What is a Language from a Biolinguistic Point of View?*

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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 the scientific study of languages (an abstract biology 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 natural objects. Considering that natural organisms are also 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 themselves. I consider some alternatives to this conception, especially those functional and cognitive approaches that view languages as cultural objects and I present a comparison between the two points of view with regard to the extent of linguistic diversity.

I conclude that every language is a 'unique evolutionary history'. This conclusion means that although the structure of languages reveals aspects of formal elegance (as the minimalist inquiry has shown) and aspects of functional efficiency (as shown by the obvious fact that they are usable for thought and communication), 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 our language organs.

Keywords: biolinguistics, parameter, language change, typology, linguistic diversity

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) recently pointed 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

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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. In Section 1, 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) are also historically modified natural objects, in Section 2 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 based on the biolinguistic point of view. Of course, the idea that languages are natural objects is not universally accepted. Thus, in Section 3, I will consider some alternatives to this conception, especially those that view languages as cultural objects. The fourth section presents a comparison between the two points of view, attempting to establish which one makes more adequate predictions about what we know about how languages change and diversify themselves in space and time. The fifth and last section is devoted to conclusions.

1. What kind of Linguistics is Biolinguistics?

Of course, simply to formulate the question above as such already implies that biolingistics is a kind of linguistics. And indeed, this is the first idea I want to address: that biolingistics is a kind of linguistics. What this means is not that I 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 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 biology to the study of language* but as *the amplification of biology so that it can study languages*.

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 internism.

1.1 Methodological naturalism

The term methodological naturalism simply implies that biolinguistics is a kind of linguistics that uses the same methodology as does 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. 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 is biological.

In fact, Chomsky has repeatedly stated (see, for example, Chomsky 2000, 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 object 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')[1].

In strictly logical terms, ontological naturalism should precede methodological naturalism, whereas ontological dualism (the belief that the mind is not part of the physical) should precede methodological dualism (the belief that natural science is appropriate for studying the physical 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, that 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 passive sentence. These sciences cannot even begin to adequately describe a morpheme or a phoneme, let alone the system of agreement rules in Russian.

Everyone accepts that language exists somewhere or somehow in the brain, but the truth is that we are no closer to understanding a simple

[1] "Unless offered some new notion of 'body' or 'material' or 'physical', we have no concept of naturalism apart from methodological naturalism" (Chomsky 2000: 143).

affirmative transitive sentence in strictly neurological terms than we are to travelling outside of the galaxy and coming back to tell of it[2].

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 naturalism 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 the methodological naturalism of Chomsky and his followers 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 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[3].

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.

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[2] "Despite huge progress, at a basic level we still do not understand how brains generate minds. This is as true of a dog's brain as for a human's, and it is true of very basic aspects of cognition, such as vision and motor control, along with language" (Fitch 2009: 285)

[3] This approach to language is considered with more detail in section 3.

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.[4]

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.[5] This idea is crucial and should confirm that I am 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 physics, 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 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

[4] "The standard that inspired the modern scientific revolution was abandoned: the goal is intelligibility of theories, not of the world (…) As the preeminent Newton Scholar I. Bernhard Cohen put the matter, these changes 'set forth a new view of science' in which the goal is 'not to seek ultimate explanations', rooted in principles that appear to us as self-evident, but to find the best theoretical account we can of the phenomena of experience and experiment" (Chomsky 2002: 68).

[5] "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: 79, *apud* Jenkins 2000: 32)

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 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 just a signal of a certain materialistic prejudice that is incompatible with true science.

In fact, this is one of the possible senses of the word biolinguistics that I rejected at the outset, as if postulated entities in theoretical linguistic models were going to acquire reality only once they were translated into things that could be seen, measured or weighted by biology or neurology. This is exactly what Dennet (1995) called *greedy reductionism* (also known as *cannibal 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 or a syntactic constituent. 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 (or as Chomsky used to put it, a double standard if we study up or down the neck).

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 scientific explanation.

Obviously, the latter is the approach that I support here. However, let me clarify this. 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).

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*.

This very notion of biolinguistics will allow me to use a different approach to the traditional comparison between languages and species. 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 contribution.

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: internism.

1.2. Internism

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 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 internist approaches.

Following Chomsky, I will define internism as an approach that considers the object of inquiry—that is, the faculty of language (FL)—as 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 internist 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, and thoughts—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:

Chomskyan Linguistics	**Functional Linguistics**
Internism	Externalism
Rationalism	Empiricism
Formalism	Functionalism
Universalism	Relativism

Chomskyan linguistics is associated with internism, 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. I will discuss the last line of the table in Section 5. The first three have their own history in the development of modern thinking and modern science, and here I will only sketch the connexion between them[6].

[6] See Mendivíl (2009) for a larger discussion of these issues, along with more opposing pairs.

For an internist, the formal study of an object precedes the study of its functions, which correlates better with a rationalist conception of the mind (and of science), whereas an externalist approach implies a preference for function over form, something that we easily associate with an empiricist conception of the mind (and of science). Insofar as the mainstream of natural science, especially theoretical physics, accommodates the rationalist point of view, it seems reasonable *prima facie* to assume that within the project of scientific reduction, internist linguistics will be more adequate than the externalist approach.

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)[7].

From this 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 the face of this supposition, 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.

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 that the brain keeps or codifies but rather is part of the structure of the brain.

So, the language a person speaks is his/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, biolinguistics

[7] The relativist linguist D. Everett states this clearly (the allusion to Pirahã in the quotation will be discussed later in section 4): "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)

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.

2. Languages as language organs

I concluded previously that biolinguistics is an abstract biology. Now, we can say more accurately that biolinguistics is the biology of an abstract organ.

Our next task is to try to ascertain the nature of this abstract organ. 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.

2.1. Comparing Languages and Species

Comparing languages and species is not new or original; indeed, Charles Darwin did it in a relatively profound manner[8].

Although Darwin's *Origin of Species* used to exceed half a thousand pages even in pocket editions, its contents can be summarised in a few words: the structure and evolution of living beings (including our minds) is nothing but the outcome of blind and random variation subjected to the effects of natural selection. Period.

In the evolution of natural species, there is no design, direction or finality. As the great man wrote in his now recently reedited memories (now without the effects of his wife's pious censorship), "There seems to be no more design in the variability of organic beings, and in the action of natural selection, than in the course the wind blows" (Darwin 1893: 63)

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 I have defined them before) 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

[8] See Alter (1999) for a detailed historical review and Mendívil (2006) y (2009) for an extended discussion on which I elaborate here.

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 are the terms of comparison—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*. Heredity explains why organisms resemble their progenitors (lions engender lions and not chickens); mutations explain why descendents 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[9]. 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[10].

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[9] See Mendívil (2006) for a review of classical and modern proposals.

[10] "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*

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'.

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[11]. 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.

Lass 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. What Lass observes remains true even if we limit ourselves to saying that the individual is unconscious and that he simply acts as a trigger of the process (that later will be guided by an 'invisible hand')[12]. 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

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which is characteristic of a person's mode of speaking corresponds with the individual" (Schleicher, 1863: 32, emphasis added).

[11] "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).

[12] 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 1990: 56-57).

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, as we will see).

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." (1997: 364).

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 (for a linguistic typology) 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.

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 hyperthrophic 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 amount 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[13]. 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.

2.2. The Terms of Comparison

We have seen in Section 1 that 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

[13] 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.

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'[14].

In summary, we have two essential notions of *historical biolinguistics* (the expression analogous to that of evolutionary theory): i-language and e-language. I-language is each person's language organ, a natural object whose phenotype depends on its genotype and development. As far as we are concerned, the genotype of an i-language is UG (which will acquire different states based on environmental linguistic data). E-language, 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:

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

Table 1. Correlation between natural and linguistic evolution (first version)

As shown in Table 1, 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

[14] 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.

Schleicher shaw 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 set 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).

It is very important not to make the mistake of identifying i-languages (i.e., language organs) with entire persons—that is, with speakers—as Croft (2000) does, for example, in his proposed analogy. 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.

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 Galician and Portuguese (order irrelevant). In the case of species, the capacity to interbreed and produce fertile offspring depends on a very pronounced genetic similarity between two individuals; in the case of languages, fluid and mutual intelligibility requires a very pronounced grammatical similarity between two language organs.

The proposal displayed in Table 1 clearly embraces the achievements of the 'high part' of the traditional analogy, the part that identifies species with languages but nevertheless bears in mind that we are using a strictly extensional definition of e-language (the population of i-languages) and not a 'traditional' notion of e-language as a set of utterances or an external object, which are the notions that underlie other proposals, especially those based on the identification of languages with complex adaptive systems (Deacon 1997, Kirby 1999, Briscoe 2002)[15].

However, let us also note that Table 1 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

[15] Such proposals are questionable not only because they use such a fuzzy and weak notion of language but also because they suggest as the analogue of natural species a kind of species (a language) that does not have clearly defined individuals.

discussed, equivalent to the individuals that form a species; the individuals that form a species, according to my proposal, are instead i-languages[16].

Before we extend Table 1 to consider other possible relevant concepts, we should quickly reflect on the meaning and implications of the proposed comparison. 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, face recognition or mathematical ability), 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.

Vision, memory and face recognition are mental organs that, like language, depend on the interaction between nature and environment but are essentially uniform across the species. In contrast, the structure of the faculty of language of a given person depends not only on natural conditioning and environmental stimuli but also on historical aspects that do not seem to have any significance for other organs, be they mental or purely physical. 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 1 relies.

However, there is still another point of clarification to be made before we continue that justifies even further the comparison between entire organisms and (mental) organs. If we look again at Table 1, 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

[16] If at all possible, the components of a language would be the correlates of the different organs that compose a living being.

reference for each one of these types of evolution is different. This means that in the proposal I am developing, natural evolution is a totally different phenomenon from linguistic evolution[17].

In Table 1, 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 referred to linguistic change in historical time: that is, as something totally different from natural evolution.

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 (to be done in Section 4) reveals that for many authors and even for entire research schools, clarification is indeed necessary. On the contrary, many authors tend to mix up and overlap the two processes. This has as the immediate effect of making the comparison between languages and species more of a problem than a way to encourage understanding.

2.3. 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).

[17] We will see later that this is a crucial difference from other approaches. My intuition is that establishing different frames of reference prevents some of the problems that we are going to consider in other approaches.

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*" [18].

The resulting table is the following:

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

Table 2. Correlation between natural and linguistic evolution (second version)

Comparing DNA to UG is sensible only if we interpret both notions as biological restricting frames. In other words, if DNA is 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 minimalist 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.

[18] 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.

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.

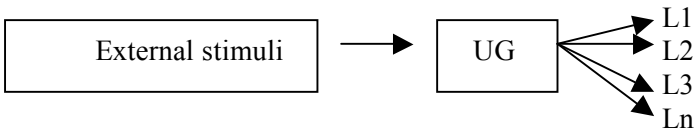

Figure 1. The role of Universal Grammar

As shown in Fig. 1, UG is usually interpreted in generative literature as the *initial state* (or S_0) of the faculty of language, whereas knowledge of a 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.

Obviously, what the scheme intends to represent is the Chomskyan innatist 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 Fig. 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) is 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 (what makes perfect sense given that newborns do not speak)[19]. This meaning does not deny that the organism is

[19] In fact, Chomsky writes, "UG is taken to be a characterization of the child's pre-linguistic initial state" (Chomsky 1981: 7).

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 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.

2.4. 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 Mufwuene (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 text 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:

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

Table 3. Correlation between natural and linguistic evolution

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' 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[20].

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 explains 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, Mendívil 2009, Biberauer 2008: 23 and ff., and Boeckx forthcoming), 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).

The influential formulation in Hauser, Chomsky and Fitch (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

[20] The 'classical' version of parametric theory amounts to say that UG principles are parametrised, that is, that 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.

language, regardless of their overlap with other cognitive domains or even with other species. Essentially, Hauser, Chomsky and Fitch propose that FLB includes a sensory-motor system, a conceptual-intentional system, other possible systems, and the computational mechanisms for recursion ('narrow syntax'). 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, Chomsky and Fitch call FLN. They hypothesise that FLN only includes recursion and is the only uniquely human component of the faculty of language. If FLN has to interact with the rest of FLB, it should also include its 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.

Thus, 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[21].

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 can be seen as an interface between (narrow) syntax, sound (PF) and meaning (LF), which would imply that what parameters express are not choices made from among previously underspecified options on UG principles but different possible structures for interface systems.

An attractive hypothesis (see Mendívil 2009 for further development) might be that during the ontogenetic development of FL, the organisation of the interfaces between components of FL is sensitive to environmental linguistic input: that is, to primary linguistic data that children employ (in classic proposals) to set the parameters. Note then that the variability of these interfaces (lexicon included) is doubly constrained: on the one hand by the computational system and on the other hand by both external systems. Of course, these systems may have their own conditioning (perhaps shared to a certain degree with other species).

[21] This implies accounting for the descriptive scope of a given postulated principle based on conditions imposed from the interfaces or general considerations of computational efficiency.

The task of interfaces is to make the representations generated by the computational system *legible* to external systems. If we assume that both conceptual-intentional and sensory-motor systems on one hand and the computational system on the other hand are essentially universal in their design, the options for variation must necessarily be limited and systematic.

This is what we find in human languages according to modern typological research: a kind of variation in which there are strong systematic correlations between the structural properties of languages.

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 interfaces. 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 system or perhaps even change the essential design of conceptual and sensorial systems, but they can crucially influence the linguistic interfaces between them.

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) recently 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 the lexicon.

The BCC is fully compatible with the model sketched above, in which parameters are 'located' at the interfaces. 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[22].

Inflectional morphology 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[23].

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.

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. 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 morphology (conceived of as an interface system 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[24].

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[22] 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 idiosyncrasic 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).

[23] It is indeed possible that the interface with conceptual-intentional systems is also a source of some linguistic variation, but as pointed out by Holmberg and Roberts (2010), it seems more probable that UG minds more about the CI interface (which would explain the paucity or lack of semantic parameters).

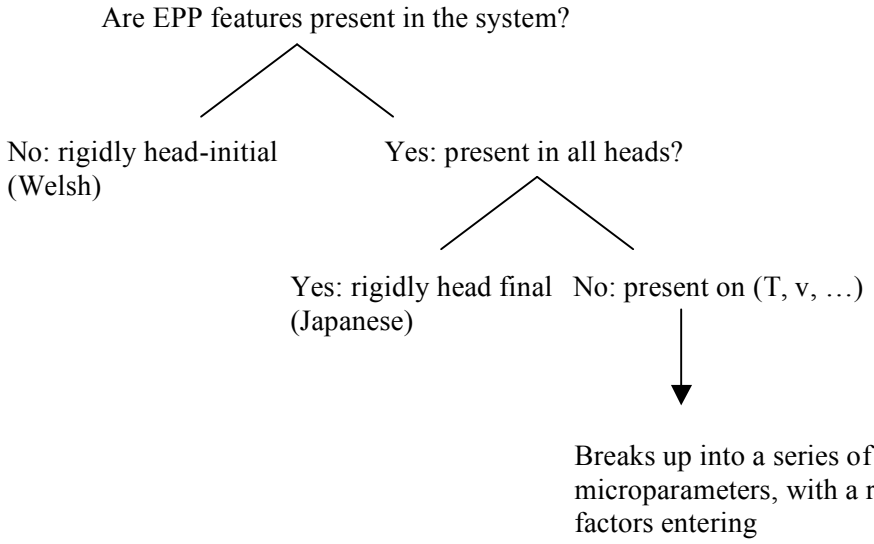
[24] Recall now Givón's famous statement: "Today's morphology is Yesterday's syntax" (Givón 1971: 413), or more recently: "As in bio-evolution, it became increasingly clear, today's extant grammatical structures and their mind-blowing diversity were best understood and explained through tracing the developmental 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[25]. 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. In the same way, so-called lexical parameters (an unfortunate expression if parameters are located at the interface) 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) and Holmberg and Roberts (2010)[26]:



[25] 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 realisator 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. *master gene*).

[26] The scheme is adapted from Holmberg and Roberts (2010). It 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 (2010), 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[27]. 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[28].

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 some possible epigenetic effects[29].

According to this view, the 'unit' of parametric selection is not an abstract principle, nor it is 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 grammatical difference.

This way of approaching the comparison suggests a close parallelism with genetic mutations; a contingent 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.

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[27] According to Holmberg and Roberts (2010), macroparameters are the result of aggregates of microparameters acting together, as a single parameter. Thus, there is no need to formulate an inherent difference between microparameters and macroparameters.

[28] See Baker (2001) for an application of this reasoning to the statistical distribution of head-initial and head-final languages.

[29] In fact, Rizzi's parameter definition suits this view well: "A parameter is an instruction for a certain syntactic action expressed as a feature on a lexical item and made operative when the lexical item enters syntax as a head" (Rizzi 2010).

As Lightfoot points 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)[30]. In ideal cases, the interface systems that children construct will be identical to those used to produce the input, but as usually happens in natural evolution, the copying process is not perfect.

Genetic mutations occur in biological replication that 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 (see Pinker 1994). 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 Catalan 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.

2.5. Two ways of conceiving linguistic diversity and change

If we return to table 3 to recap, we will see how the biolinguistic (naturalist and internist) point of view establishes language as a historically modified natural organ, just as an organism is a historically modified natural object. 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 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.

[30] "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)

Even more importantly, 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 generative grammar.

Although I am amongst those who think that this is a perfectly legitimate inference, it is also true that this inference cannot be made directly.

The crucial issue is that in the domains of both evolutionary theory and linguistic change, there is no general agreement regarding the extent of the 'creative' capacity of evolutionary processes like natural evolution or linguistic change.

When we define a language as a historically modified mental organ, we are assuming that linguistic change is the main cause of linguistic diversity, a conclusion identical to Darwin's when he suggested that species' mutability explained the existence of different species Therefore, the question regarding the extent of linguistic diversity is tantamount to the question of the transforming capacity of the change process. Can processes of linguistic change produce anything that is not a language? The answer seems clear: no. On the other hand, can linguistic change produce a language from anything that is not a language? Again, the correct answer seems to be *no*. This is the answer that I am going to support, though current theoretical approaches have not reached a consensus on the subject.

As we will see in the next section, many authors think that linguistic changes were a central part of the evolution of language (e.g., Heine and Kuteva 2007). This conclusion, in my opinion, owes a lot to an inadequate conception of language and evolutionary theory.

If, for the moment, we restrict ourselves to evolutionary theory, we must recall that there exist two main, relevant schools or traditions. On the one hand, there is the neo-Darwinist tradition, and on the other hand, there is that which I will call anti-neo-Darwinist[31].

The main difference between the two approaches is related to the role of natural selection and the adaptive character of evolutionary changes. Neo-Darwinists assume that every feature of an organism is the outcome of an adaptation process that occurs by means of natural selection, while anti-neo-Darwinists emphasise that adaptation cannot explain all existing morphology and insist on the contribution of laws of form and other principles that canalise or restrict the structure of organisms[32]. In other

[31] Note that the prefix *anti-* has scope on *neo-Darwinist*, and not only on *Darwinist*. See Gould (2002) for a detailed (and monumental) discussion.

[32] Gould states the crucial question clearly: "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).

words, this point of view objects that the neo-Darwinist choice between God and natural selection is too restrictive.

Gould (2002) has explained the terms under discussion using the metaphor of Galton's polyhedron taken from Darwin's "brilliant and eccentric cousin" Francis Galton. According to a neo-Darwinist point of view, an organism can be conceived of as a moving billiard ball. When the cue hits the ball, it produces variable and different ball movement. There exists unrestricted variability. The cue is natural selection and the ball goes wherever 'selection' pushes it. According to Gould, this conforms to an externalist, functionalist, and adaptationist theory of evolution. In contrast, from an anti-neo-Darwinist point of view, the organism would be like a polyhedron that stays poised on one side unless it is pushed hard enough to topple it onto an adjacent facet. Of course, the action of natural selection is necessary, but once the polyhedron is hit, the possibility of change is internally constrained. The polyhedron has a structure that restricts variation in such a way that some options are more probable than others and certain options are impossible, regardless of how much interesting they might be adaptively.

The main difference between the two viewpoints lies in the degree of relevance conceded to physical laws and to auto-organisation principles in the explaining the existing morphology. For a neo-Darwinist, the limits are so lax that all existing morphology follows from natural selection, whereas for an anti-neo-Darwinist, the 'skewed occupancy of morphospace' is the outcome of certain restrictions and the evident labour of natural selection is not enough to explain the forms attested.

To put it in simpler terms, the neo-Darwinist model focuses more on differences, while the anti-neo-Darwinist one focuses on resemblances and on possible restrictions on variation.

This is a familiar controversy in linguistics. It is a subject of debate in the literature on linguistic diversity, as these two contrasting quotations show:

- "Languages can differ from each other without limit and in unpredictable ways" M. Joos (1957: 96)

- "There is only a computational system and one lexicon, aport from its limited kind of variety" N. Chomsky (1995: 170)

It could be said that for Joos languages are historically like billiard balls, while for Chomsky, they are instead like polyhedrons.

We know that from a Chomskyan point of view, the brain is structured before experience; the mind is not a blank slate on which experience writes but is instead a modular system designed by evolution to learn some things and not other ones. UG is expected to be one of these

conditioning systems that will impose certain properties on languages, condemning them to resemble each other. On the other hand, the opposing point of view is based on an empiricist conception in which the mind obtains its structure from external experiences and, consequently, will not impose any specific restriction on languages beyond those common to learnable systems, in such a way that languages will be able to vary indefinitely within these lax limits.

Note that the parallelism with biology is remarkable. Thus, while neo-Darwinists deny the existence of 'laws of form' or other factors that restrict or channel the work of natural selection further than fundamental physical laws, functional linguists do not accept the existence of specifically linguistic principles or mental structures that restrict the ways in which languages can change beyond what is learnable or usable in general.

What this implies is that we are confronting non-internist approaches to language. However, if a language is not a natural object, then it must be a purely cultural object.

3. Languages as cultural objects

Martin Joos' position might seem old-fashioned, but there is nothing farther from truth. More or less explicitly, this is the stance that functional and cognitive linguistics has taken during the last thirty years. In fact, it was recently adopted by Haspelmath (2008) and by Evans and Levinson (2009)—in an article that, according to the authors, offers a new relativist research programme alternative to Chomskyan approaches.

In my opinion, what these traditions share is that they are functionalist (externalist) approaches to both language and the mind. This prevents our considering them as part of the biolinguistic enterprise.

What is essential to a functionalist approach is what is of relevance: not an object itself, its inherent structure, but rather what it is used for or what functions it has.

Focusing on human language, one can say that for the formalist, language is an attribute of the human mind/brain, whereas for the functionalist, language will be *represented* in the mind/brain. However, if it is represented, then it is external to the mind and the brain.

A clearly externalist approach to language is that of neurologist Terrence Deacon:

"In some ways it is helpful to imagine language as an independent life form that colonizes and parasitizes human brains, using them to reproduce" (Deacon 1997: 111).

This is a fascinating point of view, but as a research programme, it entails severe difficulties. The main one has to do with precisely the nature of the object under study. In this and other externalist theories, there is a tendency to believe that the real object of study is not i-language (that is, a person's language organ) but rather an external language: that is, language as a shared social object.

It is implied, then, that external languages are what really exist, whilst i-languages are nothing but manifestations or reflections of these entities in individual minds. But this is in fact Saussure's notion of *langue*.

The idea is more or less the same as (returning to our analogy between languages and species) the idea that what really exists is the species (e.g., the species of horses) and that the individuals (horses) are nothing but manifestations of this species.

It seems more reasonable to say that what really exist as natural objects are horses and individual i-languages and that natural species and external languages are nothing but populations of those objects.

The functionalist perspective implies that languages evolve by adapting to use and acquisition prerequisites. In other words, it is not the brain or the mind that has evolved for the sake of language; instead, languages have evolved to be learnable and usable for brains and minds. This idea sounds tempting but is unacceptable; it is the same as saying that organisms have evolved to be expressed by DNA.

Let us imagine water flowing through a stone channel. Of course the water adapts to the channel's shape, but it would be dangerous to ignore that then we are also meaning that the structure of the channel determines the shape that the water takes. But this is what Deacon seems to be doing:

"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)

Then, however, to explain the common structure of languages without including the contribution of the mind/brain, we need to invoke the (supposed) converging evolution of languages:

"Grammatical universals exist, but I want to suggest that their existence does not imply that they are prefigured in the brain like frozen evolutionary accidents [...] they have emerged spontaneously and independently in each evolving language, in response to universal biases in the selection processes affecting language transmission. They are convergent features of language evolution in the same way that the dorsal fins of sharks, ichthyosaurs, and dolphins are independent convergent adaptations of aquatic species" (Deacon 1997: 115-116)

Interestingly, our favourite analogy re-emerges, and again, it is going to be highly illustrative. Still, first note the flaw in the argument: again, the role that the stone channel plays in determining water's shape is ignored. If

the mind and the brain impose conditions on language acquisition and evolution (change), then there is no sharp difference between this stance and the classic Chomskyan one according to which UG (i.e., some part of the mind/brain) conditions the structure of possible human languages[33].

It is illuminating to observe how both Deacon and Briscoe seem to take for granted that the resemblance between legs, wings, eyes, and fins in the animal kingdom can be explained via pure analogy— that is, as a product of independent (convergent) evolution.

However, this is not the only conceivable path. Modern genetics has shown that there are unexpected cases of "deep homology"; that is, major phyla that are separated by more than 600 million years of independent evolutionary history today share substantial channels of development based on levels of genetic retention (for example, the so-called Hox genes) that "proponents of the Modern Synthesis had specifically declared inconceivable, given the presumed power of natural selection to modify any independent line in its own uniquely adaptive direction" (Gould, 2002: 1056).

As Sampedro has pointed out (2002: 119 ff.), Gehring's group demonstrated in 1994 that the gene *PAX-6* (*eyeless* in Drosophila) is the same master gene that controls the dozens or hundreds of genes that form the eyes both in arthropods and in human beings. It is evident again that although natural selection has modified many of these genes to produce eyes as incredibly different as a crustacean's complex eye and a human eye, the question is really one of a 'deep homology' (with a role similar then to that of UG) that determines (at least) some of the universal properties that we find in languages.

In a curiously non-trivial way, the same can be said in general terms about the rest of the appendices mentioned in the preceding quotations: together with eyes (which have been protagonists since Paley's time), wings, legs and fins have traditionally been used as examples of evolutionary analogy and convergent evolution—clear exponents of how the environment shapes the adaptation of organisms. Nevertheless, all of them have been demonstrated to have been, so to speak, invented just once in nature:

"The legs (and other appendices) of all bilateral animals are constructed following a complex design system that already existed in Urbilateria [the first postulated bilateral animal] and that all bilateral animals have used without exception, and that natural selection has not

[33] In the same vein, Briscoe (2002) makes a similar remark, in this case referring to eyes and wings: "In the framework advocated here, we can recognize that such historical pathways can be stereotypical responses to similar pressures arising in unrelated languages, in much the same way that eyes and wings have evolved independently in different lineages many times, without the need to posit a substantive theory of such changes or to see them as deterministic" (Briscoe 2002: 13).

altered fundamentally during the 600 millions of years that have passed since its emergence" (Sampedro 2002: 128, my translation JLM).

Perhaps similar historical paths or linguistic universals themselves are the consequence of convergent but independent adaptive patterns, adaptations of languages to the human mind/brain; however, the lessons of evolutionary biology do not seem to advise that we relinquish the idea of an (historically) invariable UG as the unavoidable source of such patterns.

4. The Uniformitarian Hypothesis: Assumptions and Predictions

It is important to take into account that the two approaches we have reviewed make very different predictions about linguistic changes and, consequently, about the extent and depth of linguistic diversity.

According to a biolinguistic stance, linguistic change is severely restricted by formal requirements dictated by our biological specialisation for language, or what Pinker successfully called *the language instinct*. This implies that human languages are essentially manifestations of the same system, as Chomsky claimed.

According to a non-biolinguistic standpoint, language change is constrained only by external factors (including our species' anatomy and physiology but also functional, social and cultural factors). This implies that languages can vary unpredictably and without limits, as stated by Joos.

Based on these two different predictive profiles, it could seem that the so-called *language uniformity hypothesis* should be embraced only by biolinguistics' supporters, but this is not the case.

The language uniformity hypothesis (LUH) states that all languages have the same degree of development and that primitive languages therefore do not exist:

"It can be stated that one of the most important achievements of current linguistics is to have revealed that there are no primitive languages" (Moreno Cabrera 2000: 12, my translation JLMG).

The LUH is common in handbooks and introductions to linguistics regardless of their theoretical commitments (perhaps as a politically correct compensation for the denigrating assertions of the past).

The LUH formulation that can be found in handbooks has several components:

- There are no primitive languages
- All languages have the same degree of complexity
- All languages can satisfy the same functions
- All languages have the same dignity
- All languages have, at an abstract level, the same structure

- All languages have the same basic components
- All languages offer the same degree of difficulty of acquisition as first languages

Although some of these assertions might be true or false regardless of whether the others are or not, I will use the more extended version of the LUH that somehow compiles all of them: all existing languages present the same degree of evolution such that there are no languages that represent a previous or less developed state of human language.

Although they are subtly connected, the LUH and the so-called *uniformitarian principle* (UP) should not be identified. The LUH is an empirical hypothesis (that is, an assertion regarding the existence or inexistence of certain objects), whereas the UP is a methodological principle of inquiry.

In general, uniformitarianism assumes that the same natural laws and processes that operate in the universe now have always operated in the past and apply everywhere in the universe. This concept is usually stated as the idea that 'the present is the key to the past' because it holds that all things continue as they were from the beginning of the world[34].

Historical linguistics assumed the UP early on (see Lass 1997: 24 ff.). Lass's formulation of the UP in historical linguistics is the following: "No linguistic state of affairs (structure, inventory, process, etc.) can have been the case only in the past" (Lass 1997: 28). This principle, common to every historical science (be it physical, biological or linguistic), says that on principle, we should discard reconstructions of the past that imply currently impossible states[35].

Although the UP and the LUH are different, it is clear that in linguistics the methodological value of the former depends on the empirical reality of the latter. Also note that the UP is reliable if we identify the evolution of language (as a faculty) with *natural evolution*, as I have done in Table 3, not if we identify it with *linguistic evolution*. This means that we should rely on the UP only if we neatly disconnect the evolution of the faculty of language from the historical evolution of languages.

In fact, the LUH extends naturally to documented ancient and extinct languages and, by virtue of the UP, even to reconstructed ones. There are good empirical reasons for that move:

> "This survey has uncovered no evidence that human language in general has changed since the earliest stage recoverable by the method used here. There is simply diversity,

[34] The principle is commonly attributed to geologist Charles Lyell. The subtitle of his influential *Principles of Geology* (1830) lays out it clearly: "An attempt to explain the former changes of the Earth's surface by reference to causes now in operation".

[35] Comrie (1992: 204) provides a rather trivial example: because all attested languages have consonants, a reconstruction must be rejected if it posits an ancestor language that has no consonants.

distributed geographically. The only thing that has demonstrably changed since the first stage of humanity is the geographical distribution of diversity" (Nichols 1992: 277).

Actually, the LUH poses two main problems: (i) whether it is correct, and (ii) if it is indeed correct, why.

I do not think it excessively hazardous to state that the vast majority of linguists (explicitly or implicitly) accept the LUH, and because of that, I am going to focus on question (ii). This question is important because even though there is general consensus about the LUH, it is not clear if the LUH can be *deduced* from all approaches—and this is relevant because in science, what matters is not only the outcome (apparently identical in this case, the LUH) but also how this result is obtained and whether it is predicted or not.

If we ask the countless supporters of the HUL why there are no primitive languages, we can expect two kinds of answers:

1. All current and historically recorded languages are the product of a unique language faculty, which is a result of the natural evolution of the species. Thus, every natural language spoken by a human being is restricted or conditioned by this faculty and cannot be primitive. Primitive languages disappeared when species with primitive language faculties disappeared.
2. All current and historically recorded languages have been evolving for tens of thousands of years, constrained by human processing and acquisition systems and by the cognitive and communicative functions they must fulfil. During this time, either modern human languages caused the extinction of primitive languages or the latter turned into the former.

It is easy to recognise a biolinguistic stance in answer 1, whereas a type-2 answer would be offered by those authors who think that the essential structure of languages is not biologically determined but is the outcome of those external pressures to which languages are subjected due to their use for communication and thought.

This approach underlies a good deal of functional linguistics (including the work of authors such as Givón, Comrie, and Hawkins) and Deacon's co-evolutionary model (followed to a certain extent by authors such as Hurford, Kirby, Briscoe and even Evans and Levinson 2009).

A type-1 answer can be considered *homological*—not, of course, because it is assumed that all languages come from a common ancestor, but in the sense that it is assumed that all languages are variants of a common language faculty. Type-2 answers can be considered *analogical* theories of

uniformity in the sense that it is each language's (long-term) historical evolution that would explain uniformity as confluence.

As pointed out in Section 3, a relevant (and not always remarked upon) difference between both approaches with regard to the LUH is the degree of relevance conferred to linguistic change in the process of language evolution. For a homological theory, linguistic change has no functionally positive or negative effect; it is a process that is totally independent of the evolutionary emergence of language itself, an issue that has to do with the evolution of the human species. In contrast, for analogical theories, linguistic change is sensitive to these external factors and (although this is not always explicitly recognised) is part of the evolution of language.

In English, but not in other languages, the word *language* serves two functions: it represents particular languages (cf. French *langue* or Spanish *lengua*) and also language in general (cf. French *langage* or Spanish *lenguaje*). Thus, expressions such as *language evolution* or *language development* are inherently ambiguous. *Language evolution* can refer to both the historical changes in languages and the evolutionary process that created our special ability for language[36]. However, this "unfortunate ambiguity" (Hurford 1992: 273) is not the only cause of frequent uncertainty regarding the use of expressions such as *language evolution* or *language development*. For some authors, there is really an authentic vagueness to the process of language evolution and the process of language change, which is of great importance in predicting the LUH. From this vague viewpoint, language evolves across the evolution of languages, which in turn are influenced by cultural evolution—whereas for 'homologists', languages do not evolve but simply change.

Consider, for example, the next fragment from Comrie (2003):

> "As a result of the recent development of grammaticalisation as a tool in historical linguistics it has been possible to develop a more general variant of internal reconstruction (…) that does enable us to come up with plausible hypotheses concerning earlier states of *language development*" (Comrie 2003: 249, emphasis added).

What is the celebrated linguist referring to, linguistic change or language evolution? The short text provided, which includes one reference to the method of internal reconstruction and another to the concept of grammaticalisation, may suggest that Comrie refers to linguistic change in historical time, but this is not the case. He really refers to both, simply because a purely historical conception of languages admits no precise way

[36] Normally the term *evolution* biases the interpretation towards the natural process. I use the term *evolution* without meaning 'progression', 'advancement' or 'improvement', either in the biological or in the linguistic context. In fact, I use the term simply to mean 'change'.

to differentiate between the two phenomena. This, in my opinion, weakens the LUH notably.

Later, in the same article, our suspicion is confirmed:

> "We can take grammaticalisation and base on it a kind of generalised internal reconstruction that gives us access to hypotheses concerning earlier stages of the language in question and by generalising our conclusions to earlier stages of language in general" (Comrie 2003: 249).

The reference to 'earlier stages of language in general' only has meaning as an allusion to the development of the faculty of language in the species. However, note that we are then admitting that linguistic change is directional and capable of changing the faculty of language (if recognised as such at all). Insofar as languages change at different rates and in different directions, we must also admit that the faculty of language is not uniform across the species and that, in fact, this faculty continues evolving among different groups.

Perhaps a schematic approach can clarify the difference to which I am referring. Figure 3 represents the evolutionary scenario that underlies the homological hypothesis:

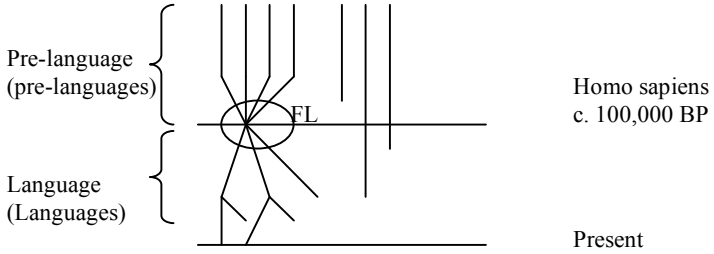

Fig. 3 A homological theory of language uniformity

Fig. 3 is intended to show the emergence of FL as an evolutionary singularity (a 'bottleneck') from which the languages developed (as descending lines in the scheme) will be uniform, although we can allow for a certain amount of diversity as a result of linguistic change (which is shown by the usual family tree model). Before this evolutionary event (that I date 100,000 BP rather arbitrarily) there existed *pre-languages*: that is, the 'languages' used by ancestors of humans not yet equipped with a modern FL[37]. On the right side, I represent the process in other species (for example, the process for Neanderthal's (pre)languages; the Neanderthals

[37] I use the terms *pre-languages* (and *pre-language*) instead of Bickerton's (1990) classic term *protolanguage* because the latter has its own use and tradition in historical linguistics (as in *proto-Germanic*, etc.). Besides, I do not mean that modern languages are historical descendents of pre-languages (they could be, of course, but according to the model adopted, that would be irrelevant).

supposedly did not make the jump to modern language and were extinct c. 30,000 years ago).

Under the singularity marked with the ellipse, all human languages—modern or ancient, recorded or unrecorded, and regardless of their morphological or phonological complexity or their typological profile—will be modern languages.

The analogical scenario may be represented as follows:

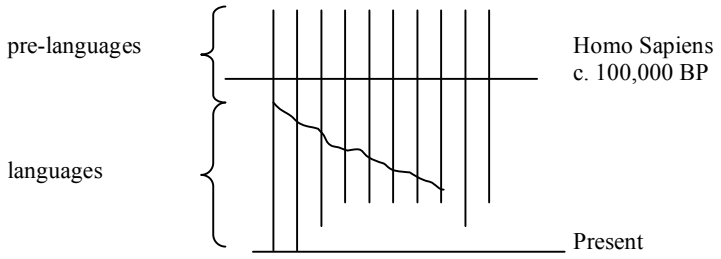

Fig. 4 An analogical theory of language uniformity

Fig. 4 represents a scenario in which human biological evolution is a condition for language development but in which biology does not specify the structural properties of languages and there is therefore no 'bottleneck'. Once languages crossed this frontier of human evolution, they started the structural complexity that now characterises them through linguistic evolution.

The critical difference is that the horizontal line that separates cognitively modern humans and their ancestors (again dated 100,000 BP rather arbitrarily) does not coincide in time with the line that separates primitive languages and modern languages (the latter is represented by the descending irregular line in the scheme). What this line is intended to show is the historical moment from which every historical lineage reaches by the effect of linguistic change the status of non-primitive language. This is a historical moment that may be different for each linguistic lineage and even for each language. The scheme includes the possibility that certain human languages (now extinct and unrecorded) did not successfully cross that frontier (e.g., the two lines on the right in the scheme).

Notably, the LUH also follows from this scenario, but in a rather different way. Under this approach, it is conceivable that some languages, although they are spoken by anatomically modern humans, continue to be primitive languages. Actually, this analogical model predicts that it is highly probable that during long time-spans, some anatomically modern human groups would speak primitive languages while others spoke modern languages.

According to this model, if there are no primitive languages today, it is because they have either become extinct or evolved historically towards

modern status. In other words, in the analogical scenario, the LUH is contingent even though it remains a highly probable theory.

Now it is easier to understand why Comrie (2003) asserts that current studies on grammaticalisation (which conceive of it as unidirectional) can give us some insight into early states of human language and why he proposes to relativise the UP, excluding from it ancestral language states.

Under this approach, the transition between modern and primitive languages is gradual and merely historical. Language evolution (beyond a general biological endowment) limits itself to the evolution of languages; therefore, the reconstruction of the past of languages is equivalent to the reconstruction of primitive language.

In my opinion, this analogical scenario presents some empirical and methodological problems that the homological one lacks: (i) the LUH is not predicted but only made probable, which gives credit to the possible existence of primitive or less developed human languages, something that does not seem to be empirically supported; (ii) linguistic change is predicted to be directional and progressive, and this claim is unsupported; (iii) the UP of historical linguistics is weakened; and (iv) to explain the evolution of language is rendered more difficult.

As for (iv), this objection is applicable only to Deaconian, co-evolutionary explanations and not to biolinguistic ones.

In Deacon's tradition, the ambiguity of expressions such as *language evolution* entails not only vagueness but also a deliberate mixture of the two meanings (this is precisely the meaning of 'co-evolution'). According to this model, the proper historical (cultural) evolution of languages was a factor in the adaptive biological evolution of the human brain. Although brain evolution is implied, it is assumed that languages evolve to be learned and used by human brains, while the brain limits itself to general qualifications:

> "The brain has co-evolved with respect to language, but languages have done the most of the adapting" (Deacon 1997: 122).

Note that if we assume this model to be correct, we need also assume that all languages have reached a sufficient level of complexity to affect their users' brains (that is, to function as adaptive factors for the brain). However, the model then predicts that it is possible that there exist human groups with pre-human cognitive abilities: those whose languages did not evolve in the proper way. I know of no one who has claimed this, including Daniel Everett, the champion of modern relativism.

The main difference between the analogical model and the homological one is that the former lacks the boundary provided to the latter by the assumption of a qualitative jump between classes of languages. The

postulation of a biological evolutionary endowment that gives rise to a modern FL allows one to establish a historical discontinuity between the classes of languages generated by evolutionarily differentiated FLs. The absence of this boundary from the analogical model causes one to attribute the LUH to the hypothesis that languages have converged into uniformity via the effect of the passage of time and external pressures. But note that then, the LUH is not predicted (let us say it is a possibility, not a necessity) and henceforth, we must entertain the possibility that this confluence has not occurred.

In fact, some authors explicitly consider this possibility:

> "Perhaps not all the languages that today exist in the world are in the same evolutionary stage" (Manjón y Luque 1997: 218, my translation [JLMG])

Others have passed from the theoretical possibility to the presumption of having discovered primitive languages—that is, languages that because of diverse historical ups and downs failed to reach the aforementioned boundary. Everett (2005) proposes as much about the Amazonian language Pirahã, which according to him lacks one of the (supposedly) central traits of human languages: recursion. Everett claims that in this language, phrases cannot be embedded into other phrases (so that there are no subordinated clauses). Everett interprets this as a cultural constraint on grammar and presents it as an empirical argument against a naturally determined FL and in favour of relativism.

Nevins *et al*. (2007) forcefully refute this analysis. In his reply, Everett (2007) admits that he does not mean that the Pirahãs' minds are limited in the use of recursion or the use of recursive languages, which settles us on the analogical model; what Everett is implying, then, is that some of the languages in Fig. 4 did not become extinct but rather still exist today (as in Figure 5). This is a denial of the HUL.

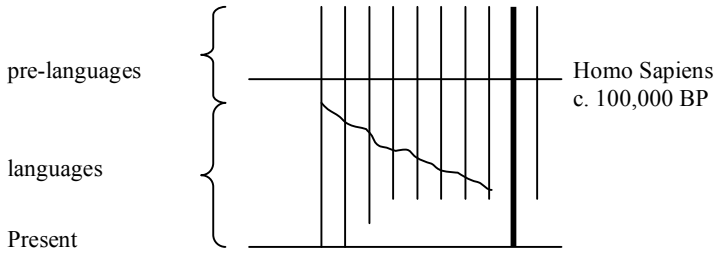

Fig. 5 A negation of the Language Uniformity Hypothesis

The scheme is the same as before except for the bold line representing a language that has not reached the critical evolutionary boundary between primitive and modern languages as of the present time.

However, what really matters is that under an analogical approach, the difference between denying the LUH and supporting it is purely accidental; it depends on whether languages have evolved properly.

It is true that the homological model smacks of circularity insofar as modern languages are defined as those spoken by modern humans. However, in the last analysis, this is the crucial matter: is the boundary between pre-language and language just cultural, or is it biological?

To answer that it is both things (as in co-evolution theories) amounts to saying that it is cultural insofar as if the historical process does not occur, then neither does the transition. If we make this claim, however, we presume that there exist modern human beings that nevertheless speak primitive languages—that is, classes of languages produced by ancestral language abilities that for contingent reasons (social or cultural) have not had sufficient time to evolve in the expected way[38]. I think that this is the only foundation on which the emerging new and radical relativism can be constructed, but it is a rather weak one.

My conclusion is that the empirical robustness of the HUL can be seen as an argument in favour of the biological theory of the uniformity of languages.

5. 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. We have also seen that in evolutionary theory, there are two main perspectives on the nature of organisms. 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 adaptive traits. However, 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 to organisms does not contradict the other two and 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

[38] Even Comrie, when he suggests distinguishing between "the human language potential" and the "realisation of the human language potential" (Comrie 2003: 250), is obliquely introducing an anti-unformitarian stance.

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).

It seems clear that all three perspectives are relevant in the explanation of biological structure and that all three should be relevant to the study of the evolution and nature of the faculty of language.

However, I do not believe that we have reason to say that all three approaches are relevant for the study of linguistic change and the subsequent linguistic diversity. In my opinion. the only relevant approach in this case is the language/organism-as-document one[39].

In fact, every language is a "unique evolutionary history". As we have seen, the Chomskyan, minimalist conception tends toward the 'language-as-crystal' approach, and the functionalist conception is most compatible with a 'language-as-artefact' approach. In my opinion, both points of view are relevant to a certain extent in explaining the evolution of the faculty of language, but I do not see any reason to extend this to linguistic change. Of course, the structure of languages reveals aspects of formal elegance (as the minimalist inquiry has shown) and aspects of functional efficiency (as shown by the obvious fact that they are usable for thought and communication), but I do not see 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. More to the point, the LUH suggests that this is not the case. If we are 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 our language organs.

It could be concluded that this point of view trivialises 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 nature of language, our central research topic as linguists.

[39] 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).

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