, is not required for the explanatory goals of the given sciences, unless, of course, the science is a branch of mathematics that is concerned with large sets and their properties, and there the identity of the entities is irrelevant. In linguistics, at least, "[n]o 'Platonism' is introduced, and no 'E-linguistic' notions: only biological entities and their properties" (Chomsky 2001: 42).

Now, of course, no scientific theory is merely concerned with what finitely happens to obtain; for example, zoology is concerned with tigerhood, as it were,

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My thanks go to David Miller, for numerous helpful remarks, and to Paul Postal, for alerting me to certain infelicities and misinterpretations in earlier drafts.

1 For, to my mind, a sound review of Postal's more general remarks against 'junk linguistics', see Boeckx (2006).

2 For wider discussion of the same themes, see Collins (forthcoming).



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not its contingent instantiation. Every theory, we may say, has an infinite import. This is because the very notion of explanation is modal insofar as it must support counterfactuals. Thus, a law does not describe phenomena but tells us what will occur under any conditions that satisfy the properties the theory posits. For example, Newton's laws don't purport to *describe* our solar system (unlike Kepler's 'laws'), but instead tell us what will occur in any circumstances that are covered by the concepts of classical mass and force, which our solar system happens to realize (within certain parameters — forget about twentieth century developments). In this sense, Newton's laws tell us about infinitely many possible systems, even though our universe is finite (we presume).[3] The same holds in the case of linguistics. A formal theory tells us about infinitely many possible states the human mind/brain can fall into, without committing itself to the idea that the mind/brain is infinite, or, of course, that there are infinitely many sentences anywhere at all, not even in Plato's heaven. To be sure, we need to employ the notion of an infinity of expressions, in Chomsky's sense, much as we are required to think about infinitely many states of any physical system (theorized, say, in terms of Lagrangians or Hamiltonians). My present point is merely that such notions, while essential in the modal sense explained above, don't attract our ontological commitment, at least not if we are working within the theory (cf. Feferman's 1998 position on the relation between science and mathematics).[4] If all this is so, linguistics looks to be in the same boat as any other science. Let us now turn to the detail of Chomsky's remarks quoted above, which Postal finds so objectionable.

Postal thinks Chomsky's reasoning is particularly irresponsible because Chomsky distorts the mathematical case (PA, a formalized theory of elementary arithmetic) to lend weight to his ontological claims about linguistics. The average linguist, we may presume, is not familiar with the relevant mathematics and so is liable to be taken in by Chomsky's conceit.

Postal reads Chomsky as suggesting that the standard metatheoretic results for a theory of elementary arithmetic (PA) do not require the postulation of a set of truths. Such a suggestion, however, *apparently* contradicts Gödel's incompleteness theorems; indeed, the results would not even be formulatable: "[To show that] a system like PA is complete, one must consider the relation between two sets […] the set of theorems of [PA] and the set of truths about [the natural numbers]", both infinite (Postal 2004: 303). So, if Chomsky's remarks are correct "not only would it be impossible to *prove* Gödel's incompleteness theorems, it would be impossible to even *formulate* them" (Postal 2004: 303). Postal's reasoning is awry.

First, Postal appears to have been misled by the familiar informal

[3] I don't presume that 'laws' necessarily determine everything that happens to occur, for many events or states might fall outside of the scope of the concepts that enter into general laws. For example, presumably, no law of physics will tell us the number of the inner planets. See van Fraassen (1989) for, to my mind, an excellent discussion of these issues.

[4] Controversy on this issue appears to arise from the common talk in linguistics and philosophy of natural languages being infinite. The infinity of English, as it might be, however, is not a phenomenon. The phenomenon is that particular organisms are continuously novel in their speech and understanding, which we theorize in terms of unbounded generation.

presentation of Gödel's (first) incompleteness theorem as showing that there are unprovable truths of PA. There is, I should say, nothing wrong with such a characterization just so long as one does not take it to reflect the actual mechanics of Gödel's proof (there are many ways of presenting Gödel's theorems; here we are concerned with Postal's claims about what is *essential* to them). Gödel set himself the task of showing that finitary PA is incomplete on its own terms, i.e., without making any non-finitary assumptions, such as the set of every PA truth, for such a set is inadmissible in finitary metamathematics. Gödel's result is wholly syntactic/combinatorial and so finitary in the sense that it can be carried out within PA itself.[5] Gödel wrote:

> The method of proof […] can be applied to any formal system that, first, when interpreted as representing a system of notions and propositions, has at its disposal sufficient means of expression to define [such] notions […] and in which, second, every provable formula is true in the interpretation considered. The purpose of carrying out […] the proof with full precision in what follows is, among other things, to replace the second of the assumptions just mentioned by a purely formal and much weaker one.
> (Gödel 1931/1986: 151; cf. p.181)

As mentioned above, Gödel adopts this weaker formulation in order to be consistent with Hilbert's finitary scruples on metamathematics. The weaker formulation substitutes formal (syntactic/combinatorial) consistency principles — 'simple consistency' and 'ω-consistency' — for 'truth'. This syntactic character of the result became clearer still with Rosser's later technique of generating undecidable formulae without appeal to ω-consistency.

So, Postal's claim that the relevant theorems are unformulatable without the positing of a set of truths is false; the opposite is the case, at least if the theorem is not to be question begging against finitary metamathematics of the kind that concerned Gödel.[6]

Secondly, the general notions of completeness and incompleteness do not require the postulation of two sets; both notions can be construed in finitary proof-theoretic terms, which are the notions Gödel employed. Gödel's proof proceeds by a lemma that allows for the construction of PA formulae that are such that, if PA is consistent, then neither the formulae nor their negations are theorems of PA ('simple consistency' also needs no postulation of a set of truths; PA is consistent if and only if there is at least one PA formula that is not a PA theorem). The incompleteness of PA follows, on the assumption that it is consistent (the 'undecidable' formulae become theorems in a system richer than

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5 The so-called 'Hilbert Program' was dedicated to establishing finitary consistency proofs for classical mathematics (arithmetic, analysis, etc.), proofs that, *inter alia*, make no essential appeal to 'actual infinities' by way of, for example, unrestricted use of excluded middle or negations of universal quantifications. There are currently many different ways of construing finitary scruples. Postal (2004: 301) accuses of Chomsky of "distortion" for conflating the axioms of PA with a proof theory. It is perfectly standard, however, to use 'PA' to designate a first-order formalization of the axioms along with a given proof theory, as Chomsky makes clear in his talk of "suitable notation".

6 It is worth noting that Gödel's completeness proof for first-order logic *does* require the postulation of a set of truths in that the proof shows that there is *no* first-order satisfiable formula that is not a theorem.

PA, which is what the theorem amounts to, in one sense). So, one can prove Gödel's theorem, as Gödel himself did, without the postulation of a set of truths.[7]

Postal is correct to think that Gödel refuted the ambitions of strict finitary proof theory as laid out by Hilbert, but Chomsky does not even hint at the contrary.[8] Chomsky's point is almost banal. One can, by familiar techniques, codify first-order PA as a Turing machine program (in fact, the main business of Gödel's 1931 paper was to show that the relevant metamathematical concepts are all decidable apart from 'provable', which is only semi-decidable). The program is a finite object that has an infinite output. One is not thereby committed to the output being a set in Plato's heaven anymore than an astronomer is committed to the infinite set of the trajectories of Halley's Comet. In formal proof theory, one thinks of a proof as a (finite) set of formulae drawn from the relevant formal language. Whether a formula is a theorem ("2 + 2 = 4") or not ("2 + 2 = 7") is determined by the axioms and rules of inference of the theory (the properties of the 'finite object'). Imagine coming a across a machine, like Paley and his watch, that spews out arithmetical formulae. One can investigate the 'program' of the machine as a finite object realized in the physical structure of the machine. One may, for instance, wonder whether there is a formula compatible with the 'program' (well-formed), which is such that neither it nor its negation will be produced. Such an approach towards the machine does not involve "postulating any entities beyond [the program] and its properties". Of course, Gödel showed that any formal system of sufficient richness (capable of representing arithmetic) will express undecidable propositions, but this result does not flow from looking beyond the finite system (incompleteness follows from finitary reasoning, which was Gödel's point), nor does it show that any particular metaphysics of mathematics is correct.[9] Likewise, linguistics may freely posit a finite object realized in the human mind/brain and theorize over its derivations and sets of expressions without thereby committing itself to any entities beyond the biology of the mind/brain. If there are such entities, then they have to be established, much as, *mutatis mutandis*, Gödel established the limitations of certain formalizations, but the mere availability of a set theoretical formulation does not take one beyond whatever entities are one's primary concern.

Postal raises serious issues about the interpretation of formalism vis-à-vis phenomena, or, equivalently, the status of abstractions in science. My brief has been to argue that no quandaries over this issue selectively strike at work in linguistics; more specifically, the status and interpretation of Gödel's results have

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7 Via techniques introduced by Tarski, one can define a truth predicate for the language of PA in a richer metalanguage, where the undecidable formulae generable in PA are rendered as theorems, i.e. true. It is in this sense that the (first) incompleteness theorem shows that there are unprovable truths of PA (keeping to the resources of PA itself). The class of second-order PA truths, however, remains undecidable.

8 One can be finitary, however, without being Hilbertian. Kleene (1986: 138) notes: "Gödel's second incompleteness theorem, rather than ending efforts at finding finitary consistency proofs, pointed out the road to success". That is, Gödel showed that certain *elementary* procedures were inadequate.

9 Gödel was a Platonist by inclination, but he did not take his incompleteness results to establish the truth of Platonism; indeed, he was also sympathetic to Leibniz and Kant on the philosophy of mathematics. See Gödel (1961/1995).

no particular bearing on linguistics. Postal's remarks are certainly not the most irresponsible in the history of linguistics; equally, to say the least, they go no way to show that Chomsky's remarks are either.

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★ FORUM ★

# The Incoherence of Chomsky's 'Biolinguistic' Ontology

Paul M. Postal

## 1. Background

I am indebted to the editors of *Biolinguistics* for their unsolicited invitation to comment in this forum along with John Collins. A bit of background. 2004 saw the publication of a book of mine whose chapter 11 explicated the harsh claim that a selection from the work of Noam Chomsky *was the most irresponsible passage written by a professional linguist in the history of linguistics*. The only justification would depend on the claim being both essentially correct and important. Given the extraordinarily influential (even dominant) role which Chomsky's work has uncontroversially played in the linguistics of the last half century, if the claim of massive irresponsibility is true, there is no way it could fail to be important, at least to linguists. For it would support the view, central to Postal (2004), that much of the persuasive force of Chomsky's linguistics has been achieved only via a mixture of intellectual and scholarly corruption.[1] So one would only need to focus on issues about the truth of the claim. But I do not intend to revisit directly the responsibility issues of chapter 11; anyone concerned with them can refer to the original. Such a move is in any event unmotivated, since Collins's remarks do not address *most* of the criticism in chapter 11 and none of that in other chapters (and of course nothing from Levine & Postal 2004).

Rather, the present goal is only to briefly indicate why Chomsky's ontological position, his so-called biolinguistics, is *absurd*.[2] The issues of irresponsibility and Chomsky's ontological position are closely intertwined. It is, I suggest, the hopeless quality of Chomsky's ontology which underlies the irresponsibility addressed in chapter 11. For the failures of intellectual standards I invoked involved remarks aiming to justify his ontological view. If Chomsky had a *defensible* position on this matter, he presumably would have defended it seriously over something like the four decades (see fn. 14 below) since he began advancing it. This would have required addressing the extremely strong critiques made of it; these have previously multiply accused the position of being

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I am greatly indebted to Shalom Lappin and Geoffrey K. Pullum for very helpful discussion of the issues of this article. All errors and inadequacies are mine.

1 A view even more explicit in Levine & Postal (2004).

2 Incidentally, it is not improper to issue such a characterization *prior to* justification in this article because it has been justified in much previous work cited below, which Chomsky has ignored. Section 4 documents another, ironic, reason.



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incoherent.[3] But such responses are inexistent. Instead one has throw away passages like that criticized in chapter 11 and occasional defensive remarks by *others* favorable to Chomsky's ideas. Collins's contribution here seems to fall into this latter pattern. From this perspective, these comments are mostly substantively repetitive. Perhaps though they may help familiarize an audience unaware of the previous work with some of its essential points.[4]

## 2. Oddities

It is odd for my opposite in the present exchange to be anyone other than Chomsky. For a reader might ask this. As the target of a vitriolic and shocking accusation like that addressed against Chomsky in Postal (2004: chap. 11), one appearing in a refereed volume published by one of the most prestigious university presses in the world, would *they* not seek to vigorously refute the charge? But in the intervening five years, Chomsky has, to my knowledge, not even mentioned the criticism; ditto for the uncompromising criticism in Levine & Postal (2004). By exercising his undeniable right of silence here, Chomsky leaves unimpeded the inference that he has not attempted a refutation because he cannot; see fn. 9 below for the substanceless alternative which Chomsky has adopted. In the present discussion, Collins has then chosen to defend something its own author is unwilling to.

It is equally odd that any remarks of *mine* should appear in a journal named as is this one. For my view is that not only is there no such thing as biolinguistics as this term is understood in the work Chomsky has influenced, *there cannot be such a thing*. The reasons are the same as those precluding there being a biomathematics or a biologic understood in the same way.[5] The objects treated in these latter fields are not physical objects, ipso facto not biological objects. And so it is with those treated in linguistics proper. These are natural languages (NLs), which like numbers, propositions, etc. are abstract objects, hence things not located in space and time, indeed not located *anywhere*. They are also things which cannot be created or destroyed, which cannot cause or be caused. Rather, NLs are collections of other abstract objects normally called *sentences*, each of which is a set. This view defines a contrasting ontology of NL one can, following Jerrold J. Katz, its modern developer, call the *realist view*.[6] What preceded in this

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3 See Langendoen & Postal (1984: chap. 6), Katz & Postal (1991), Katz (1996), and Postal (2003, 2004: chap. 11).

4 Anyone interested in the ontological matters touched on here is advised to consult the works of Jerrold J. Katz listed in the references. Those writings manifest substance and quality of argument at an incomparably higher intellectual level than the material criticized here.

5 Were mathematics biological, brain research might resolve such questions as whether *Goldbach's Conjecture* is true. Were logic biological, one might seek grants to study the biological basis of the validity of the principle of *Modus Ponens*. The ludicrous character of such potential research is a measure of the folly of the idea that these fields study biological things. One aspect is that a biological grounding of such fields would render their truths *contingent*, mere empirical properties of human nature, like facts about the normal range of blood pressure, etc. That clashes utterly with the recognized *necessary* nature of e.g. Modus Ponens and similar principles, which gives them inter alia their prescriptive force.

6 Katz's terminological choice was not persuasive definition. Rather (see Quine 1953: 14), *realism* names one of the three main mediaeval points of view regarding universals, that

paragraph was not a defense of the realist view, only a brief exposition of its essence.

The realist ontological position evidently contrasts utterly with Chomsky's biolinguistic one. The driving force behind my 2004 concern with the passage chapter 11 analyzed was my perception of irresponsibility in the way Chomsky had dealt with earlier criticisms of his own position advanced by advocates of the realist view. Nothing has changed since. Chomsky (2005) repeats the same sort of things at issue in chapter 11 without citation of any of the criticisms arguing his views are incoherent.

## 3. Chomsky's Biolinguistic View

The underlying substantive question of the ontological character of NL is an unavoidable topic for linguists. It concerns such issues as what sort of things NL sentences are, where they stand in the universe of objects, how they relate to other things which have a connection to NL — knowledge of NL, use of NL, physical structures involved in the learning and use of NL, etc.

To these deep and complicated issues the realist position, like Chomsky's biolinguistic one, offers basic answers. My goal here is *not* to as such defend the realist position; see e.g. Katz (1981, 1984, 1996, 1998), Katz & Postal (1991), Langendoen & Postal (1984: chap. 6), and Postal (2003, 2004: chaps. 11, 13) and Carr (1990). Rather, I seek only to (re)show that what Chomsky has called the biolinguistic view is not even a serious *competitor* for the correct ontology of NL. Such a demonstration supports the realist position only indirectly by eliminating one alternative.

Typical formulations of Chomsky's biolinguistic view of NL spanning almost a quarter century are multiply available:

(1) Chomsky (1983: 156–157)

> In contrast, a mentally represented grammar and UG are real objects, part of the physical world, where we understand mental states and representations to be physically encoded in some manner. Statements about particular grammars or about UG are true or false statements about steady states attained or the initial state (assumed fixed for the species), each of which is a definite real-world object, situated in space-time and entering into causal relations.

(2) Chomsky (2000a: 5–6)

> The approach is 'mentalistic,' but in what should be an uncontroversial sense. It is concerned with 'mental aspects of the world', which stand alongside its mechanical, chemical, optical and other aspects. It undertakes to study a real object in the natural world — the brain, its states, and its functions.

(3) Chomsky (2000b: 8)

> We can take a language to be nothing other than a state of the language

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view, often called Platonist.

> faculty. […] So let's take a language to be (say, Hindi or English or Swahili) a particular state attained by the language faculty. And to say that somebody knows a language, or has a language, is simply to say their language faculty is in that state. […] The language, in that sense, provides instructions to the performance systems. […] There is another assumption that comes along: [I]t does it in the form of what are called 'linguistic expressions'. […] The technical terminology for that is that the language generates an infinite set of expressions; that is why the theory of a language is called a 'generative grammar'.

(4) Chomsky (2005: 2)

> The biolinguistic perspective views a person's language as a state of some component of the mind, understanding 'mind' in the sense of eighteenth century scientists who recognized that after Newton's demolition of the only coherent concept of body, we can only regard aspects of the world 'termed mental' as the result of "such an organical structure as that of the brain" (Joseph Priestly).

This view is abstracted from any linguistic substance, by which I mean the sort of facts which actual linguistic research (including that by Chomsky) deals with. There is little exegesis of how this philosophical position relates to facts like those subsumed by island constraints, conditions on parasitic gaps, binding issues, negative polarity items, etc. The gap involves the sort of entities linguistics studies, entities normally called *sentences*, a term Chomsky tends to avoid in recent years in favor of substitutes like *expressions*. The issue arises of what the ontology says about such objects. Details aside, since NLs are taken as biological, associated with brains, internal to Chomsky's view, sentences have to be aspects of brains. And there the ontology immediately crashes. For whatever goes on in brains having to do with NLs, it cannot involve sentences but only *sentence tokens*.

At one time, many linguists believed that the substance of NL was formed of utterances.[7] But utterances are not sentences, but merely tokens of sentences, organism-external tokens. Under Chomsky's ontology, with sentences identified with physical aspects of brains or brain functioning, he is, like the descriptivists of decades ago, talking at best about sentence tokens, not sentences, merely *brain-internal tokens*.

Sentence tokens exist in time and space, have causes (e.g. vocal movements), can cause things (e.g. ear strain, etc.). Tokens have physical properties, are composed of ink on paper, sounds in the air, electrical impulses, require energy to produce, etc. Sentences have none of these properties. Where is the French sentence *Ça signifie quoi?* — is it in France, the French Consulate in New York, President Sarkozy's brain? When did it begin, when will it end? What is it made of physically? What is its mass, its atomic structure? Is it subject to gravity? Such questions are nonsensical because they advance the false presupposition that sentences are physical objects.

Excellent accounts of the distinction between sentence and sentence token, originally due to Peirce (1958: 423), explicated in Quine (1987: 216–217) and cited in Katz (1996: 274), are represented in:

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7 These were the descriptivists, like Leonard Bloomfield. See Katz (1981) for discussion.

(5) a. Peirce (1958: 423)

> There will ordinarily be about twenty *the*s on a page, and of course they count as twenty words. In another sense of the word *word*, however, there is but one *the* in the English language; […] it is impossible that this word should lie visibly on a page or be heard in any voice.

b. Quine (1987: 216–217)

> ES IST DER GEIST DER SICH DEN KÖRPER BAUT: [S]uch is the nine word inscription on a Harvard museum. The count is nine because we count *der* both times; we are counting concrete physical objects, nine in a row. When on the other hand statistics are compiled regarding students' vocabularies, a firm line is drawn at repetitions; no cheating. Such are two contrasting senses in which we use the word *word*. A word in the second sense is not a physical object, not a dribble of ink or an incision in granite, but an abstract object. In the second sense of the word *word* it is not two words *der* that turn up in the inscription, but one word *der* that gets inscribed twice. Words in the first sense have come to be called tokens; words in the second sense are called types.

There is of course a possible NL ontology which recognizes sentence tokens but no sentences. That was in effect the view of those who took linguistics to be about utterances. And a parallel view could claim that NLs involve nothing but brain-internal tokens. *But that cannot be Chomsky's view*. Moreover, while such an ontology would be thoroughly untenable, it would be far superior to Chomsky's, since it could have at least the virtue of coherence. The most palpable reason his ontology lacks even that is this. While the assumptions of (1)-(4) determine that NL can only yield brain-internal tokens, Chomsky has for more than half a century insisted on a property of NL entirely inconsistent with taking NL to involve brain-internal (or brain-external for that matter) tokens. Specifically, he has repeated dozens of times as in (3) the claim that NL is discretely infinite:

(6) Chomsky (1957: 13)

> Each sentence is representable as a finite sequence of these phonemes (or letters), though there are infinitely many sentences.

(7) Chomsky (2000b: 51–52)

> For example, the most elementary property of the language faculty is the property of discrete infinity; you have six-word sentences, seven-word sentences but you don't have six-and-a-half word sentences. Furthermore, there is no limit; you can have ten-word sentences, twenty-word sentences and so on indefinitely. That is the property of discrete infinity.[8]

Chomsky's ontological position then incoherently asserts the following:

(8) a. An NL is a physical state of the language faculty conceived as a biological object, an organ, an aspect of a brain.
b. That organ (brain) state yields an infinite set of expressions.

[8] Talk of six-and-a-half word sentences is just game playing because there are no half words.

However, anything that a physical system yields is physical. Consider a liver and its production of bile, a heart and its production of pulses of blood; all physical and obviously finite. And so it must be with any cerebral physical production. There is for Chomsky thus no coherent interpretation of the collection of brain-based expressions being infinite, since each would take time and energy to construct or, in his terms, generate (see below for common equivocation on this term), store, process, or whatever.

That taking NL to be both biological and infinite is incoherent was observed in Langendoen & Postal (1984: chap. 6) in direct criticisms of Chomsky's positions, characterized there via the terminology 'radical conceptualism'. This past century critique illuminates the substantive repetitiveness of the present remarks, motivated by Chomsky's refusal to confront the matter.

(9) Langendoen & Postal (1984: 131–132)

> Even for an attested NL like English, the claim that a grammar, even a psychogrammar, generates mental representations immediately creates otherwise unnecessary fundamental problems. Evidently, either standard or radical conceptualist must minimally assume that any actual human mind or brain is finite, and thus that its very nature limits the objects which are in fact representable therein, for trivial non-linguistic reasons. Consequently, if psychogrammars generate mental representations of sentences and mental representations are, as the term suggests, things actually present in real minds, in something like the sense in which, say, data or computations are present in real computers, the radical conceptualist position claims that NL grammars have a finite output, one containing no representation of cardinality greater than some finite *k*. This is inconsistent not only with the traditional generative position but with claims in the very works where radical conceptualism is advocated that the domain of grammar is infinite.

The incoherence of a *biolinguistic* view claiming NL is infinite was specifically observed as well in Katz & Postal (1991: 547–548), Katz (1996), and Postal (2003, 2004: 300–301). Although constantly returning to foundational issues, Chomsky has never mentioned any of these criticisms either.[9]

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[9] Worse, he indulges in the make believe that not only are his foundational assumptions viable, but that those (always unnamed) who purport to reject them in fact nevertheless *adopt them without realizing it*.

(i) Chomsky (1999: 33)

a. We study these objects more or less as we study the system of motor organization or visual perception, or the immune or digestive system.

b. It is hard to imagine an approach to language that does not adopt such conceptions, at least tacitly. So we discover, I think, even when it is strenuously denied, but I will not pursue the matter here.

These assertions imply that *inter alia* the authors of the works cited in e.g. fn. 3 tacitly adopt Chomsky's incoherent foundational assumptions without knowing it. But those authors have made these assumptions explicit, forcefully and repeatedly rejected them and argued for their incoherence.

Six years later, the same assertion is not only made but strengthened to a claim that the need for coherence would actually require those rejecting his ontology to adopt it.

The incoherence of Chomsky's position is worth putting slightly differently. To say a collection is discretely infinite is to say its members can be put in one to one correspondence with the members of one of its own sub-collections and also with the positive integers. What Chomsky's ontology then asserts is that the human brain component he calls FL or a particular state of it defining a particular NL embodies as aspects a collection of things which can be put in such a correspondence with the integers 1, 2, 3,…. But there can't even begin to be enough of *anything* in a human brain or its functioning to ground such a correspondence. It is no accident then that across the decades of adumbrating his biolinguistic view, Chomsky has never even sought to specify the nature of supposedly biological entities which manage to have a transfinite cardinality.

At least three different professional linguist supporters of Chomsky's biolinguistic view who have in effect addressed this criticism of the incoherence of his position in conversation or correspondence with the present author have advanced the following defense.[10] This grants that while a finite physical system cannot *actually* assemble an infinite number of expressions as elements of the physical world, it can nonetheless theoretically accomplish infinite generation under an *idealization* in which time and physical constraints are eliminated. This possibility putatively exists because the physical structure embodies a recursive system of rules, which jointly specify an infinite collection if given the 'resources'.

Such an account involves though illegitimate equivocations on the concepts idealization, recursive, rule and generate. The invocation of the first has nothing in common with its sensible use, as when in making certain practical physical calculations one idealizes to a system in which there is no friction. The 'idealization' here is rather parallel to one which claims the solar system has an infinity of planets (e.g. of tinier and tinier sizes filling the spaces between the usually cited planets). Actually, this silly 'idealization' is sounder than the one at issue, since at least, all the putative (though nonexistent) entities are physical.

Second, nothing physical is a rule or recursive. Physical things are destructible, recursive functions not. At best a physical structure can encode rules. But that involves an interpretation of physical things as representing particular abstractions, something Chomsky's explicit brain ontology has no place for. And, as discussed in Postal (2003), Chomsky's view here amounts to a pun on the word *generate*. Standard formal theory use of this item refers to a relation between two sets, sets being nonphysical objects; see e. g. Partee, ter Meulen & Wall (1993: 435) and below. Such things do not exist in space or time,

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(ii) Chomsky (2005: 2)

> The decision to study language as part of the world in this sense was regarded as highly uncontroversial at the time, and still is. A more careful look will show, I think, that the arguments advanced against the legitimacy of the approach have little force (a weak thesis) and that its basic assumptions are tacitly adopted even by those who strenuously reject them, and indeed must be, even for coherence (a much stronger thesis).

But unlike the earlier realist documents I have cited and unlike the present one, Chomsky's claims here are of course entirely unsupported by argument.

10 Some suggested that Chomsky has at least implied something like this defense in conversations and lectures. But since such a formulation has not to my knowledge appeared in any publication, I do not attribute it by name to anyone.

thus are not subject to the laws of nature. That is why assumptions that some are infinite raise no problems of coherence. However, in quotes like (3), *generate* is used in the sense of a relation between a physical object and some physical outputs, roughly as a synonym of *produce*. But every physical production takes time, energy, etc., and an infinite number of them requires that the physical universe be infinite and, internal to Chomsky's assumptions, that the human brain be.[11]

The inadequate defense of Chomsky's infinite brain output ontology hinges on a failure to specify seriously the ontological nature of NL sentences (expressions). The view essentially also equivocates on both the notions expression/ sentence and grammar. On the branch where a putatively biological NL is a physical thing, the expressions it 'generates' are also physical; each has time and space coordinates; they have to be some kind of *tokens*. But on the branch of the equivocation where the biological NL 'ideally' generates an infinite collection, most of the 'expressions' in the collection cannot be physical objects, not even ones in some future, and the NL cannot be one either.[12]

Under the supposed idealization, the elements said to be generated divide into two distinct types. Those limited enough in size and small enough in number to be given a physical interpretation can at least be made grossly consistent with a biological perspective (by conflating sentence and sentence token). But almost all sentences are too complex and too numerous even for that and thus must be given a distinct interpretation. In effect, a distinction is made between real sentences and merely 'possible' ones, although this 'possibility' is unactualizable *ever* in the physical universe. According to the biological view, that is, the supposedly 'possible' sentences are, absurdly, actually biologically impossible. Thus internal to this 'defense' of Chomsky's biolinguistic view, the overwhelming majority of sentences *cannot be assigned any reality whatever internal to the supposed governing ontology*. This means the ontology can only claim NL is infinite because, incoherently, it is counting things the ontology cannot recognize as real. In effect, despite a perhaps enticing conceptual packaging, in talking about the idealization and infinite output, the attempted defense of Chomsky's biolinguistics abandons its physicalist (biological) basis but does this *surreptitiously* by adopting inconsistent assumptions about the nature of expressions, NLs, rules, and grammars.[13]

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[11] Were it true that Chomsky's linguistics, which claims that NL is both biological (hence an element of the physical universe) and yet infinite, has shown that NL is infinite, then, it *has shown that the physical universe is infinite.* Would a physicist even bother to laugh!

[12] Everett (2005) claimed rightly that if NL were biological, syntactic trees should be visible in CAT scans. Anderson & Lightfoot (2005) reject this idea, speculating that it "seems to reflect more on the adequacy of current brain imaging techniques than it does on the nature of language" (p.81). But this defensive speculation is ungrounded at best. They give no idea of what physical structure they imagine NL sentences have that could be scanned, no indication of where the supposed limitations of current scanning technology lie, hence none about what scanning progress is supposedly required. Electron microscopes can provide images of individual atoms. As in Chomsky's writings, missing is an appreciation of the logical gap between physical and abstract objects. That renders the idea of physically scanning sentences as illusory as that of scanning *Syntactic Structures,* contrasted with the perfectly possible scanning of one of its tokens (copies).

[13] The fallacious character of the argument considered here was in effect already dissected in ~~a~~

One could put the matter still differently. The proposals at issue here defend a biolinguistic view which incorporates the infinity claim only under a counterfactual. That is, if there were infinite time and the human brain were infinite, then a biological production of tokens could have an infinite output. But real inquiry is not concerned with things which could exist if the world were different than it is. One cannot justify a claim that there are perpetual motion machines via a counterfactual conditional.

Almost as if to *emphasize* the incoherence of his view of NL, Chomsky has recently stated:

(10) Chomsky (2005: 4)

> Universal Grammar (UG) […] must provide […] means to construct from these lexical items the infinite variety of internal structures that enter into thought, interpretation, planning, and other human mental acts, and that are sometimes put to use in action, including the externalization that is a secondary process.

Here Chomsky says flatly that an *infinite* variety of internal structures *enter into thought and other human mental acts*. But that entails that human mental acts are infinite, an incoherent consequence for anyone who, like Chomsky, takes the human mind to merely be some aspect of the very finite human body and lifespan; see (4). Again, Chomsky systematically confuses NL sentences, which are abstract and possessed of no location internal or external to minds, with some kind of mental representation or physical coding of (a very finite) subset of them.[14]

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Katz (1996) (irrelevantly in terms of the type/token issue for *mathematics*). Due to space limitations, I only excerpt one passage.

(i) Katz (1996: 275)

> Less canny nominalists than Quine and Goodman have said that the term 'expression' means 'possible expression token'. There are, of course, now enough tokens for all the mathematical objects, but only because of the non-actual ones among them. The non-actual ones make the numerical difference. But, from a nominalist standpoint, a non-actual possible token is an unacceptable entity if anything is. Expressions destined to occur at some time and place in the future, even in the distant future, can be legitimately regarded as tokens, as it were, sub specie aeternitatis. But expression tokens fated never to occur — because there simply isn't enough matter in the universe — cannot be legitimately regarded as tokens in any sense. Since the customary notion of a token is that of a concrete thing which instances a type, a never to be actual token is, strictly speaking, a contradiction in terms.

[14] Tracing the *origin* of Chomsky's biolinguistic foundational doctrine is hard. Katz (1981) showed to be mythical the widespread idea that this dates to the *beginning* of his linguistic work by noting (i):

(i) Chomsky (1955: 21–22)

> I think that there is hope of developing that aspect of linguistic theory being studied here on the basis of a small number of operational primitives, and that introduction of dispositions (or mentalistic terms) is either irrelevant, or trivializes the theory.

Katz observed that this entirely non-mentalistic view, inconsistent with all of Chomsky's recent decade talk of NL as mental/biological, is absent from the published 1975 version of

One other point is worth making about any claim that NL is both biological and infinite. Chomsky claims (see fn. 9) that in his perspective one studies NL like biologists study the digestive system; see fn. 9. This is ludicrous in many ways, notably as an account of Chomsky's own work in linguistics, which nowhere resembles biological research. To see this clearly, one should consult actual pieces of biological science. One might (thanks to David E. Johnson for this reference) look at Hokkanen (1986), a study of how physical constraints and laws limit the maximum weight of an animal living on the land portion of the earth. Similarities with any work by Chomsky appear to be zero.

I am unaware of any reports by Chomsky of his use of x-ray machines, microscopes, tissue samples, and so on. So in total contrast to actual biological science, in four decades he has not specified *a single physical property* of any linguistic object, not surprisingly from a realist point of view, since they have no such properties. The illusory nature of Chomsky's claim is palpable with respect to claims of infinity. What is supposed to be the *empirical* evidence for it? Chomsky provides nothing of the sort.[15] Moreover, he could not, since, as argued above, the claim of biological infinity is incoherent and there can be no empirical evidence for an empirical impossibility. See the discussion of the *Veil of Ignorance Argument* in Katz (1990) and Katz & Postal (1991).

While the clash between Chomsky's jointly held views that NL is biological and infinite is the most blatant incoherence in his ontology, it is by no means the only one. Consider:

(11) Chomsky (2005: 11)

> An elementary fact about the language faculty is that it is a system of discrete infinity. Any such system is based on a primitive operation that takes *n* objects already constructed, and constructs from them a new object: [I]n the simplest case, the set of these *n* objects. Call that operation Merge.[16]

Merge is according to Chomsky the essence of human syntax. And yet he describes it in *set-theoretical terms*. See also:

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this work, which postdates Chomsky's adoption of the psychological/biological point of view. The excision was one basis for the myth mentioned above. While Chomsky's *right* to exclude (i) is unchallengeable, what function did it serve other than obfuscating the real history?

A key element of Chomsky's biological view development is found here:

(ii) Chomsky (1972: 169, n.3)

> Since the language has no objective existence apart from its mental representation, we need not distinguish between 'systems of beliefs' and 'knowledge,' in this case.

Besides its clash with (i), view (ii), central to all of Chomsky's ontological thinking since, was merely dogmatically asserted, not argued or defended.

15 The most he ever offers is discourse like (7), which doesn't qualify as any kind of science. Pullum & Scholz (2005) call claims like that in (7) *the standard argument*, and argue it is fallacious.

16 The unclear claim that discretely infinite collections are 'based' on some operation is completely unsupported. They can, as in e.g. Peano arithmetic, be regarded as collections characterized by some finite set of axioms.

(12) Chomsky (2005: 15)

> With minimal technical apparatus, a syntactic object X with label A can be taken to be the set {A, X}, where X is itself is a set {Z, W}.

But even a set of concrete elements, say two pencils, cannot be found in time or space, is not subject to gravity, cannot be destroyed, etc. The space/time coordinates of each pencil could be specified, but not those of the set itself. The pencils can be photographed, x-rayed, weighed, etc., but the set cannot be physically recorded. See Quine (1987: 217–218) and Katz (1996) for further discussion of the abstract nature of sets (classes). Therefore, since Chomsky's current foundational view stipulates that an NL is an organ state, incoherence emerges from the fact that he takes NL to be set-based. For an organ state, an aspect of the spatiotemporal world, cannot 'generate' an abstract object like a set. So Chomsky's ontology provides no way in which his biological notion of NL could coherently relate to the set-theoretical claims he advances about syntactic structure. No aspect of NL can manifest both a set structure and be a biological object, as the latter have a physical structure not a set-theoretical one. Contradictorily, Chomsky is stating that sentences both do and do not exist in space/time. Viewed slightly differently, in advancing set-theoretical accounts of NL structure, Chomsky again just abandons his own *putative* ontology and proceeds, but only incoherently, as if he had a realist one which permitted him to sensibly view sentences in set-theoretical terms. That is, his ontology is evidently so awful that even *he* pays no attention to it when actually considering real linguistic matters.

The aspect of Chomsky's ontology just criticized has also been defended by supporters of Chomsky's position in private discussion. The defense claims in effect that Chomsky's appeal to formal science (e.g. his set-theoretical discussions) are no different than e.g. a physicists appeal to mathematical formulations. In using formal structures, the defense runs, Chomsky is no more committed to claims that NL is formal than a physicist's use of mathematics involves a commitment to the abstract nature of physical reality. The flaw in this defense is that in fact Chomsky's appeal to formal science in his characterization of NL and a physicist's appeal to formal science via e.g. invocation of various complex mathematical equations are entirely distinct. For the physicist never *identifies* his equations with the physical structures characterized or conversely. But as in (11), Chomsky takes the putative biological entity to be constructing abstract objects, sets. This is entirely distinct from using some piece of formal science to describe something nonformal. And this conflation by Chomsky of formal and physical objects is one of determinants of the incoherence of his ontological position.

Space precludes discussion of other types of incoherence in Chomsky's bio-linguistics. See Katz & Postal (1991).

## 4. Irony

The untenable aspects of Chomsky's ontology have a mildly ironic character since its inadequacy *was in effect pointed out by Chomsky himself almost half a century* *ago*:[17]

(13) Chomsky (1961: 7–8)

> A grammar, in the sense described above, is essentially a theory of the sentences of a language; it specifies this set (or generates it, to use a technical term which has become familiar in this connection) and assigns to each generated sentence a structural description. It is not, however, a model of the speaker or hearer. It neither synthesizes particular sentences, as does the speaker, nor does it recognize the structure of presented sentences, as does the hearer. It is quite neutral as between speaker and hearer in this respect. […] Notice that although the grammar $G_i$ mastered by the user of a language is of course finite, it is not to be expected (and, in the case of natural languages, it is not in fact true) that a finite automaton *can* be constructed which will be able to accept (or generate) all and only the sentences generated by $G_i$, or which will be able to 'understand' just these sentences (i.e., give the structural descriptions assigned to these sentences by $G_i$ as outputs, when these sentences, but not others, are provided as inputs). This is no stranger than the fact that someone who has learned the rules of multiplication perfectly (perhaps without being able to state them) may be unable to calculate 3, 872 x 18,694 in his head, although the rules that he has mastered uniquely determine the answer. […] It would be *absurd* [emphasis added — PMP] to require of the grammars of (1a) that their output be the kinds of sets of strings, or sets of structural descriptions, that can be handled by strictly finite automata, just as it would be absurd to require (whether for the purposes of mathematical or psychological researches) that the rules of arithmetic be formulated so as to reflect precisely the ability of a human to perform calculations correctly in his head. Such a requirement would have neither theoretical nor practical motivation.

Among the many assumptions in passage (13) are (14a–e):

(14)
a. There are things called sentences;
b. these form a set;
c. grammars are *theories* of these sentences;
d. grammars *generate* the sets of sentences;
e. some view is absurd.

While these 1961 remarks of Chomsky's were essentially reasonable, one can still discern definite flaws. It is erroneous to equate theories and Turing machines. Theories are sets of *statements*, not elements of a computer program. One consequence of their faulty identification is that (14c–d) confuse the issue of whether NL grammars are generative, proof-theoretical or model theoretical; see e.g. Pullum (2007), Pullum & Scholz (2001, 2005), Postal (2004: chap. 6). From the current perspective, this is a marginal issue, which I say no more about.

The major inadequacy in (13) was that while recognizing the reality of sentences and collections of them, it offered no account of the kinds of things Chomsky took sentences and NLs to be or what they are made of? This gap has

[17] I had forgotten about this passage when writing Postal (2004), which did not cite the 1961 work.

been persistent; see Postal (2003: 249).[18] Should they be assumed to be psychological, biological, physical, abstract? And why? But his absurdity claim had definite if partial relevant implications.

In 1961 Chomsky saw as absurd any requirement that NL grammars reflect the limitations of human beings, e.g. the limitation of being able to parse expressions only up to certain lengths or complexities. This was good sense. Least importantly, those limitations reflect not properties of NL, but properties of a type of organism and, as Chomsky noted, could be partly altered, e.g. by providing external memory. There is nothing specifically linguistic about the fact that people cannot parse or produce sentences which are a million words long. The same thing also precludes e.g. additions of sequences of numbers with a million elements. It is just the inherent biophysical limitations of finite organisms, which bound every physical world activity. More importantly, if NLs are collections of abstract, set-theoretical objects, there is no coherent way that any physical limitations of human beings could determine their nature.

Nonetheless, Chomsky's later ontological position, as alluded to in Postal (2004), incorporates what Chomsky called absurd in 1961. Moreover, it does this in the absence of any argument for abandoning his earlier view, *which Chomsky has never provided*. The later position incorporates the absurdity simply because it identifies an NL with an aspect of human nature, necessarily attributing to it any and all limitations of the latter.

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[18] One finds only remarks like these:

(i) Chomsky (2000a: 160)

> The representations are postulated mental entities, to be understood in the manner of a mental image of a rotating cube, whether it is a consequence of tachistoscopic presentations or a real rotating cube, or stimulation of the retina in some other way; or imagined, for that matter.

(ii) Chomsky (2001: 91)

> No questions arise about the ontological status of the set of expressions {Exp} generated by L; its status is somewhat like that of potential visual images or plans for limb motions.

But there is no sensible comparison between the subjective, episodic, individual mental things Chomsky refers to and e.g. an English sentence, which is not subjective, not episodic and which can be known by innumerable people. My plans for limb motions are temporally limited and mine, but no English sentence is either. Katz put the matter quite nicely:

(iii) Katz (1996: 292)

> Different speakers who intuit the fact that a sentence is well-formed or ambiguous are epistemically related to one and the same grammatical object. That object is no more one speaker's than another's. The sentence is independent of all the speakers. In contrast, different people who introspect the colour of an after-image, the intensity of a pain, or the displacement of a figure in double vision are epistemically related to different things.

Basically, (i) and (ii) simply illustrate Chomsky's failure to rationally distinguish type and token.

## 5. Sentences/Expressions as Unreal

The contradiction between Chomsky's claims that NL is both infinite and biological is, I speculate, what underlay his truly desperate claim in the passage criticized in Postal (2004: chap. 11) that *sentences are not real*.[19] I break the claim into two parts:

(15) Chomsky (1999: 34)

a. These [that is, expressions, the language faculty outputs — PMP] are not entities with some ontological status; they are introduced to simplify talk about properties of [the language faculty], and they can be eliminated in favor of internalist notions.

b. One of the properties of Peano's axioms PA is that PA generates the proof P of '2 + 2 = 4' but not the proof P' of '2 + 2 = 7' (in suitable notation). We can speak freely of the property 'generable by PA', holding of P but not P', and derivatively of lines of generable proofs (theorems) and the set of theorems without postulating any entities beyond PA and its properties.

Consider first (15a). It might seem for a microsecond that the contradiction between NLs being infinite collections of sentences and NLs being (states of) organs vanishes if there really are no sentences at all. But (15a) doesn't even begin to resolve that contradiction. Since Chomsky takes FL and its states to be finite, were there no real expressions, there would remain nothing in his ontology to have the cardinality of the integers. To say there are no sentences/expressions is to say the collection of them is the null set, which cannot be put into one to one correspondence with any other set. Or, slightly differently, (7) above says there are six word, seven word, ten word, twenty word, etc. sentences, with no bound. That is supposedly why NL is infinite. But (15a) says sentences aren't real. Thus NL in Chomsky's terms can again only be infinite because he is counting unreal

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[19] For while he has never addressed this contradiction seriously, he could not have been unaware of it. He himself documents such awareness. The issue arose in a question after the 1996 lecture published as Chomsky (2000b); see Postal (2003).

(i) Chomsky (2000b: 62–63)

*QUESTION:* Infinite use of finite means; doesn't it entail an inconsistency? Isn't the model of an infinite potential in, a finite organ inherently inconsistent?

*CHOMSKY:* That was the problem until about a century ago. It did look like an inconsistency. One of the important discoveries of modern mathematics is that it isn't an inconsistency. There is a perfectly coherent sense to notion of infinite use of finite means. That is what ended up being the theory of computability, recursive function theory and so on. […] So, yes, it looks like an inconsistency but it simply isn't. There's a very simple account of it that is not inconsistent. I can't go into it any further here.

Chomsky only addressed the irrelevance of how a finite abstract procedure can specify an infinite set. But the questioner's *organ* reference shows he addressed not that issue but rather the sense that could be made of the elements of an infinite set in organ state terms. Chomsky's implication that recursive function theory solves that problem was fakery, as that piece of formal science can say nothing about the issue.

things, utilizing 'reasoning' that equally demonstrates that there are an infinite number of ghosts in e.g. Buckingham Palace.

Issues of infinity aside, (15a) states that all linguistics-internal references to sentences can be eliminated in favor of purely proof-theoretic talk about grammars. If so, one would be able to translate e.g. the following into such terms, permitting the so-called elimination Chomsky invoked:

(16) The sentences *Steve likes monkeys* and *I hate monkeys* end in the same word.

Obviously such true descriptive statements are the foundation of grammatical work. Or consider:

(17) The grammatical English example *The letter was never shown her* falsifies a common view of English syntax.

This expression is the sort appealed to in linguistics to argue for and against (see Postal, 2004: 240–242) specific hypotheses. How is it to be eliminated in favor of whatever Chomsky is willing to say exists? The problem is that (16) talks about partial identity between two things Chomsky claims are nonexistent, while (17) claims that a particular object whose existence he denies is inconsistent with general properties of a right grammar. How can parts of nonexistent things be identical? How can something unreal be used to argue for or against some claim? Can ghosts falsify some theoretical physical claim?

Basically then, internal to Chomsky's views, (15a) is prima facie gibberish. In all cases like (16) and (17), the burden of proof that the situation is not that falls entirely to Chomsky. And, typically, he has nowhere even tried to meet such a burden.

Turn briefly then to (15b). One must presume that by analogy, this bizarre claim is supposed to support that in (15a). Ignoring the pointless subjectivity ("We can speak freely"), all (15b) states is that one can view the derivation of theorems in formalized arithmetic proof-theoretically. No doubt. But as such that gives no ground for why the derivation of '2 + 2 = 4' and non-derivation of '2 + 2 = 7' rather than e.g. the opposite pair of derivations is desirable. Viewed proof-theoretically, these formulae are meaningless. It is potentially misleading that they are given in a notation which everyone understands how to link to statements about *numbers*. For according to (15b), numbers are exactly the entities distinct from PA and its properties which need not be posited. But if there were no numbers, there would be no (standard) interpretation of the PA formalism, no question of the truth of any of the theorems and hence no significance to what strings are derivable or not. Significance is only given by the notion truth, which is not proof-theoretic in general (although there are (complete) systems where derivability and truth are coextensive). In short, looked at in terms of interpreted mathematics rather than as an uninterpreted formalism, Chomsky's claim about 'without positing any entities' is absurd.

Moreover, the notion 'expression' in a generative system is identified with the last lines of generated derivations/proofs. How can there even be derivations/proofs if there are no lines? But if there are such lines, it is incoherent to state that expressions which correspond to last lines in proofs, are unreal. If they are, there are no derivations and the biolinguistic ontology makes no contact with linguistic reality at all.

## 6. Chomsky's Ontology: Summary

The inescapable conclusion is that Chomsky's ontological position is simply not serious. His twenty-five year history of ignoring the need to show how there is any consistent position claiming that NL is both biological and infinite highlights the point. I see no way to interpret his silence on this matter in the face of repeated published claims of incoherence other than as in Postal (2004: chap. 11), that is, as utterly irresponsible.

## 7. Collins's Remarks

Finally, I turn John Collins's remarks. These exist because of two different types of relevant error in Postal (2004: chap. 11). The first was my having bothered to comment on Chomsky's analogy between grammars and issues of axiomatized arithmetic in (15b) and, worse, to have pursued that to the point of invoking Gödel's incompleteness theorems. This was attempted overkill on my part; as in the preceding sections, the absurdity of Chomsky's ontology and his irresponsibility in dealing with critiques of it are perfectly documentable without mentioning anything about such theorems.

The second and graver error was my statement:

(18) Postal (2004: 303)

> So, if Chomsky's remarks are correct 'not only would it be impossible to *prove* Gödel's incompleteness theorems, it would be impossible to even *formulate* them.

This was an awful and misleading formulation, and Collins is entirely correct to dismiss it as erroneous. Certainly, Collins is right that Gödel's proof was entirely formal, proof-theoretic. That is the basis of its extraordinary force. Because he showed how to represent statements *about* arithmetic, that is, meta-mathematical statements, as statements in the formalism of arithmetic (via the device of Gödelization of the symbolism of logic utilized). So certainly, contrary to (18), Gödel's incompleteness theorems are *formulated* independently of reference to a collection of truths of arithmetic.

Here is how two logicians put it:

(19) a. Quine (1987: 85)

> He [Gödel — PMP] showed how, given any proper proof procedure, to construct a sentence in the notation of elementary number theory that says of itself, in effect, via Gödel numbering, that it cannot be proved. Either it is false, and provable, God forbid, or true and not provable; presumably the latter. One could strengthen the proof procedure by adding this stray truth as a further axiom; but, by a repetition of the argument, there will always be others.

b. Hintikka (2000: 39)

> What the incompleteness of formalized first-order arithmetic shows is the need of distinguishing sharply between the truth of arithmetical statements and their provability. […] Gödel's result […] showed that the model-theoretical notion of arithmetical truth is not exhausted by the proof-theoretical notion of formal provability.

One observes that while both Quine and Hintikka characterize the proof in proof-theoretic terms, in indicating its significance they of course reference the notion of truth. Without that reference, the proof is an uninterpreted piece of arithmetical reasoning. That is, the significance of the proof-theoretic demonstration arises because there is a notion of truth of arithmetic independent of such demonstrations.

What I intended (but failed utterly) to express adequately was that it would be impossible to state the *significance* of Gödel's purely proof-theoretic demonstration without a comparison of the incomplete collection of provable formulas with the collection of true formulas. Viewed in purely proof-theoretic terms, it is of no significance that some Gödel number is not derivable from some set of axioms. What renders it of enormous importance is that under the intended interpretation, the unprovable formula states a truth.

Once the badly misleading implications of my remark (18) are cleared away though, the mistake therein turns out to be irrelevant to the general point I was making. For what Chomsky's invocation of Peano arithmetic in (15b) was supposed to show was that it made sense to deny the reality of NL sentences, which could somehow be reduced to talk of states of FL just as it made sense to deny the reality of numbers, which could somehow be reduced to talk about arithmetical axioms. But if the analogy shows anything, it is the exact opposite. If there are no numbers independent of sets of axioms, there are no truths about them. And statements of the meaning of Gödel's proof, like e.g. Quine's or Hintikka's, become senseless. For the only way one can have the meta-mathematical conclusion that formalized arithmetic is incomplete, is by appealing to true formulas which are not provable, which requires true formulas…showing that derivability is distinct from truth. Therefore, since sensible discussion of Peano's axioms and their theorems inevitably invokes the notion of true statements about the numbers, by parity of reasoning, sensible discussion of (generative) grammars and the derivations they generate would have to involve the relation between those derivations and something else, presumably, actual NL sentences. Just as it makes no sense to think of Peano's axioms exclusively as an uninterpreted formalism, it makes none to think of NL grammars as such.

I conclude then about Collins's remarks as follows. His genuine determination of a serious error in Postal (2004: chap. 11) has no bearing on the adequacy of Chomsky's ontology and almost none on the question of the responsibility of the passage criticized by me there.

The last claim is reinforced by the fact that the attempt to absolve Chomsky's passage of irresponsibility on Collins's part must fail due to its very structure. My Chapter 11 indicated that the putatively irresponsible passage involved *twenty* successive sentences. Each was listed and analyzed, and claims of irresponsibility were advanced about many of them. The point which Collins defends in this journal only surfaced *in sentence fifteen*. But nothing Collins says addresses anything about the others, nor anything from the other chapters. In spite of that, the last line of his submission states:

(20) Collins (2009: 104)

> Postal's remarks are certainly not the most irresponsible in the history of linguistics; equally, they go no way to show that Chomsky's remarks are either.

One can only read the phrase 'go no way' in (20) as expressing the author's belief that his remarks fully refute the charge of chapter 11. But in the absence of discussion of what preceded sentence fifteen, that is evidently impossible.

To conclude, nothing interferes with the conclusion that Chomsky's promulgation of his ontological views perfectly instantiate what was called 'junk linguistics' in Postal (2004).

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