

## Position Paper

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# What are Languages? A Biolinguistic Perspective

**Abstract:** The goal of the present contribution is to explore what kinds of objects languages are from a biolinguistic point of view. I define the biolinguistic point of view as a naturalistic study of languages and I show that from this point of view, languages are human language organs, that is, they are natural objects. However, languages change over time; therefore, they are also historically modified objects. Considering that natural organisms are historically modified natural objects, I look for inspiration in evolutionary theory to better specify what kinds of objects languages are and how they change and diversify. I conclude that every language is a ‘unique evolutionary history’ within a restricted space of design. This conclusion means that although the structure of languages reveals aspects of formal elegance and aspects of functional efficiency, there are no arguments to state that these aspects are manifested more or less intensely in some languages than in others. Then their formal and functional aspects are part of what is common to all languages, while variable parts of language are a reflection of the essentially historical nature of the lexical interface between the components of our language organs.

**Keywords:** biolinguistics, linguistic theory, philosophy of language, parameters, language change, typology, linguistic diversity

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## 1 Introduction

Everyone is inclined to think of languages as complex entities that are difficult to acquire, that vary from place to place, and that are inextricably connected to social norms and culture. As Boeckx (2010, 5) points out, the biolinguistic point of view inspired by Chomsky during the last 50 years has made us drastically change our perceptions (although not without resistance), suggesting that if we really want to make a serious scientific advancement in the study of language, we must understand it as something that is acquired effortlessly, that is shared by the entire species, and that is fundamentally uniform across varying cultures and social rules.

It might seem, then, that a biolinguistic approach should put languages aside and focus on the species-common biological endowment for language. This is true to a certain degree, but represents a dangerous simplification.

My main goal is to explore the implications of a biolinguistic point of view for our knowledge about the nature of what we call Russian, Chinese or Spanish—that is, to explore what kinds of objects languages are from a biolinguistic point of view. I will define the biolinguistic point of view more accurately, and I will show that from this point of view, languages are human language organs. If languages are human language organs, then they are natural objects. However, languages change over time; therefore, they are also historically modified natural objects. Considering that natural organisms (such as animals or plants)

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are also historically modified natural objects, I will look for inspiration in evolutionary theory to better specify what kinds of objects languages are and what predictions we can make about their capacity for variation in space and time.

## 2 What Kind of Linguistics is Biolinguistics?

Of course, simply to formulate the question above as such already implies that biolinguistics is a kind of linguistics. And indeed, this is the first idea I want to address: that biolinguistics is a kind of linguistics. What this means is that I do not see biolinguistics as a mixture of biology and linguistics or as an application of biology to the study of languages (if such things could be possible at all). Rather, what I mean is that *biolinguistics* is the name we give to a kind of *linguistics* (= ‘the study of languages’) that forms a part of natural science. Or, in other words, biolinguistics is the discipline that studies human languages from the viewpoint of natural science.

Therefore, when I claim that biolinguistics is a kind of linguistics, I intend not to deny that biolinguistics is a biological study of language but only to point out that the expression *biological study of language* should be understood not as *the application of (current) biology to the study of language* but as *the amplification of biology so that it can study languages*. This means that biolinguistics should be considered an abstract layer of biology, a previous step for the necessary integration of the study of language in current natural science.

When I speak of degrees of abstraction in sciences I am actually talking about scales of complexity in the sense of Murray Gell-Mann (1994). If we consider the relationship between physics, chemistry and biology, we can say, according to this author, that physics is more fundamental (less complex) than chemistry, because the laws of chemistry are derivable from physics, “provided the additional information describing suitable chemical conditions is fed into the equations” (Gell-Mann 1994, 110-111). Moreover, those conditions are special in the sense that they do not hold throughout the Universe. Meanwhile, biology is even more complex: “[a]n enormous amount of specific additional information must be supplied, over and above the laws of physics and chemistry” (Gell-Mann 1994, 114) to characterise biological terrestrial phenomena, what allows us to conclude that “the science of biology is very much more complex than fundamental physics because so many of the regularities of terrestrial biology arise from chance events as well as from the fundamental laws” (Gell-Mann 1994, 115).

The emergence of the nervous system, of the brain, of the human brain and of the language faculty added huge amounts of additional special information to feed into the fundamental equations of physics, and caused the subsequent emergence of disciplines that address these areas of complexity (neurobiology, psychology, linguistics, and other cognitive sciences). As Gell-Mann suggests, “the enterprise of science involves investigating those laws at all levels, while also working, from the top down and from the bottom up, to build staircases between them” (Gell-Mann 1994, 112). Biolinguistics has both missions: investigating the “additional information” of language structure and, at the same time, contributing to the building of staircases in search of unification and principled explanation.

From this point of view, it seems clear then that languages, these things that we call, for example, Russian and French, are not improper objects of study for this discipline but, on the contrary, are in fact its proper and main objects of study. Asking biolinguistics to study the human faculty of language directly while ignoring languages would be akin to asking biology to study life without studying living organisms. Obviously this would be impossible and absurd. But, curiously, the prospect does not seem as impossible and absurd to those of us in the domain of language. It could be said in principle that the biological study of language should be conducted by analysing the brain and the genes and that the study of Russian or French is a matter to be kept separate. However, this is a common mistake that the present contribution seeks to avoid.

The two main traits that characterise biolinguistics as a science in relation to other types of linguistics are methodological naturalism and internalism.

### 3 Methodological Naturalism

The term methodological naturalism simply implies that biolinguistics is a kind of linguistics that uses the same methodology as do the natural sciences. This methodological program does not justify in itself that we call the discipline *biolinguistics*. If anything, it would justify us in calling it *natural linguistics* or something to that effect.<sup>1</sup> In fact, one might rightly object that the *bio-* morpheme of our term is present because Chomskyan linguistics postulates that the object of inquiry (that is, language) is a natural object (and more accurately a biological one) and not just because it uses the same methodology as the natural sciences. It is pointless to posit a methodological naturalism for the study of something if you do not believe—or suspect—that this thing is a natural object, and it is pointless to use the form *bio-* if it is not assumed that the subject has biological foundations (Lenneberg 1967).

In fact, Chomsky has repeatedly stated (see, for example, Chomsky 2000, Chomsky 2002) that language is a mental organ; he has also added that the ‘mental’ is a part of the ‘real’, just like the ‘electrical’ or ‘chemical’, so that language is just another natural object. If anyone has spoken about nativism and natural conditioning for language in our species, it is Chomsky. Nevertheless, he has never been very keen on the term biolinguistics, perhaps because his conception of science does not really grant importance to the distinction between calling something mental, neurological, chemical, magnetic, electrical or physical; these matters are purely empirical ones that depend on the historical degree of development of various disciplines, whereas the relevant point is to consider language as a natural object. This reasoning explains why Chomsky’s intellectual commitment has been always to methodological naturalism (i.e., ‘the mental’ and ‘the physical’ must be dealt with using natural science) and not to ontological naturalism (i.e., ‘the mental’ is part of ‘the physical’): “Unless offered some new notion of ‘body’ or ‘material’ or ‘physical’, we have no concept of naturalism apart from methodological naturalism”. (Chomsky 2000, 143)

In strictly logical terms, ontological naturalism should precede methodological naturalism, whereas ontological dualism (the belief that the mind is not part of nature) should precede methodological dualism (the belief that natural science is appropriate for studying nature but not the mind). We could ask, then, why Chomsky starts with methodological naturalism to arrive at ontological naturalism—a shift that, if not well understood, may cause confusion and misunderstanding. The most reasonable explanation is that the more overtly logical shift (that is, the reverse: the move from ontological naturalism to methodological naturalism) is really ineffective.

It seems clear that the split between the so-called ‘two cultures’ (the sciences and the humanities) is a manifestation of methodological dualism. What is not as well known is that not every instance of methodological dualism is also an instance of ontological dualism. Of course, there is a lot of ontological dualism behind the methodological dualism in which we are immersed; many people think that issues such as language, consciousness, ethics or feelings belong not to the realm of the natural sciences but rather to the world of so-called human sciences. Nevertheless, even in the domain of modern cognitive science, it is not strange to discover that ontological naturalism (which everybody claims to support) gives rise to methodological dualism and not, as expected, to methodological naturalism.

There may be several reasons for this surprising fact, but I find that the main one is the inherent difficulty of the logical path from ontological naturalism to methodological naturalism. If we start from ontological naturalism, we must assume that the natural sciences (that is, physics, chemistry, biology, etc.) should be sufficient to explain mind and language, for example. However, it is obvious they are not. Neither physics, or chemistry, or biology as they are today can explain or predict the structure and meaning of a

<sup>1</sup> McGilvray (2013, 46) suggests the more appropriate (yet—in his own words—awkward) “bio-chemico-physico-compulinguistics”.

passive sentence. These sciences cannot even begin to adequately describe a morpheme or a phoneme, let alone the system of agreement rules in Russian.<sup>2</sup>

Everyone accepts that language exists somewhere or somehow in the brain, but the truth is that we are no closer to understanding a simple affirmative transitive sentence in strictly neurological terms than we are to travelling outside of the galaxy and coming back to tell of it. Thus, the more frequent conclusion is that if these things (i.e., passive sentences, phonemes, morphemes or constraints on constituent movement) are not physical, chemical or biological objects, then they either are irrelevant or belong to the realm of other non-natural, purely descriptive sciences. Here it is methodological dualism emerging from ontological naturalism—that is, a return to the two cultures.

Although it might seem surprising, many physicists, chemists and biologists believe this, and so do a significant number of linguists. However, what is implied in Chomsky's methodological naturalism is that if any theory of language structure is empirically adequate, then that theory is already part of the body of scientific, natural research on language. This stance makes sense if we recognise that we cannot prejudge what kind of physical reality language will have and if we limit ourselves to studying it like just another natural object. This implies that the discipline at hand, although it does not work with bosons, isotopes or proteins, is a natural science.

This stance is frequently rejected—not only by biologists or physicists but also by linguists and philosophers, which is even more surprising. Why are so many people reluctant to accept that grammatical theory is already a part of natural science? The first reason is, of course, that many people simply do not believe that language is a natural phenomenon; they instead see it as something purely external, of a social or cultural nature. However, even for those who accept that language is a property of the mind and brain, there is great resistance to the idea of biolinguistics' naturalistic character. Here, the most plausible explanation is that the concept of natural science in itself is inadequate.

There are two main approaches to science, the empiricist conception and the rationalist one (see Chomsky 2002, and Hinzen 2006 for inspiring discussion). According to the empiricist view, the goal of science is to discover the causes and nature of things, whereas according to the rationalist view, the aim of science is to translate nature into the language of mathematics. Contrary to popular belief, the task of science is not to find concepts or representations of the entities that compose reality but to construct mental realities (concepts and theories) and try to determine through experiments which ones find support in what we perceive. In this sense, biolinguistics cannot be different: It should not focus on trying to explain why we say what we say in a particular way but should instead consider how to construct formal models and theories that make the object of inquiry intelligible. The target of a naturalistic linguistics cannot be other than to translate the object of study into 'mathematical language'. In the end, linguistics has its own (although modest and coarse) mathematics: grammatical theory. A grammatical theory is nothing but a formal model that we devise to try to understand what exists in a person's mind/brain that allows her to acquire, use and understand one (or more than one) language.

The empiricist point of view implies that reality exists outside and that the mind is able to represent it and, in a certain sense, understand it. The rationalist point of view recognises that the world is not comprehensible in itself and that the only things that we can understand are theories about the world: "The standard that inspired the modern scientific revolution was abandoned: the goal is intelligibility of theories, not of the world" (Chomsky 2002, 68). Starting from a methodological naturalism thus implies a rationalist conception of science; it is assumed that what is real, what exists, is not predetermined—and that, of course, it does not constitute 'the material' or 'the physical' as is assumed in narrow views of science.

Physicist Steven Weinberg states that what is real is what is included in a coherent theory: "Wave functions are real for the same reason that quarks and symmetries are—because it is useful to include them in our theories" (Weinberg 1992, *apud* Jenkins 2000, 32). This idea is crucial and should confirm that I am

<sup>2</sup> An anonymous reviewer points out that contributions such as Mesgarini *et al.* (2014) seem to argue to the contrary. Of course, these contributions are a sign of progress in biolinguistics, but they do not change the fact that phonetic features were discovered by linguists, not by neurologists.

not speaking about dualism or magic. Models by Weinberg and others, like Newton's model in the past, are abstract and counterintuitive but adequately predict the behaviour of the observable world, that is, what matters. When we speak about quantum mechanics, it is pointless to say (as people often do) that entities are first postulated and subsequently confirmed once their 'material correlates' are located. Indeed, it is pointless to speak about 'material correlates' when we are considering Higgs boson or other particles or fields because they are postulated just to explain what is what we call 'matter'. Physics is empirical in a deeper sense: Physics' postulates acquire reality and existence not when their 'material reality' is discovered experimentally (an absurd suggestion when we consider wave functions or superstrings) but only when the theories of which they form a part adequately predict the observable world.

If we take methodological naturalism seriously in our approach to language, then we must admit that the same procedure should be applied to linguistics. If we adopt a rationalist concept of science, then it is clear that the task is not to apply natural sciences to the study of language but instead to start from the abstract inquiry and then from there, thanks to methodological naturalism, extend the domain of natural science. Let me insist on it: The task of biolinguistics is not to reduce linguistics to biology but to rationally extend the domain of biology and, if necessary, enlarge the concept of (linguistic) reality.

Chomsky has often pointed out that methodological dualism is a huge problem for this line of development of linguistics and other cognitive sciences. He refers (see Chomsky 2000, 112) to the double standard used to validate what is explicative in natural sciences and in human sciences. Hence, on the one hand, there are physics, chemistry or biology (sciences that are considered self-justified and that do not require, for example, that philosophers validate their results), while on the other hand there are human sciences such as linguistics or the cognitive sciences, disciplines for which additional evidence (for example, evidence of 'psychological reality') is required if they are to be interpreted realistically. At first sight, this may seem reasonable insofar as every natural science must necessarily be an empirical science. However, we must take into account that in this way, we are limiting the explicative power of one science in function of the limitations of another one. So, given that we cannot present any neurological or molecular evidence of a given grammatical restriction, it is supposed that we should think such a restriction unreal—or, even worse, think that it is not real *yet*. Nevertheless, this is pointless. It is what Dennett (1995) called *greedy reductionism*.

Taking naturalism seriously does not imply, as is sometimes suggested, that the brain exists and the mind does not. Again, this is greedy reductionism or materialistic naturalism, which is a simple consequence of an empiricist (and fundamentally erroneous and insufficient) conception of science. What is implied in a serious commitment to naturalism is that both the brain and the mind exist, although in different degrees of abstraction. This does not represent a return to dualism. It only means that we cannot say that, for example, a protein is more real than a phoneme. The evident difference between a protein and a phoneme is simply that the former is less complex (less abstract) than the latter, but this does not necessarily make it more real.

If we had demanded that linguistic theory be formulated in terms of psychological, neurological or biological reality, concepts such as morphemes, words, phrases, or ergative cases would simply not have been formulated. Neither French nor Navajo would be a licit object of inquiry. The result would be that the study of language would not be possible to pursue scientifically: That is, we would encounter a clear instance of dualism sprung from naturalism.

The mind, consciousness, meaning, intentions, desires and of course languages are properties of the brain. We know this because we know that all of them disappear once the brain dies and because we have no serious reason to attribute them to anything that lacks a brain. Let us be clear on this point: Languages do not exist outside the brain. However, this does not mean either that they can be explained just by looking at the brain or that traditional natural sciences (physics, chemistry or biology) are the most appropriate sciences for approaching and explaining them. This is not just an opinion, either: It is an objective fact that neither physics, nor chemistry, nor biology can explain or predict the meaning or the structure of an utterance in any language. In light of this, there are two options: Either we say that these phenomena must be explained by another approach (that is, methodological dualism again), or we recognise that the ongoing methodological naturalistic research on this issue is already part of a (necessarily incomplete) scientific explanation.

We certainly must use natural science to study language (and languages), but we must do so amplifying the reach of natural science. The desired reduction of linguistics into biology cannot be achieved by trying to translate linguistic principles and entities into biological principles and entities; this can only occur if we amplify the reach of biology (and more concretely, those disciplines more related to the study of the brain). Let us recall now that the reduction of chemistry into physics implied a drastic change in physics, not in chemistry.

Biolinguistics is thus a kind of linguistics that belongs to the natural sciences, a kind of abstract biology. In fact, we could define biolinguistics as *an abstract biology of languages*. Needless to say, this level of inquiry is the *beginning* of the foundation of linguistic principles in biological terms, not the end. An abstract biology is a necessary step, but it is not enough.

Anyway, if we define biolinguistics as an abstract biology of languages, then we are assuming that languages are abstract organisms, and this is precisely what I am going to propose as an answer to the question formulated in the title of this paper. However, if the notion of languages as abstract organisms is going to make sense at all, we must now turn to the other central feature of biolinguistics: internalism.

## 4 Internalism

It is well known that there are linguistic theories that reject the point of view of Chomskyan linguistics, especially so-called functionalist and cognitive linguistics. These alternative approaches also present themselves as the science of language, and occasionally, an author will indicate that his/her approach is methodologically naturalistic (e.g., Givón 2002, 2009). Nevertheless, I do not feel that they can be considered biolinguistic approaches. The main reason is that these theories are not internalist approaches.

Following Chomsky, I will define internalism as an approach that considers the object of inquiry—that is, the faculty of language (FL)—an internal property or organ of the mind. What this means is mainly that the primordial source of the structure of FL is not outside the mind and brain but inside it. This internalist conception implies, then, that the mind has its own structure. This idea, a typically rationalist one, opposes the empiricist and externalist viewpoint, according to which the mind is essentially a historical object.

Fregean analytic philosophy has focused on entities that are external to the mind—propositions, referents—in such a way that the mind *represents* them. The mind and the brain are then like a kind of tool for storing and representing propositions. This is really a functionalist conception of the mind and brain, given that from this viewpoint, the mind and brain are mainly instruments.

I think that the following table skilfully summarises the deep methodological and theoretical differences between these two types of linguistics:

**Table 1:** Two types of linguistics in opposition.

Chomskyan Linguistics	Functional Linguistics
Internalism	Externalism
Rationalism	Empiricism
Formalism	Functionalism
Universalism	Relativism

Chomskyan linguistics is associated with internalism, rationalism, formalism, and, as a corollary, universalism. On the other hand, functional linguistics is associated with externalism, empiricism, functionalism (by definition), and, as a corollary, relativism (see Mendívil-Giró 2009 for a discussion of these issues, along with more opposing pairs, and Mendívil-Giró 2012 for a discussion on linguistic relativism/universalism).

To a great extent, functional and cognitive linguistics depend on a functionalist conception of the mind. Note that from this point of view, linguistic expressions and even languages are not objects of study

in themselves but are instead a means of representing reality (whatever it is) or of communicating thoughts (whatever they may be). The relativist linguist D. Everett states this clearly:

My own theory, Ethnogrammar [...] makes the case that language is a tool for communication and thought. Different components of language, e.g. recursion, binding, phrase structure, and so on, are themselves subtools. Numbers, color words, and the like are themselves cognitive tools. To say that Pirahã culture doesn't need or desire certain cognitive tools is no more to disparage them than it is to criticize someone who doesn't play golf for lacking a set of golf clubs. (Everett 2010, 14)

From this functionalist viewpoint, linguistic expressions convey propositions that are by definition external to the mind, and consequently lack intrinsic structure. It is supposed that languages lack structure beyond what is necessary to fulfil certain cognitive or communicative functions. In fact, languages themselves are considered external to the mind (although represented on it). The most influential position in this regard is, of course, Saussure's view of *la langue* as a social institution (a shared code), which is widely extended among philosophers of language:

Si nous pouvions embrasser la somme des images verbales emmagasinées chez tous les individus, nous toucherions le lien social qui constitue la langue. C'est un trésor déposé par la pratique de la parole dans les sujets appartenant à une même communauté, un système grammatical existant virtuellement dans chaque cerveau, ou plus exactement dans les cerveaux d'un ensemble d'individus; car la langue n'est complète dans aucun, elle n'existe parfaitement que dans la masse. (Saussure 1916, 30)

Far from being old-fashioned, this stance is shared by more recent approaches to the co-evolution of language and the brain, such as Deacon's (1997), which has had evident influence on Evans and Levinson's (2009) programmatic paper:

The extra support for language learning is vested neither in the brain of the child nor in the brains of parents or teachers, but *outside brains, in language itself*. (Deacon 1997, 105, emphasis added)

I will show (see section 9 on the 'anatomy' of languages) that this externalist conception of languages is due to a great extent to a misconception of what human languages are, in the sense that certain traditions tend to consider some external components of languages as if they were the whole thing.

In the face of this approach, what does the statement that languages are an internal property of the mind really imply? The main implication is that languages are not necessarily the outcome of external factors, nor are they necessarily the outcome of their adaptation to any communicative need; rather, languages owe their structure, to some degree at least, to constrictions internal to the structure of the mind and the brain. Moreover, from a biolinguistic point of view, a language is not something that the mind represents but is instead a property of the mind; it is not something that the brain keeps or codifies but rather is part of the structure of the brain. So, the language a person speaks is her language organ. If a language is a person's language organ, we can say that from a biolinguistic point of view, a language is a person's faculty of language.

Significantly, this really implies that there is no substantial difference between language and languages—or, at least, no more than there is a distinction between life and living beings. The distinction between language and languages is artificial. It may be useful in some contexts, but we should not take it seriously. We can say informally that biology studies life, but it really studies living beings. In the same way, (bio)linguistics studies language, but what it really studies are languages. Saying that languages are concrete manifestations of language is the same as saying that living organisms are concrete manifestations of life. Both things are as apparently true as they are irrelevant.

In the same vein, the Saussurean/Deaconian externalist view of languages would say that internal languages (internal to each individual) are 'manifestations' of external languages, what would be the same as saying that tigers are 'manifestations' of the species of tigers. Both things are senseless from a biological point of view. What exist primarily are tigers and internal languages inside persons' heads. The attribution of ontological preference to language as a collective (external) object over language as a (internal) human organ is another defining feature of linguistic functionalism. I will address the possible causes of this attitude later in section 9.

## 5 Languages as Language Organs: Comparing Languages and Species

I concluded previously that biolinguistics is an abstract biology. Now, we can say more accurately that biolinguistics is the biology of abstract organs. Our next task is to try to ascertain the nature of these abstract organs. It should not be surprising that I look for inspiration in the life sciences. The best source of inspiration comes from evolutionary theory because it confronts a similar problem: what is life, what is the nature of living beings, and what is the nature of the groups of organisms that we call species.

Comparing languages and species is not new or original; indeed, Charles Darwin did it in a relatively profound manner (see Alter 1999 for a detailed historical review and Mendívil-Giró 2006, 2009 for extended discussion). A few years after the publication of Darwin's *Origins*, the German linguist August Schleicher wrote, "not a word of Darwin's need be changed here if we wish to apply this reasoning to languages" (Schleicher 1863, 64).

If the comparison between languages and species has any sense at all, it is because somehow, the evolving objects, languages and organisms are objects of the same nature: both languages (as internal systems of knowledge) and organisms are *historically modified natural objects*. What this really means is that the only difference between a bacterium and a rhinoceros is history, and therefore, that the only difference between Russian and Chinese is history. The first assertion seems clear in the light of biology (in fact, this is the most relevant fact discovered by evolutionary theory). The second one is what concerns us now, if it is correct and what follows from it.

Of course, to adequately understand why the comparison works (if it does at all) and to determine where it takes us, we must first establish what exactly the terms of comparison are—that is, we must establish the linguistic correlates of the biological concepts (genes, DNA, organisms, species etc.) used in the explanation of natural evolution.

I will address this shortly, but suffice it to say for now that if the comparison has survived so long, it is because the mechanism of evolution (=change) is the same—that is, it can be explained with reference to the same principles: namely, *heredity*, *mutation*, and *isolation* (see Pinker 1994). Heredity explains why organisms resemble their progenitors (lions engender lions and not chickens); mutations explain why descendants are not identical to their progenitors. Genetic mutations produce variation in phenotypes, and natural selection operates on phenotypes. The same is true for languages. Children speak almost, but not exactly, the same language that their elders speak. Sometimes reanalyses produce variation, and it is variation upon which the social selection operates that moves us to adopt new forms and to transmit them, sometimes together with the traditional ones, to successive generations of speakers. Isolation explains why variation may be distributed differently in once identical groups.

With a few exceptions (amongst them Schleicher's proposal), previous formulations of the analogy have been constructed vaguely and inadequately. On most occasions, languages were identified with species, and the components of languages (mainly words and sounds) were identified with organisms; then, on other occasions, languages were identified with competing organisms in the fight for survival. However, if we are to support the idea that a theory of evolution must be applicable both to languages and to species, we must clearly specify the terms of comparison. I suspect that we need something more than the more or less ingenious applications of the biological model to cultural objects, which is what we find in most approaches (e.g., Greenberg, 1992, Steels 1997, Nettle 1999, or Croft 2000), since from a biolinguistic point of view, a language is not (only) a cultural object.

The proposal that I am going to present seriously considers the biolinguistic point of view and thus most resembles the proposal of correlation that emerged from Schleicher's naturalist approach:

The species of a genus are what we call the languages of a family, the races of a species are with us the dialects of a language; the sub-dialects or patois correspond with the varieties of the species, and *that which is characteristic of a person's mode of speaking* corresponds with the individual. (Schleicher, 1863, 32, emphasis added)



Another source of inspiration is Lass's (1997) approach to linguistic change. Although Lass's purpose is not to pose an explicit theory about the correlations between the two processes, but rather to develop a theory of linguistic change free from the logical traps characteristic of functionalist explanations, he posits a generalised theory of evolution in which species and languages are particular chapters, a generalised theory that would imply 'imperfectly replicating systems'.

Traditional evolutionary theory says that those organisms that best adapt to the environment are the ones that finally survive, which seems correct. Nevertheless, in popular approaches, the idea is formulated in other terms: Organisms adapt to survive, or even worse, organisms evolve to adapt themselves better to the environment and thus be able to survive. This is obviously incorrect. However, when we speak about languages, things are different. Most people, including many notorious researchers, think that linguistic change is somehow directed toward an end. However, it is not. Languages do not change to adapt to anything or to become more expressive or more coherent, simpler or more complex. They simply change because they are replicated imperfectly.

In a certain sense, Lass reintroduces the Schleicherian model in which a language is an autonomous entity that evolves independently of speakers and their purposes and propensities:

Languages are organisms of nature; they have never been directed by the will of man; they rose, and developed themselves according to definite laws; they grew old, and died out. (Schleicher 1863, 20-21)

Lass holds that explanations of language change that are formulated in terms of 'action' on the part of speakers, 'unconscious rationality' or 'cognitive objectives' are theoretically and empirically inadequate. He observes that many linguistic structural changes (such as a change in word order or case system) unfold over long periods of time, most often beyond the lifetime of any individual speaker; therefore, such changes cannot be attributed to individuals' intentions or actions. This remains true even if we limit ourselves to saying that the individual is unconscious and that she simply acts as a trigger of the process (that later will be guided by an 'invisible hand').<sup>3</sup> Lass (1997) also observes that some theoreticians find the explanation of changes in certain universal tendencies that should affect 'unbalanced' or 'sub-optimal' systems—yielding more balanced, closer to optimal, more natural, simpler, less marked or easier-to-learn systems—to use a terminology that is more or less equivalent under different approaches. However, we encounter a significant problem if we seek to justify a change as the elimination of an undesirable or harmful redundancy or as a trend towards more balanced or coherent systems: We should note that some previous change must have produced this 'improvable state', and we will probably find languages whose similar (or greater) degree of 'complexity', for example, has remained the same for centuries. Let us imagine language L1, whose feature F has changed, and a language from the same branch, L2, whose feature F has not changed (as usually happens). If L1's feature F were in an unsteady state, we cannot explain why L2 has not changed based on these same tendencies. We could not say that the outcome reached by L1 is 'good' or 'better' without implying that the present state of L2 is 'bad', 'worse' or 'undesirable'.

Note that we do not find badly designed or outdated species in nature. We do not find unsuccessful species simply because such species could not have evolved. In the same way, thinking that there are more or less perfect languages, more or less 'evolved' languages, or more or less primitive languages is simply wrong (although it is tacitly suggested in some approaches; see Moreno, Mendiivil-Giró 2014 for a critical overview and references).

In any event, the basic line of Lass's argumentation against directed, intentional or adaptive changes is that even if we were to admit that some linguistic innovation is functionally motivated, it is a well-known fact that such innovation does not happen simultaneously but instead propagates through social channels. As Lass remarks, "unless a motivation is arbitrary, its implementation ought not to subject to contingent factors like age, sex, prestige, etc." (Lass 1997, 364).

<sup>3</sup> Keller, who supports an "invisible hand" theory of linguistic change, defines linguistic changes as "phenomena of the third kind": that is, a kind of phenomena that are not natural and that out of things that are really the result of human action, they are not intentional (like artefacts) but are instead "things which are the result of human action, but not the goal of their intentions" (Keller 1994, 56-57).

One of the most striking points of this discussion is that it is reproducing, in up-to-date terms, the same model that Darwin introduced into his famous analogy between languages and species: Linguistic changes, like genetic mutations, are blind; they do not point towards a functional or adaptive aim, nor do they follow laws of evolution guided by an ‘invisible hand’. Once we have eliminated the teleological prejudice regarding linguistic changes (a battle that has not yet been won), we can see that the blind and random model of classic Darwinism applies to language change, as Schleicher claimed, without the need to change a word of it.

Nevertheless, we must resist another temptation. It is true that, although in a blind and random way, organisms are adapted to their environment. Let us think of a hippopotamus or a whale. Accordingly, we could say that somehow languages adapt to the culture in which they are used or that languages adapt to their speakers’ worldview. That is, we might postulate that there is some kind of correlation between languages and the cultural environment in which they live. This is the viewpoint of many modern relativists (e.g., Everett 2005, 2010). I think that this is mostly wrong, apart from relatively superficial facts concerning the richness of some parts of the substantive lexicon. In this case, the mistake is not to attribute directionality or finality to linguistic changes (although this is done implicitly) but rather to wrongly identify the environment to which languages (supposedly) adapt.

When we say that an organism adapts to the environment, we should not mean that the environment moulds or gives structure to the organism. What we are saying is simply that certain phenotypic traits have a greater or lesser probability of being selected given the environment. In linguistic change, it happens in the same way: Some linguistic variants have more of a chance of being chosen, usually according to external factors such as prestige or fashion (see Labov 2001).

In the natural world, the environment may be rather complex. If we think of a whale, water comes to mind, but if we think of a peacock, we find that its physical environment is not very useful in explaining its hypertrophic tail unless we include peahens’ mating preferences as part of their ‘physical environment’. Peacocks’ wonderful tails do not help them to nimbly escape predators or go unnoticed, but it seems that peahens select this kind of tail, perhaps simply because they like it.

Delimiting the environment for natural selection is not as easy as it seems, and the exercise is exponentially harder for ‘linguistic selection’. The number of different factors that can affect the destiny of a linguistic variant is so complex and varied that the concept of adaptation to the environment (‘culture’) is simply not concrete. Both in nature and in the use of languages, the notion of the environment is so complex and heterogeneous that it cannot form part of any relevant equation. In fact, adaptation is a subjective and relative notion: a hippopotamus is well adapted to live in a river only if we compare it to a horse instead of to a goldfish, for example. Because of this, the notion of environment does not play a significant role in the analogy between languages and species that I am going to present.

## 6 The Terms of Comparison

From a biolinguistic point of view, a language is a person’s language organ. This is what Chomsky (1986) called an *internal language* (*I-language*). However, this language organ or I-language is also a historical object. Without a doubt, the language organ of a person who speaks Russian is different from the language organ of a person who speaks French. Perhaps both organs share a common unhistorical layout (which we conventionally call *Universal Grammar*, UG), but they differ because of contingent events that we can only explain historically. Migrations, diverging changes, borrowings and isolation have produced two different natural objects (in fact, millions of them—as many as there are speakers of both languages).

Still, although the I-language of a person who speaks French and that of a person who speaks Russian are historically different, this difference should not lead us to think that they are purely historical objects—in the same way that a rhinoceros and a gazelle are different historical objects but are also natural objects.

*External languages* (*E-languages*) such as Russian or French (that is, things we can say are spoken in Moscow and Saint Petersburg or in Paris and Québec and that they are written with Cyrillic or Latin alphabets) are also historical objects but are not natural objects. They are, as Saussure (1916) put it, social institutions.

In fact, the notion of E-language is even more complex and heterogeneous than that of I-language. I will use the expression E-language to denote simply *a population of very similar I-languages*. Intuitively, ‘very similar’ means ‘similar enough to allow mutual inter-comprehension’ (as a useful measure of similarity). Note then, that when we say that two persons speak *the same language* we are not speaking of I-language (individual by definition), but of E-language, that is, we are saying that the I-languages these persons speak are similar enough to be considered of the same group.

Of course, there are other possible definitions of E-language, but the one that is relevant to this contribution is that of the grouping or population of I-languages. It is especially appropriate to exclude from our definition of E-language notions such as ‘the outcome of the use of the I-language’, ‘a set of utterances’ or anything that counts as behaviour. This is a crucial difference between the present proposal and some other approaches, such as that of Kirby (1999, 38) and others, who tend to identify I-language with genotype and E-language with phenotype.

In summary, we have two essential notions of *historical biolinguistics* (the expression analogous to that of evolutionary biology): I-language and E-language. I-language is each person’s language organ, a natural object whose phenotype depends on its ‘genotype’ and development. E-language (where the *E*- evokes more ‘extensional’ than ‘external’), then, simply consists of a population of I-languages that allow their possessors to communicate with each other.

The next table shows my proposal regarding the correspondence between natural and linguistic evolution:

**Table 2:** Correlation between natural and linguistic evolution (first version)

Natural Evolution	Linguistic Evolution
Organism	I-language
Species	E-language

As shown in Table 2, the linguistic equivalent of organisms, the individuals (animals or plants) that make up a species, is not the *components of a language* (such as phonemes, morphemes, words or constructions) as in usual analogies; rather, the comparison is between organisms and I-languages, individual language organs—that is, what Schleicher saw as “that which is characteristic of a person’s mode of speaking”.

A ‘linguistic species’ (or E-language) will then be formed by a *set* of language organs, a population of historically modified natural objects, just as in the case of natural organisms themselves. The species of horses (*Equus ferus*) does not exist in itself; it is nothing but the set of all horses. A natural species is a population of historically modified natural objects, and a linguistic species (E-language) is also a population of historically modified natural objects (I-languages). Of course, there are also social linguistic norms and different types of cultivated languages (written language, for example). But these are not natural languages, but social institutions and cultural objects created to elaborate and regulate the use of natural languages. The lack of distinction between natural languages (populations of I-languages) and cultivated languages (cultural elaborations and social institutions) is another source of discrepancies between current theoretical approaches to language (see Moreno, Mendiávil-Giró 2014).

It is important not to identify I-languages (i.e., language organs) with entire persons. The individuals that make up a ‘linguistic species’ are not speakers but are instead their I-languages or language organs. Thus, a bilingual person features two I-languages belonging to different species. In this sense, the individuals that form linguistic species, like those that form natural species, have no adaptive inclinations or evolutionary tendencies. For I-languages the speaker’s desires or intentions are just part of the environment.

A natural species is a population, a grouping of individuals (animals, etc.), which are similar enough to allow viable interbreeding. Likewise, a ‘linguistic species’ is a population of individuals (I-languages), which are similar enough to allow mutual intelligibility. In this way, the criteria for species identity are similar (though not identical) in the two domains. Furthermore, in both cases, the boundary is blurred

(and in some sense arbitrary)—let us think of horses and donkeys, or of Spanish and Portuguese (order irrelevant). In the case of species, the capacity to interbreed and produce fertile offspring depends on a very pronounced phenotypic similarity between two individuals; in the case of languages, fluid and mutual intelligibility requires a very pronounced grammatical and lexical similarity between two language organs (this is the only approach that Dixon 1997 calls *linguistic* criterion; all the other criteria we use are, also in terms of Dixon, *political* criteria).

The proposal displayed in Table 2 embraces the achievements of the ‘high part’ of the traditional analogy, the part that identifies species with languages but let us also note that Table 2 does not reflect the ‘low part’ of the traditional Darwinian analogy. The components of a language (roughly words, morphemes, phonemes, constructions) are not, as we have discussed, equivalent to the individuals that form a species; the individuals that form a species, according to my proposal, are instead I-languages.

Before we extend Table 2 to consider other possible relevant concepts, we should quickly reflect on the meaning and implications of the proposed comparison (based on the primary equivalence *language = life*). Note that I am taking an organ of the human body (the faculty of language) and making it equivalent to a natural organism like a horse. This is more or less the same as saying that the human stomach is equivalent to the organism and that the species is made up of all human stomachs. This concept may have limited interest, but it is not illegitimate in itself.

Nevertheless, the language organ has relevant peculiarities that make the analogy interesting and justifies this apparently vacuous extension. Of course, the language organ differs from the stomach in that the former is a mental organ—but that is not the only distinction we can make. As pointed out by Longobardi (2003), the language organ, unlike other mental organs (such as vision, memory or face recognition), has a relevant cultural history. All individuals’ capacity for vision, granted certain inevitable peculiarities, is essentially the same no matter how socially near or distant they are. However, people’s language organs vary and group themselves according to the historical changes that they have undergone. As a result, the study of change and variation in language organs is especially relevant to the study of the human mind, adding a distinctive factor and a new source of evidence with regard to the study of other, more uniform mental organs. The study of the faculty of language then affords a comparative perspective that is not available in other domains of human nature. I think this justifies the raising from ‘organ’ to ‘organism’ on which the analogy in Table 2 relies.

However, there is still another point of clarification to be made before we continue. If we look again at Table 2, we will see that there is a correlation between *natural evolution* and *linguistic evolution*. My proposal is that the analogy is relevant only if the frame of reference for each one of these types of evolution is different. This means that in the proposal I am developing, natural evolution is a different phenomenon from linguistic evolution. More specifically, they are *formally identical* but *substantially different* in the sense that in Table 2 the natural evolution column must be understood as referring to the evolution of natural species in geological time (including, of course, the evolution of our kind). The expression *linguistic evolution* must be understood as referring to linguistic change in historical time. This is especially relevant when considering the evolution of language as a human faculty. The clarification I have just made means that according to the biolinguistic point of view, the evolution of language (as a species capacity) is an issue from the left column (natural evolution) instead of the right one (linguistic evolution). This implies that there is a radical split between these two phenomena, the evolution of language (which is a part of natural evolution) and linguistic change (which is obviously a part of historical linguistics). Most readers may consider this clarification to be superfluous or unneeded, but a closer review of some recent literature (Heine and Kuteva 2007, Givón 2009) reveals that for many authors and even for entire research schools (so-called Grammaticalization Theory), clarification is indeed necessary. In fact, many authors tend to mix up and overlap the two processes, as they see the evolution of language as the consequence of the evolution (i.e. historical change) of languages (see also Kirby 1999). This has as the immediate effect of making the comparison between languages and species more of a problem than a way to encourage understanding.<sup>4</sup>

<sup>4</sup> For an extended discussion on the ‘confusion’ between the evolution of the Faculty of Language and the processes of historical linguistic change see Moreno, Mendívil-Giró 2014, chapter 3

## 7 Extending the Analogy I: UG as DNA

With these points of clarification in mind, we can attempt to complete the analogy to include other important factors of natural evolution. It is a well-known fact that all life on Earth is based on DNA. What is the equivalent of DNA in historical biolinguistics?

Molecular biologist and scientific journalist Javier Sampedro (2002) has remarked on the role of DNA quite vividly:

All living beings are based on DNA [...]. All living beings use the same genetic code in spite of the fact that there are thousands of millions of possible genetic codes that would work as well [...]. The fact that the reader can feed on sugar is due to the same reason -the same in all its extremely complex details- as the fact that the most humble bacterium can feed on the same sugar. It is possible to imagine many different ways of storing genetic information and of translating it into useful things and of feeding on sugar, but it is the case that the dozens of thousands of millions of species on Earth do it just in the same way. It would be a great coincidence if all of us, bacteria, cherry trees and human beings would not have a common origin. (Sampedro 2002, 23, my translation JLMG)

It may be unsurprising that the correlate of DNA that I would like to propose is precisely UG. If we permit ourselves an amusement and modify the previous quotation, exchanging references to DNA for references to UG and references to bacteria, readers and other living beings for references to Russian, Chinese and other languages, the resulting text would seem extracted from a generativist handbook (altered fragments emphasised):

All *human languages* are based on *UG* (...). All *human languages* use the same *grammatical* code in spite of the fact that there are thousands of millions of possible *grammatical* codes that would work as well (...). The fact that the *European* reader can *acquire her language* is due to the same reason -the same in all its extremely complex details- than the fact that the most *remote Chinese* reader can *acquire her language*. It is possible to imagine many different ways of storing *linguistic* information and of translating it into useful things and of *acquiring a language*, but it is the case that the dozens of *hundreds of languages* on Earth *have the same structure*. It would be a great coincidence if all of *them, Russian, Chinese* and *Swahili* would not have a common *source*.

Note that I have substituted the word *origin* for *source* in order to avoid the reading according to which linguistic universals could be explained as a homology, that is, as the result of a common heredity from a protolanguage. See Pinker (1994) for a line of argument against the idea that the existence of universals should be seen as a proof of the monogenetic hypothesis. Pinker shows that a common heredity cannot be used to explain universals, although the monogenetic hypothesis might be right.

The resulting table is the following:

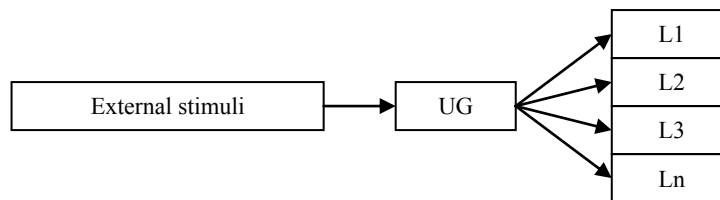
**Table 3:** Correlation between natural and linguistic evolution (second version)

Natural Evolution	Linguistic Evolution
Organism	I-language
Species	E-language
DNA	UG

Comparing DNA to UG is sensible only if we interpret both notions as biological restricting frames. In other words, if DNA contains the chemical code that expresses and transmits life, UG is the biological conditioning that the brain and the mind impose on the development of the faculty of language. It could be said then that if DNA offers some possibilities and a style of codification for organisms based on its biochemical properties, UG offers a range of biologically restricted possibilities for a language organ based on its neurobiological properties.

Now it is necessary to clarify which notion of UG is required for the comparison to work. It is important to take into account that a Principles-and-Parameters style UG would not be a good correlate for DNA. Actually, I think that only a minimal(ist) notion of UG would serve this purpose. To begin with, it is important not to confuse UG with the faculty of language (FL); this is a frequent error even in generative literature and especially in introductory textbooks. UG, unlike FL, is not a component of the mind. UG is not a language organ but is instead the arbitrary name that we use to refer to those natural factors that regulate the development of FL. A possible split between type 1 (genetic) and type 3 (general) factors in Chomsky's (2005) terms is not relevant here.

As shown in figure 1, UG is usually interpreted in generative literature as the *initial state* (or  $S_0$ ) of the faculty of language, whereas knowledge of language, I-language (L in the scheme) is considered a *steady state* (or  $S_s$ ): that is, the language organ of an individual. I do not disagree with the second statement, but I would like to suggest that the accuracy of the first statement is questionable at best.



**Figure 1:** The role of Universal Grammar in standard approaches.

Obviously, what the scheme intends to represent is the Chomskyan nativistic conception according to which the organism imposes certain properties on the systems of knowledge that are finally developed (L1, Ln), properties that must not be acquired from the environment. UG is conceived of then as the set of properties that every natural language must satisfy and that cannot be captured from the environment according to the famous poverty of stimulus argument. I agree that this approach is essentially correct but sense a crucial mistake: that an approach like the one roughly schematised in figure 1 is somehow suggesting that UG exists in the mind or in the brain *previous* to the existence of a particular language.

Technically it can be said that UG is the initial or  $S_0$  state of FL, but this is appropriate only if we are aware that this idea has the same meaning as if we were speaking about the  $S_0$  state of the pancreas or the circulatory system. In fact, there is no human pancreas that is not somebody's pancreas, nor does there exist a circulatory system that does not belong to a particular animal. Besides, there is not a universal pancreas that later is parametrised for each species or each individual. Of course, however, this does not mean that the development of any pancreas or circulatory system (or language organ) are not restricted by a serious common biological conditioning.

If language is a human organ, the real meaning of the expression *initial state* is that the organism literally lacks the faculty of language before experience and development (which makes perfect sense given that newborns do not speak). In fact, Chomsky writes, "UG is taken to be a characterization of the child's pre-linguistic initial state" (Chomsky 1981, 7). But this meaning does not deny that the organism is somehow designed to develop and to simultaneously restrict the formal properties of the resulting FL, which is what we call UG.

So, other than metaphorically, we cannot say that languages are manifestations of UG. This would be equivalent to saying that animals are manifestations of DNA when in fact, animals (and the rest of living beings) are, in a certain sense, manifestations of *genes*. Thus, depending on how genes are organised into DNA, we obtain one organism or another. We clearly need a linguistic equivalent of the biological notion of the gene.

## 8 Extending the Analogy II: Parameters as Genes

I have rejected the idea that the components of a language (roughly words, morphemes, phonemes, constructions) are equivalent to individuals (recall that individuals are I-languages). We might suspect then that the components of languages are in fact the linguistic equivalent of genes. In fact, some authors, including McMahon (1994), Croft (2000), and Mufwene (2002), either suggest or openly propose it. However, I think that this correlation is as mistaken as the one between words and individuals in the past.

In a certain sense, I am proposing that the components of languages are the equivalent of genes but in a rather different sense of the notion *component of a language*. Baker's 2001 book is entitled *The Atoms of Language*, implying that the atoms of language are *parameters* understood as the *atoms of linguistic diversity*. This notion of parameters (as the minimal units of linguistic diversity) is what I suggest that we identify with genes.

On this basis, I present the final version of my proposal in the next table:

**Table 4:** Correlation between natural and linguistic evolution (final version)

Natural Evolution	Linguistic Evolution
Organism	I-language
Species	E-language
DNA	UG
Genes	Parameters

We know that genes are the basic units of heredity in a living organism. Genes hold the information necessary to build and maintain an organism's cells and pass genetic traits to offspring. Thus, the genes of an I-language must be those features that regulate its development and that also express the structural differences amongst I-languages. In short, the genes of an I-language could be conceived of as configurations of features that produce different 'instantiations' (different phenotypes) of systems of knowledge restricted by UG. In this sense, broadly speaking, we can say that a 'gene' of a given I-language would be, for example, the attachment of agreement features to the verbal root, which perhaps would lead to the movement of verbs out of the VP in that language, whereas a variation in that 'gene' (or set of 'genes') might cause another I-language's verbs not to move in overt syntax.

DNA does not express at a single point of the genome the total design of an organism (for example, a dog); instead, each group of genes expresses the specific and relative design of the diverse organs that form a dog, many of them identical to those that form a person, a horse or a fly. In the same way, UG does not codify a language choosing this or that parametric value, but there are 'genes' that determine the phonologic system of vowels, whether there will be head-marking or not, or whether there will be overt movement of verbs.

But what notion of linguistic parameters is needed for parameters to be appropriate correlates of genes?

I have already rejected the 'classical' notion of UG as 'containing' principles and parameters, which implies that we cannot use the traditional notion of a parameter as a principle containing a choice. The 'classical' version of parametric theory amounts to saying that UG principles are parametrised, that is, they can be conceived of as "a complex and intricate network of some sort associated with a switch box consisting of an array of switches that can be in one of two values" (Chomsky 1988, 62). Each language (each core grammar) would be then a particular combination of chosen values.

Actually, even detractors of parametric theory (e.g., Haspelmath 2008, 80) recognise that it is a very attractive theory because of its elegance and simplicity. Its appeal is based on the idea that the theory does two things at the same time: it simplifies the explanation of how children acquire language (instead of learning rules and unattainable properties, they 'press a button' that supplies sets of properties), and simultaneously, it offers a direct way of explaining the typological clustering of features, a descriptive requisite for every linguistic theory.

The problem is that Principles-and-Parameters style parametric theory presents both theoretical and empirical difficulties. I have no space here to review them (see Newmeyer 2005, Biberauer 2008, Mendívil-Giró 2009, and Boeckx 2011), but in short, it could be said that from a theoretical point of view, one of the main problems is the proper development of the so-called Minimalist Program (MP).

## 9 The Anatomy of I-Languages

In order to understand parameters in a minimalist context, we need to look closer at the architecture of the Faculty of Language. The influential formulation in Hauser *et al.* (2002) establishes a distinction between the faculty of language in a narrow sense (FLN) and the faculty of language in the broad sense (FLB). FLB includes all of the different mechanisms implied in the knowledge and use of language, regardless of their overlap with other cognitive domains or even with other species. Essentially, Hauser *et al.* propose that FLB includes a sensory-motor system (SM), a conceptual-intentional system (CI), other possible systems, and the computational mechanisms for recursion ('narrow syntax' or the computational system, CS). Given that language as a whole is specific to human beings, it is plausible that a set of FLB is uniquely human and language-specific. This set is what Hauser *et al.* call FLN. They hypothesise that FLN only includes the computational system (syntax) and is the only uniquely human component of the faculty of language. If FLN has to interact with the rest of FLB, FLN should also have interfaces with those systems.

One of the main advantages of this model is that it is compatible with the hypothesis that FLB is a species adaptation that shares many aspects with other species' systems of knowledge and communication while maintaining simultaneously that the mechanisms underlying FLN are specifically human and specifically linguistic (see Balari, Lorenzo 2011 for a discussion and elaboration of this hypothesis).

On the other way, the adoption of a minimalist perspective has the direct consequence of creating the need to eliminate as many as possible of the purely language-internal properties one postulates. This implies accounting for the descriptive scope of a given postulated principle considering conditions imposed from the interfaces or general issues of computational efficiency. What is of relevance for us now is that if UG is emptied of specific linguistic principles except for abstract aspects of design, the possibility of explaining linguistic types as parametric options is diluted. As Biberauer has pointed out,

the simplified MP architecture, comprising just a Lexicon, the Computational System (which 'does' Narrow Syntax) and (possibly) two interface components, PF and LF, allows for two general possibilities: either parameters are located in the Lexicon or they are located at one of the interfaces. (Biberauer 2008, 24)

Nevertheless, it should be noted that these two possibilities are not mutually exclusive. Actually, the lexicon itself (in a sense to be specified) can be seen as an interface between meaning (CI), syntax (CS), and sound (SM), which would imply that what parameters express are not choices made from among previously underspecified options on UG principles, but different possible structures for this lexical interface.

An attractive hypothesis might be that during the ontogenetic development of FL, the organisation of the lexical interface between components of FL is sensitive to environmental linguistic input: that is, to primary linguistic data that (in classic proposals) children employ to set the parameters. Note then that the variability of the interface is doubly constrained: on the one hand by CS and on the other hand by both 'external' systems (CI and SM). Of course, these systems may have their own conditioning (perhaps shared to a certain degree with other species).

Chomsky has suggested that CS is supported by the operation of *unbounded merge*, (that is, the ability to join two units recursively), and that CS, together with CI, is essentially a 'language of thought', that is, an ability to link together concepts in a new and unlimited way:

Such change takes place in an individual, not a group. The individual so endowed would have the ability to think, plan, interpret, and so on in new ways, yielding selectional advantages transmitted to offspring, taking over the small breeding group from which we are, it seems, all descended. (Chomsky 2007, 14)



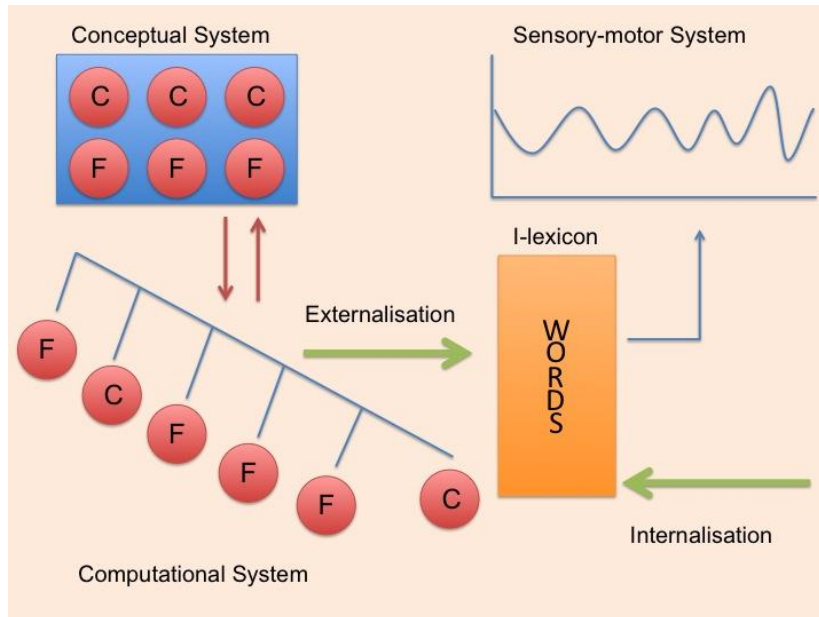
This scenario suggests that CS is part of a ‘language of thought’ independent of communication and of the externalisation systems: “the earliest stage of language would have been just that: a language of thought, used internally” (Chomsky 2007, 13).

This view crucially implies that the relationship between CS and the CI and SM systems is asymmetric. Chomsky (2007) argues that there are clear indications that the design of the internal syntax (CS) is optimised for its connection and interaction with the CI system, and not for its connection and interaction with the SM system (see also Berwick, Chomsky 2011). This asymmetry would explain why it is in the process of ‘externalisation’ of the internal syntax where linguistic diversity appears:

Various considerations, then, seem to converge rather plausibly on the conclusion that language may be optimized relative to the CI interface, with mapping to SM an ancillary procedure, and complex to the extent that SM has no prior adaptation to these needs. Insofar as SMT [Strong Minimalist Thesis, JLMG] holds, generation of structures mapped to CI will be optimal for the CI interface and common to languages apart from parametric and lexical choices (phenomena that require explanation), while phonology, morphology, and whatever else is involved in externalization might be variable and complex and subject to large-scale historical accident, satisfying the linking condition in ways that are as good as possible. (Chomsky 2007, 14-15)

Note that Chomsky cautiously assumes that parameters and lexical choices may be different types of variation from the externalisation components (morphology and phonology). The model I am suggesting tries to unify these sources of linguistic variation into the lexical interface between the ‘language of thought’ (CI+CS) and the SM system. According to this model, syntax would be minimal (in the sense that it would be governed by criteria of computational efficiency), universal (common to all languages) and invariant (insensitive to historical change); syntax would be a human computational system to generate thoughts and new concepts from concepts.

In figure 2 I propose a schematisation of the FLB (that is, of any I-language) according to this asymmetry.



**Figure 2:** The anatomy of an I-language. The conceptual system and the computational system (as well as the sensory-motor system) are uniform across languages, while differences belong to the I-lexicon, which externalises syntactic derivations.

As shown in figure 2, the conceptual system (part of CI) provides the computational system with concepts. Simplifying a lot, I represent two types of concepts: those corresponding to the ‘meaning’ of traditional lexical roots (C in the picture) and those corresponding to the ‘meaning’ of functional categories (F in

the picture). The red arrows try to represent ‘thought’ as the result of the interaction between CS and the conceptual system. The ‘externalisation’ of syntactic-conceptual derivations would imply that the internal syntax is linked to systems external to the FLN (but internal to the FLB), mainly the sensory-motor systems. I represent this interface for externalisation as an *Internal Lexicon* (I-lexicon), which includes at least what is commonly known as morphology and phonology, connected to other sensory-motor systems that lead to the effective materialisation (in sounds or visual signs) of syntactic derivations.

I do not consider here a possible direct link between the conceptual system and the SM system, which would be plausible in order to handle sound symbolism and ideophones, and that would reflect the existence of a pre-syntactic or pre-linguistic lexicon, an “expressive lexicon” in Déchaine and Mudzingwa’s (2014) terms.

The main hypothesis here is that traditional ‘words’ are steady links (part of memory) between fragments of syntactic derivations and morphological-phonological representations. That is, words serve the function of ‘materialising’ syntactic computations in ‘flat’ chains interpretable at the SM interface. Note that I am assuming a radically non-projectionist view of the relation between lexicon and syntax, in line with the ‘exo-skeletal’ model of Borer (2005), or with the ‘nanosyntactic’ approach (Starke 2009). Both models share the crucial feature of considering words as the spell-out of syntactic structures, and not as the terminal nodes which syntax combines. The I-lexicon then must be seen as specific guide (for each language) to externalise uniform syntactic derivations. In my view, the I-lexicon would include a realizational morphology that generates phonological representations, which enter the phonological component.

Evidently there is a contradiction in the statement that the internal syntax is *externalized* in a given *internal* lexicon (I-lexicon) of an *internal* language (I-language). Here we must note that an I-language is a mental organ, a system of knowledge, which includes biologically determined components (e.g. internal syntax) but also a component ‘internalised’ from the environment (the I-lexicon), which is precisely what distinguishes the world’s languages from each other.

As Sigurdhsson (2011) has pointed out, we must distinguish between the emergence of the internal syntax, a biologically determined process during brain development, and the subsequent (though early) processes of development of the I-lexicon (including morphology), a process strongly influenced by the environment and helped by general learning resources (one of the subcategories of the type 3 factors mentioned by Chomsky 2005):

That is, early internal language growth is ‘nativistic’. In contrast, lexical and structural expansion in later learning of external language, including second language learning, evidently involves internalization. (Sigurdhsson 2011, 374, fn. 14)

An I-language thus includes an I-syntax, and a morphology-and-phonology (I-lexicon) ‘internalised’ from environmental stimuli. This then implies that the SM component that is part of the FLB is much more complex and structured than it would be if it only included the already complex systems involved in the production (and interpretation) of sounds or visual signs. In this sense, we might say that the I-lexicon of a language is a component developed in the process of language acquisition, which is external to the FLN but internal to the FLB; in other words, it is the interface between the ‘internal language’ and the SM system. In this sense, only the parts of language that are internalised are susceptible to change and variation.

From this perspective, it is tempting to consider that some points of deep disagreement between current linguistic schools (specially those between formalists and functionalists-cognitivists) have their roots in a misunderstanding of what a language is. According to the biolinguistic point of view reflected in figure 2, every I-language includes all three components of FLB (CIS, CS and SMS) and an interface I-lexicon that singularises each one because of its historical nature. But it seems that from the externalist point of view that underlies functionalist and cognitivist approaches, there is a misidentification of human languages with their I-lexicons. If we (mis)identify an I-language with an I-lexicon, then it follows rather naturally (i) that linguistic universals are a myth so that “structural differences should instead be accepted for what they are, and integrated into a new approach to language and cognition that places diversity at centre stage” (Evans and Levinson 2009, 429), (ii) that grammatical categories are language-specific (Haspelmath 2007), (iii) that functional categories and syntax are the historical consequence of linguistic change—grammaticalisation—

(Heine and Kuteva 2007), (iv) that “people who speak different languages do indeed think very differently and that even flukes of grammar can profoundly affect how we see the world” (Boroditsky 2009, 118), or (v) that common features of languages “come not from any universal grammar, but rather from universal aspects of human cognition, social interaction, and information processing—*most of which were in existence in humans before anything like modern languages arose*” (Tomasello 2009, 471, emphasis added). Note that Tomasello is explicitly subtracting “universal aspects of human cognition” from languages, what seems to me a clear misidentification of I-languages with their I-lexicons.

It could be argued that this is a licit operation if one wants to study language and not ‘general cognition’, but it is not. Let’s look again to biology to understand why. It is known that humans and chimpanzees share over 99% of DNA. But we are not tempted to say that this 1% of specifically human DNA is a human being or can give rise to a person. In fact, we need the 100%.

## 10 Deriving Linguistic Baupläne

It seems true that the minimalist approach is incompatible with classic parametric theory, but the model that postulates a minimal and abstract computational system as a nexus between relatively independent components of the mind is especially able to capture the nature of the structural differences amongst languages not as the selection of *ad hoc* parametric values, but as a result of the limited array of possible solutions that will satisfy the output conditions on the interface. More interestingly, this limited array is actually historically conditioned, too.

Stylistic preferences regarding the use of language, migrations, and territorial expansion cannot affect the computational and the sensory-motor systems (or perhaps even change the essential design of conceptual systems), but they can crucially influence the process of internalisation of the I-lexicon (leading, for example, to processes of structural reanalysis). Parameters, understood in the original sense of systematic correlations between grammatical properties, are real (although perhaps less deep than assumed in the past). The mathematical notion of a parameter is related to a value that determines the behaviour of a system. This is the original use in Chomsky’s formulation, and I think that we must retain it, although it is not easy to fit it into a minimalist framework. In this context, parameters are to be seen not as substantive but just as effects of an absence of UG specification.

Approaches to linguistic typology in minimalist research are mainly grounded in what Baker (2008, 353) called “The Borer-Chomsky Conjecture” (BCC): The hypothesis, already formulated in Borer (1984), that parameters of variation are deducible from differences in the features of particular items (mainly functional categories) in what I call the I-lexicon.

The BCC is fully compatible with the model sketched above (fig. 2), in which everything is interlinguistically uniform except for the I-lexicon. As Baker points out, the BCC implies that ‘traditional’ macroparameters (which he calls *grammatical parameters*: that is, parameters not associated with lexical items) do not exist; instead, all parameters are lexical (or *microparameters*). However, this view might make it more difficult to explain why there are parametric clusters—that is, why languages (or large parts of them) do not form a continuum instead of grouping according to clearly identifiable types. As Baker states,

the strict microparametric view predicts that there will be many more languages that look like roughly equal mixtures of two properties than pure languages, whereas the macroparametric-plus-microparametric approach predicts that there will be more languages that look like pure or almost pure instances of the extreme types, and fewer that are roughly equal mixtures. (Baker 2008, 361)

It seems, then, that the existence of microparameters should not preclude the existence of higher-range parameters, although the status of the latter is unclear. To shed some light on this, let us recall that Borer (1984) advanced that the locus of structural variation in languages is inflectional morphology. Her (now influential) statement deserves to be fully replicated here:

The inventory of inflectional rules and of grammatical formatives in any given language is idiosyncratic and learned on the basis of input data. If all interlanguage variation is attributable to that system, the burden of learning is placed exactly on that component of grammar for which there is strong evidence of learning: the vocabulary and its idiosyncratic properties. We no longer have to assume that the data to which the child is exposed bear directly on universal principles, nor do we have to assume that the child actively selects between competing grammatical systems. Rather, just by learning the inflectional rules operating in her/his environment, the possibilities offered by UG are narrowed down so as to give rise to Core Grammar. (Borer 1984, 29)

Morphology, the central component of the I-lexicon (together with phonology) is an ideal candidate for an interface between syntax and the sensory-motor system, and it is tempting to consider it the main locus of the structural differences between languages. To put it simply, morphology is the association of sounds (*words*) with syntactic structures. If this is like that, different morphologies (different word structures) imply not different syntaxes but only different systems of connection between syntax and sounds. And this is the role of the I-lexicon in figure 2.

Much research published during the last twenty-five years has shown that the grammatical profile of a language depends to a great extent on how the morphological component is organised (see Julien 2002). Thus, if verbal forms are inflected for tense and agreement, it is highly probable that verbs move in the syntax; if noun phrases are case-marked, it is possible that they have a less restricted order than if they are not; and if verbs incorporate pronominal arguments, it is more probable that we will find a polysynthetic structure.

Turning back to our comparison, we could then say that a language's I-lexicon (conceived of as an interface between narrow syntax and sensory-motor systems) is the authentic 'genome' of an I-language. The reason why languages' morphologies are so diverse is at least partly (and perhaps exclusively) related to processes of historical change. Recall now Givón's famous statement: "Today's morphology is Yesterday's syntax" (Givón 1971, 413), or more recently:

As in bio-evolution, it became increasingly clear, today's extant grammatical structures and their mind-blowing diversity were best understood and explained through tracing the developmental pathways that gave them rise. (Givón 2009, XVII)

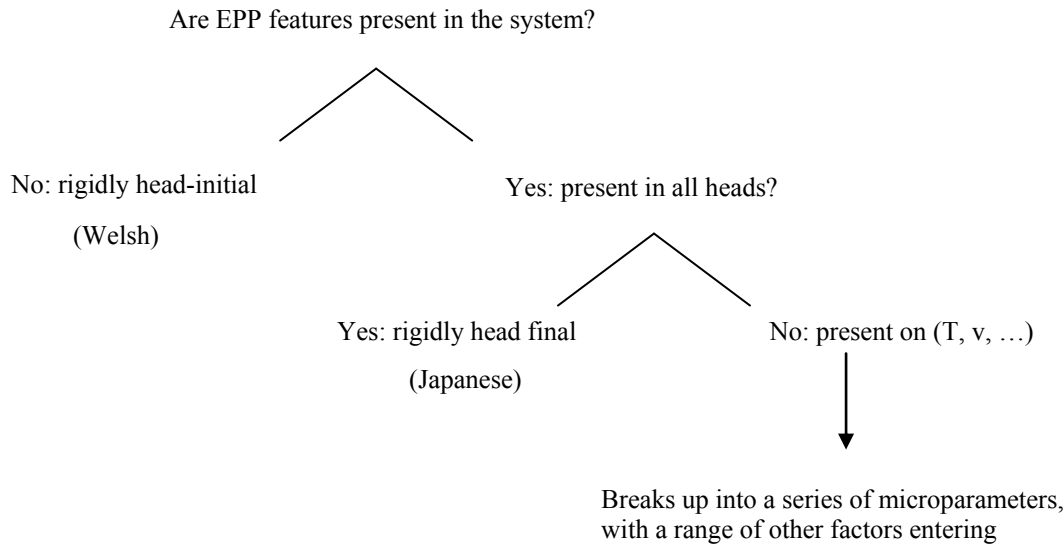
This view would mean that parameters are really historical registers of the past, and this is indeed what genes are: historical registers of the past.

What the deep logic of the parametric theory suggests is that parametric clusters (i.e., typology) are the consequence of how certain options chosen in the development of a system will condition the development of the rest of the system or some parts of it. This will provoke the emergence of types, those high-level parametric clusters that have always captivated linguists. Of course, this is a familiar scenario in developmental biology. There are genes (master genes) that regulate the expression of other genes in complex cascade effects. Consider, for example, the role of Hox genes:

Hox genes act at many levels within developmental gene hierarchies: at the 'executive' level they regulate genes that in turn regulate large networks of other genes (like the gene pathway that forms an appendage). They also directly regulate what are called realisor genes or effector genes that act at the bottom of such hierarchies to ultimately form the tissues, structures, and organs of each segment. (Wikipedia, s.v. *Hox gene*)

This essentially means that genes are organised hierarchically. The role of a gene in the development of biological structure is not inherently coded but rather depends on the sequence of activation and the scope of some genes over others (it is not casual that most of the 1% of difference between human and chimpanzee genomes is in master genes).

In the same way, so-called lexical parameters may have a different effect on a language's shape according to its position in a hierarchy. Baker's (2001) parameter hierarchy is the most important antecedent of a reasonable conception of the issue in analogous terms. The next figure shows a simplified parameter network of the type advocated by Baker (2001), from Holmberg and Roberts (2010):



**Figure 3:** A hierarchical network of parameters. The scheme, adapted from Holmberg and Roberts, represents the authors' proposal regarding the so-called Head Parameter and relies on conventions of markedness and other details that are not relevant here.

As stressed by Holmberg and Roberts, if there are parameter hierarchies, those parameters with high levels of generality at or near the root will be 'macroparameters', and the progressively more specialised ones that we see as we move down the hierarchy will be 'microparameters'. As these authors also point out, this concept is compatible with a minimalist outlook, including a restrictive theory of UG. According to Holmberg and Roberts (2010), macroparameters are the result of aggregates of microparameters acting together. Thus, there is no need to formulate an inherent difference between microparameters and macroparameters. If we situate a collection of parameters (clusters of grammatical properties) in a hierarchy like this, then as we move successively down the tree, systems become more marked and parameters become more 'micro' and have longer descriptions.

The P&P model proclaims that language acquisition amounts to the fixation of parameters. In fact, as both Baker (2001) and Holmberg and Roberts (2010) suggest, parameter fixation in acquisition is a definition of learning. Fixation may be just part of maturation and may be largely driven by environmental linguistic data. Holmberg and Roberts suggest that the networks predict the learning path and that they "can be thought of as 'epigenetic landscapes' down which the learner charts its path until it comes to a natural resting point, i.e. when it reaches a 'terminal node'".

In fact, comparing the ontogenetic development of organisms with language acquisition is not new, and it is a natural extension of our analogy. In both cases, an 'organism' (an animal or an I-language) is configured starting from specific 'instructions' with particular rates of growth and other epigenetic effects. According to this view, the 'unit' of parametric selection is not an abstract principle, nor is it an entire language or grammar (as in holistic typology); it is instead a set of functional heads that must be 'translated' into grammatical morphemes. For each language, the 'code' of this 'translation' may be slightly different, occasionally allowing an exponential explosion of phenotypic difference.

This way of approaching the comparison suggests a close parallelism with genetic mutations; a casual change in the distribution or the appearance of certain traits in environmental stimuli may imply the construction of a new I-language, though always under the control of UG. For example, a phonetic change may delete some morphemes—e.g., case morphemes—which could cause a change in word order. As Lightfoot pointed out, children scan their environment for abstract cues:

Contingent changes in the distribution of those cues may trigger a grammar, which generates significantly different sentences and structures. Change is not random, but it is unpredictable, a function of contingent changes in the distribution of cues. (Lightfoot 1999, 259)<sup>5</sup>

In ideal cases, the I-lexicon that children construct will be (almost) identical to that used to produce the input, but as usually happens in natural evolution, the copying process is not perfect. Genetic mutations occur in biological replication, which generate phenotypic variation. Actually, a genetic mutation is the natural equivalent of a structural reanalysis, a change in meaning or simply the adoption or spread of a new grammatical construction. As in natural evolution, a ‘genetic’ mutation may be immaterial from an evolutionary point of view, or (depending on external circumstances) it may be crucial. Thus, a lexical change may scarcely affect a language’s general physiognomy, whereas a phonetic change may bring about a vertiginous transformation that gives rise to a new I-language.

In our analogy, a child’s I-language will usually have some mutations and will be slightly different from that of her parents, but it will belong to the same ‘species’, whereas on some occasions we will have a distinct I-language (that belongs to another ‘species’, though of course it will be very similar phylogenetically). Thus, Latin and Spanish are different languages, although there is a continuous chain of native linguistic acquisition mediating their relationship. We see that the same paradox emerges here that appears in the origin of new species and that caused so many problems for Darwin.

If we return to table 4 to recap, we will see how the biolinguistic (naturalist and internist) point of view establishes languages as historically modified natural organs, just as organisms are historically modified natural objects. Insofar as languages are mental organs, their variation is limited by the biological conditioning that restricts human nature, just as organisms are restricted in their variation by the range of production of proteins and other molecules made available by chemical properties of DNA. To the extent that languages are historical objects, they show variation as a result of their peculiar and contingent history, and in the same way, living organisms show variation as a result of their peculiar and contingent evolutionary history.

The proposed analogy could permit us to conclude that if organisms are really historical variations on the same theme, languages are too, thus strengthening the universalist point of view that has always characterised Chomskyan linguistics in front of the more traditional view (but still active, see Haspelmath 2008, 93) according to which “languages can differ from each other without limit and in unpredictable ways” (Joos 1957, 96).

In fact, biochemist Michael Sherman (2007) has suggested that from an abstract enough stand point, there would be only a multicellular organism with surface patterns of variation, which is clearly reminiscent of Chomsky’s views, as reminiscent of Chomsky’s approach as the notion of *Universal Genome* Sherman uses.

## 11 Conclusions: Languages as Documents

I have suggested that from a biolinguistic point of view, languages are historically modified natural objects just as biological organisms are.

In evolutionary theory, there are two main perspectives on the nature of organisms (another interesting parallelism with linguistics). As biologist and celebrated evolutionist G. C. Williams puts it, “mechanistic biologists assume an *organism-as-crystal* and adaptationists an *organism-as-artifact* concept” (1992, 6). The organism-as-crystal view emphasises the weight of general laws of nature in conditioning or even determining the structure of living beings, while the organism-as-artefact view focuses on functions and adaptive traits as the sources of form (see Gould 2012 for a comprehensive review).

But, as modern evolutionary theory is showing (mainly in the realm of developmental approaches to evolution), both options are not necessarily opposing or exclusive, but perhaps they are ‘successive’.

<sup>5</sup> “A cue is some kind of structure, an element of grammar, which is derived from the input. The cues are to be found in the mental representations which result from hearing, understanding, and ‘parsing’ utterances” (Lightfoot 1999, 149).

As pointed out by Kauffman “biology since Darwin is unthinkable without selection, but may yet have universal laws” (Kauffman 1993, 25). In this sense, as suggested by the formal biological model proposed by Kauffman, part of the order that exists in the natural world exists “not because of selection, but *despite* it” (Kauffman 1993, 16, original italics). Gould states the alternative between the relative weight of these two types of factors in the evolution of organisms in the following terms:

In what ways does the skewed and partial occupancy of the attainable morphospace of adaptive design record the operation of internal constraints (both negative limitations and positive channels), and not only the simple failure of unlimited number of unconstrained lineages to reach all possible position in the allotted time? (Gould 2002, 1053)

One possible answer to this question includes the view according to which natural selection only operates into a restricted space of design channelized by those (still poorly understood) sources of order “on which natural selection was privileged to work” (Kauffman 1993, 643).

It seems then that natural organisms are both like crystals and like artefacts, and the same happens with human natural languages. They are an entangled mixture of biology (syntax) and history (morphology and phonology) from whose disentangling we can learn a lot on the Faculty of Language.

Actually, Williams adds a third perspective: “An *organism-as-document* approach should also be recognized for biologists interested mainly in unique evolutionary histories” (Williams 1992, 6). The organism-as-document approach is related mainly to the historical nature of living beings. Thus, the same Williams, an adaptationist, admits that “many features of living organisms are functionally arbitrary or even maladaptive” (1992, 7). He mentions that all vertebrates are capable of choking on food because the digestive and respiratory systems cross in the throat (this state of affairs is especially bad in humans, precisely as a putative adaptation to language). Williams observes that this trait is “understandable as historical legacy, descent from an ancestor in which the anterior part of the alimentary tract was modified to form a previously unneeded respiratory system” (Williams 1992, 7).

In fact, every language is a “unique evolutionary history”. Whenever a human being constructs from environmental data the interface (the I-lexicon) between the computational system and the SM system, she is adding to her FL a historical record of how this interface was made in previous generations.

The Chomskyan minimalist conception tends toward the ‘language-as-crystal’ approach, and the functionalist conception is most compatible with a ‘language-as-artefact’ approach. And, of course, the structure of human languages reveals aspects of formal elegance (as the minimalist programme of inquiry has shown) and aspects of functional efficiency (as shown by the obvious fact that they are usable for thought and communication), but there are not arguments to state that these aspects are manifested more or less intensely in some languages than in others, or in some linguistic types than in others. If I am correct, then their formal and functional aspects are part of *what is common to all languages*. This is not surprising if these factors have shaped evolutionary UG, the architecture common to all languages. Variable parts of language are therefore a reflection of the essentially historical nature of the ‘externalisation component’ of our language organs, the I-lexicon. In this sense, in my opinion, the relevant approach to languages from a biolinguistic point of view is the language/organism-as-document one. A functionalist linguist such as Givón shares this opinion: “Diachrony has the most direct causal bearing on the shape of any particular language, and thus on the diversity of human languages” (2009, 41). Of course, Givón’s approach to linguistic change is different to the one presented here because for him there exists an “invisible teleological hand that guides the ever-shifting but still roughly-isomorphic matching of structures and functions” (2009, 42).

It could be concluded that a biolinguistic approach would trivialise the research on historical linguistics and typology, but nothing could be farther from truth. On the contrary, language offers the unique opportunity to cultivate a comparative perspective on numerous diverse historical configurations of the same mental organ. This fact converts (both synchronic and diachronic) comparative linguistics into a central domain for our understanding of the true nature of language, the central research topic of biolinguistics.

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