Biological pluralism in service of biolinguistics

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Abstract

The aim of this chapter is to offer a fresh perspective on what has come to be known as biolinguistics, a term which, in our view, encompasses all research and methods devoted to the unveiling of the biological foundations of human language. More specifically, our aim is twofold: first, we point out some of the shortcomings of the naive view of biology that has been in place in linguistics since the 1950s and 1960s, namely the notion of the faculty of language as a novelty and the sharp distinction between I- and E-language, which, we contend, has not provided any major insights into the biological nature of language; second, we offer some of the insights from biology, which may provide the theoretical and methodological framework which allows for a truly biological study of language, and thus for a re-hauled biolinguistics Chomsky (1957).

1 Introduction

The first sign of a biological orientation for the study of language was the work of Noam Chomsky and Eric Lenneberg, among just a few others, who in the 1950s and 1960s rejected the structuralist linguistics of the time, believing instead that languages, despite meticulously described, were not explained as a natural phenomenon. The overarching assumption of their work is that

languages are not learned in the conventional sense of the term (i.e. the way one would learn a craft or how to play a musical instrument), but rather a product of a biologically determined and biologically constrained capacity of humans, located in the brain, which must be innate. This biologically determined capacity is considered to be the main focus of Generative Linguistics, and all efforts carried out within this approach since its inception are said to ultimately contribute to its study. Later, mainly in the 1970s, various interdisciplinary meetings were held, and the term "biolinguistics" was ultimately chosen as the name of the enterprise that arose in those discussions (Piattelli-Palmarini, 1974). Many linguists have indeed adhered to this conception of the field, which is something that, one would expect, must result in important insights, after around five decades of intensive research. However, a brief, random survey of the thousands of papers, chapters and books on generative linguistics will reveal a pattern: is it customary to start with a mention of the biological character of language, attributable to the genetic endowment of humans, and to convey the intention of approaching it as such, but the biological jargon is soon diluted in formal linguistic analyses as the sections unfold, with a possible reprise in the conclusion. Upon close inspection, one concludes that the larger part of the issues that most generative linguistics work covers are philological in character, albeit through the use of sophisticated tools and notation.[1] Thus, a very important (and unfortunate) realization when looking at the generative linguistics literature is that its main premise (that language is a biological property of humans) does not entail, guide or constrain linguistic research in any meaningful way. In other words, if that premise were not held, the import of most linguistic work would remain largely unaffected, which is quite odd, for that premise is precisely one of the main tenets of generative linguistics. This apparent lack of interest for the biological half of biolinguistics is one of two problems regarding how investigations into the nature of the language faculty have been carried out. The second, related problem is the conception of biology itself that has served as the (rhetorical) backbone of some of these investigations. We will discuss the treatment that notions like novelty and variation have received in the linguistic literature, sometimes under the "biolinguistics" rubric, and offer some insights and counter-evidence from evolutionary

[1] We are aware that ascribing a philological character to work in generative linguistics causes discomfort and even confusion among its practitioners for historical reasons, but we lack a better word for characterizing work on the particularities of specific languages, which is not what generative linguistics is supposed to be concerned with.

biology, in favor of a biologically informed study of language.

2 Novelty: the case of language

Circumstancial evidence uncontroversially suggests that language is unique to humans. While other—if not most—species display some system of computation and/or communication, it is observed that none of these systems come close to human language as a whole, and that language has a lot to do with why humans have thrived.[2] All serious linguistic traditions have held this assumption, and the Chomskyan tradition has taken it to be not only an assumption but also its main focus of inquiry (which we, as stated above, do not think has resulted in new insights, despite the good intentions.)

One of the issues frequently discussed when it comes to language is domain specificity. Apart from references to the notion of "genetic blueprint", which are based on a naive and superficial understanding of biology, upon closer inspection of the biolinguistic literature of the last decade or so one also finds references to evolutionary biology, and Evo-Devo. This is rather odd, as linguists seem to claim on the one hand that language is acquired virtually instantaneously, yet on the other they often claim to assume a biology where development is important. The discussion of the contributions of both genes and developmental processes to the linguistic phenotype is a very interesting endeavor, but alas it is rarely pursued in linguistics. That linguists make references or are even aware of Evo-Devo literature is a virtue, but most times it is hard to see exactly which Evo-Devo linguists have in mind. The Evo-Devo literature teaches us a lesson which is more consonant with the ideas of Lenneberg, but which is actually quite incompatible with what linguists seem to need to back up their claims about the nature of language, biologically speaking. The illustration we choose to make this general point comes from a famous and influential paper published by Hauser et al. in 2002.

One of the most—if not the most—well-known notions that have emerged from that paper is the distinction between the Faculty of Language in the Narrow Sense (FLN) and Faculty of Language in the Broad Sense (FLB).

[2]Of course, statements of this kind should not be taken as qualitative, that is, the sophistication attributed to human language does not imply a higher degree of biological sophistication in comparison with other species and traits. Rather, it just so happens that language—whatever it is—has given humans behavioral advantages as individuals and as groups which have allowed them to become a privileged, sophisticated species.

This distinction has actually been taken as foundational in the field of biolinguistics, or at least in the field of linguistics when the biological foundations of language are the concern. We will, however, try to show that from a biological perspective this distinction is not useful. This should already be apparent, given the contrast between the great deal of attention it has received and the lack of results that have come out of it.

Evo-Devo is concerned with "organismal form, shape, morphological structure and the generative mechanisms underlying their evolution" (Müller, 2005). These are the main topics that an Evo-Devo researcher ultimately wants to address and understand. Regarding the generative mechanisms underlying the evolution of structures, Evo-Devo practitioners have stressed the importance of understanding the origins of novelty, both in the context of development, ontogeny and evolution, phylogeny. One of the central issues of current Evo-Devo is thus what explains the emergence of radically novel structures. As central as it might be, however, this issue is largely unresolved:

> [W]hile biologists have made great progress over the past century and a half in understanding how existing traits diversify, we have made relatively little progress in understanding how novel traits come into being in the first place.
>
> (Moczek, 2008, 432)

In fact, the issue of novelty was disregarded in the context of the Modern Synthesis. Echoing the general sentiment of the biologists of time, Ernst Mayr said the following:

> The problem of the emergence of evolutionary novelties consists in having to explain how a sufficient number of small gene mutations can be accumulated until the new structure becomes sufficiently large to have selective value.
>
> (Mayr, 1960, 357)

Current Evo-devo tries to bring back this issue into the central fold of biology. It is one thing to address the diversification of something already present, and it's another thing to find out about how that something got there in the first place. One of the things Evo-Devo quickly learned is that

the extraordinary morphological diversity that exists at the level of organisms and their parts is not paralleled by corresponding diversity in genetic and developmental mechanisms. This has led to the idea that the mechanisms of development and also genetic circuits involved are highly conserved across species. Nonetheless, the phenotypes at the end product are quite different. At the lower level of genetics or maybe even development one finds something that's highly conserved and not specialized.

Müller & Wagner (1991) made an important contribution to Evo-Devo by defining a novelty as a structure as follows: "[a] morphological novelty is a structure that is neither homologous to any structure in the ancestral species nor homonomous to any other structure of the same organism" (p. 243). This definition is quite significant, and aware readers should recognize it as a generalization of what Hauser et al. said in their famous paper about language. Even though they did not mention the Evo-Devo literature, FLN was defined by Hauser et al. (2002) as something specific to language and specific to humans, something which echoes precisely the definition of Müller & Wagner (1991) of novel trait, applied to language. In this sense. the component(s) of language they want to highlight are a very clear case of novel trait.

However, we must point out, biologists nowadays find it extremely difficult to regard traits as novel if Müller & Wagner's (1991) distinction is taken seriously. From a reasonably representative set of examples, (Moczek, 2008) is left with two that conform to it: butterfly wing patterns which are unique to *Lepditoptera* (Nijhout, 1991), and firefly lanterns and their lighting patterns, which are not present in any other insect or arthropod (Lloyd, 1983). It seems that the more we fine-tune our criteria for novelty on the basis of our knowledge of modern evolutionary biology, the shorter the list of possible candidates becomes.

Hauser et al. (2002) continued a trend that Chomsky has been famous for, namely that there is something highly specific to language and humans. In biological terms, the claim that a trait is unique to a species amounts to a claim for its novelty, especially if the trait in question is thought to be unprecedented in nature. What's interesting to our discussion is that, according to the authors, the real motivation for the FLN/FLB distinction was an attempt to make research advance:

> "Linguists and biologists, along with researchers in the relevant branches of psychology and anthropology, can move beyond un-

> productive theoretical debate to a more collaborative, empirically focused and comparative research program aimed at uncovering both shared (homologous or analogous) and unique components of the faculty of language."
>
> (Hauser et al., 2002, 1578)

We contend that this distinction actually leads to unproductive theoretical debates. Hauser et al. (2002) are correct in that it seems that some subset of the mechanisms of FLB are both unique to humans and language itself: that's the novelty aspect. The exact characterization of what constitutes FLN, however, has not been very precise. In line with the Chomskyan tradition, FLN has received the most attention in the linguistics literature, despite the vagueness of the characterization provided in the paper, as illustrated by the following passages, all taken from neighboring pages of Hauser et al. (2002):

> We hypothesize that FLN only includes recursion and is the only uniquely human component of the faculty of language.
>
> (p. 1569)

> We assume [...] that a key component of FLN is a computational system that generates internal representations and maps them into the sensory-motor interface by the phonological system, and into the conceptual-intentional interface by the (formal) semantic system. [...] All approaches agree that the core property of FLN is recursion.
>
> (p. 1571)

> In fact, we propose in this hypotehsis that FLN comprises only the core computational mechanisms of recursion as they appear in narrow syntax and the mappings to the interfaces.
>
> (p. 1573)

> At minimum, then, FLN includes the capacity of recursion.

(p. 1571)

The exact content of FLN is not of immediate interest for the aims of this paper, but still some brief considerations can be made about Hauser et al.'s putting their money on recursion and what that means for their distinction. If recursion is the content of FLN, this means that it is uniquely human and recently evolved. The contents of FLB, on the other hand, are expected to have a long, cross-species, traceable evolutionary history. Hauser et al. (2002) offer a list of possible experimental studies which might shed some light on what FLB and FLN are and where they came from, the replication and extension of which ultimately proves their hypothesis right or wrong. This means that, in principle, it can turn out that recursion is present in other species, and thus their hypothesis is wrong. This would be satisfactory, were it not for the fact that FLN is equated with recursion virtually as part of the hypothesis. FLN meaning recursion is in principle something that can be studied under a comparative method, assuming that recursion is a well understood and stable notion (cf. Fitch, 2010b); one must look at other species and look for recursion, experimentally proving or disproving it as what constitutes FLN. However, FLN as the set of what enters into the faculty of language and is specific to humans and to language, independently of what constitutes it—which is how Hauser et al.'s rhetoric goes—, leaves no room for comparative inquiry and is not conceptually sound. It seems that FLN makes sense only insofar as it is defined after its content is determined. FLB does not present such a problem, since its definition does not clash with the comparative method like FLN's does. FLB as the set of what enters into the Faculty of Language which is neither specific to language or to humans, unlike FLN, readily opens way for inquiry in other species and domains. It seems then that, conceptually, it would make more sense to assume that recursion is part of FLB (being something that enters into language), and look for it in other species like the FLB component it is. Under a biological perspective, with no assumptions about what components enter into each of the senses of FL, it could be said that FLN and FLB only make sense as labels that pertain to the organization of the components of language, with no reference to their exact content.

In one of their later articles, the same authors (Fitch et al., 2005) make similar claims:

> It seems likely that some subset of the mechanisms of FLB is both unique to humans, and to language itself. We dub[bed] this

> subset of mechanisms the faculty of language in the narrow sense (FLN).
>
> (p. 1571)

Witness, however, the following passage from Fitch (2010a, 384):

> What all of these examples make clear is that the distinction between general and linguistically specialized mechanisms is hard to draw, even in those cases where the mechanisms seem fairly clearly defined. Most areas of language are not, and will not soon be, so clearly defined, and thus the distinction itself is of little use in furthering our understanding of the mechanisms.

Fitch was one of the authors of Hauser et al. (2002), and his updated take on the issue presents us with two possible scenarios: either he is contradicting himself, or he has abandoned the distinction, which goes to show that the main motivation for its being proposed in the first place has proved unproductive with time.

Fitch et al. (2005, 182) had already hinted at this possibility in a passage whose importance has not been given the attention it deserves:

> Something about the faculty of language must be unique in order to explain the differences between humans and the other animals—if only the particular combination of mechanisms in FLB.'

Actually, Chomsky himself had voiced this idea before the Hauser et al. (2002) paper, the most recent instance of it being perhaps the following passage:

> Now a question that could be asked is whether whatever is innate about language is specific to the language faculty or whether it is just some combination of the other aspects of the mind. That is an empirical question and there is no reason to be dogmatic about it; you look and you see. What we seem to find is that it is specific.
>
> (Chomsky, 2000)

These rare nods in the biolinguistic literature seem to confirm the conclusion of Bloomfield et al. (2011), who say that "perhaps this is a good time to reconsider whether attempting to distinguish between qualitative and quantitative differences is helpful if the quantitative advantage is vast" (p. 948). Here the authors allude to a point that Lenneberg (1967) had already made: perhaps we should recognize that general mechanisms could be at the heart of something highly specific in terms of behavior. This doesn't seem to be a point that Hauser, Chomsky and (at least initially) Fitch appreciated. The debate between the authors and Ray Jackendoff and Steven Pinker shows as well that the distinction isn't particularly productive. Jackendoff & Pinker (2005) were right to point out that "the narrow/broad dichotomy [...] makes space only for completely novel capacities and for capacities taken intact from nonlinguistic and nonhuman capacities, omitting capacities that may have been substantially modified in the course of human evolution" (p.224). More generally, we believe Jackendoff & Pinker (2005) are right in demurring "from some of [Hauser, Chomsky and Fitch's] dichotomies, which prejudge the issues by making some hypotheses—in our view the most plausible ones—impossible to state" (p.224). These are the hypotheses that Lenneberg, incidently, would have favored. For Hauser et al., it is crucial that FLN be a subset of FLB structures, and not a superset. In other words, Hauser et al. can't assume Lenneberg's suggestion that when there are various general mechanisms becoming integrated with one another, a novel structure can actually arise, simply as the result of interactions of shared mechanisms. Their hypothesis is simply not possible to state in light of the FLN/FLB distinction, as it would require FLN to a be a superset, rather than a subset, of FLB. Thus, we conclude, much like we think Fitch has, although it is not clear that the (bio)linguistic community has followed, that the FLN/FLB distinction isn't particularly productive.

When Chomsky (2000) acknowledges that we must "look and see", perhaps he is being too simplistic, but certainly he has been consistent in that we seem to find that whatever is innate about language is specific, that is, that it is *sui generis*. It is indeed an empirical question, but biology strongly suggests that the only way to get novelty is through combinations of other aspects of the traits in question. In this case, the mental organ we call language. Here we find an important aspect of current biolinguistic thought, one that's been preserved for 40 years: we seem to want something highly specific right from the beginning, for example in the genes, but that's just not how novelty arises. In the words of West-Eberhard (2005), "phenotypic

novelty is largely reorganizational rather than a product of, say, innovative genes." This seems to be a lesson that linguists haven't assimilated from biology. So even though we find a lot of references to the Evo-Devo literature, one of the central issues, namely novelty, has yet to be assimilated. This is one aspect of how biologically illiterate linguists have been for 40 or 50 years. If language researchers are serious about having a biological object of inquiry, they should pay more than lip service to biology.

3 Rethinking the I-Language/E-Language distinction

More foundational than the FLN/FLB dihotomy, the distinction between Internal language (I-language) and External language (E-language) is probably the most famous in modern linguistics. It was explicitly introduced by Chomsky (1986), and since then it has become a rule of thumb of generativism. I-language refers to the speaker-internal linguistic knowledge that reflects competence in a given language, while E-language refers to the socio-cultural construct, that is, one of the possible materializations of an I-language. Chomskyan linguistics is said to be focused exclusively on I-language; this has been useful in the sense that it ignores the messy concept of development that arises once one goes beyond the genes, and it fits nicely with Chomsky's (1955/1975) idea that acquisition is instantaneous. The problem is that not only linguists hold a naive version of genetics and biology in general, but biology is much more than genes. The fact that linguists have equated I-language with genetic endowment on the one hand, and E-language with all things non-genetic on the other had the goal of circumscribing their object of study, but it has only resulted in a divide of something that cannot be divided: both genes and development are crucial for the characterization of language, and the interaction of all kinds of factors is what creates the phenotypes that linguists attribute to genes (and to make matters worse, they do so in a simplistic and implausible fashion). There have been some recent attempts at reconciling internalism and externalism, suggesting that the two are mutually reinforced (Lassiter, 2008; Mondal, 2012), but the I/E distinction is undoubtedly a sharp one.

Contrary to what one observes in linguistics, in biology the link between the genetic makeup of an organism and the environmental factors that affect

its development is exploited through the study of its phenotype. This should be true also in the case of language. In the words of Lewontin (2000, 28, emphasis added), "human beings can speak because they have the right genes *and* the right environment." This is the message of the Extended Synthesis: genes determine the capacities of organisms, yet the limits of these capacities may never be explored, depending on how adequate the environment factor eventually proves to be. An important principle in biology is Reciprocal Causation (Mayr, 1961), by which an action is simultaneously cause and effect. In the case of language, reciprocal causation refers to the area of intersection behind the terms I-Language and E-language which reflects the point where the development of biological traits (I-properties) is affected by environmental, external triggers (E-factors). This kind of interaction ties in with the evolutionary process of niche construction, whereby organisms partly determine the selective pressures they undergo and, in a certain sense, build their own environment (Oyama et al., 2001; Robert, 2004).

Linguistic data are insufficient to account for or facilitate a clear-cut distinction between I-language and E-language which linguistis base their theories on. This insufficiency is due to the fact that both I-language and E-language are brought *together* behind the data.

In the case of language, a biolinguistic interpretation of some characteristics of instances of recent language emergence can be used to illustrate the complex dynamics between internal and external factors. There are indeed cases of languages which are still at an early stage of development, and as such have not undergone all the developmental stages which result in the end product one would expect. One such case is Al-Sayyid Bedouin Sign language (ABSL), a language that emerged in the last 70-75 years in an isolated community in Isreal, now in its third generation of signers. The presence of a gene for non-syndromic, genetically recessive, profound prelingual neurosensory deafness (Scott et al., 1995), along with consaguineous marriage patterns have resulted in the birth of a relatively large population of deaf individuals in a short time frame (Sandler et al., 2011). Fieldwork on ABSL suggests that even properties traditionally treated as design characteristics of language (cf. Hockett, 1960) emerge as a response to environmental, externalization-related factors. These *ab initio* absent properties include:

- Signifier-signified consistency (the sound/sign and meaning pairings are synchronically stable within E-languages, disallowing inter- or intraspecific variation in general): Studies on ABSL and other languages show

that there is an absence in signifier-signified consistency. Meir et al. (2010) for ABSL, Senghas (1997) for Nicaraguan Sign Language and Washabaugh (1986) for Providence Island Sign Language all give similar reports on how consistency improves over new generations of speakers, reflecting environment needs.

- Grammaticalization (process whereby lexical items lose some of their phonological substance and/or semantic specificity and instead develop finer morpho-syntactic functions): ABSL first-generation signers have the tendency to break an event that requires two arguments into two clauses, each with its own verb sign, which predicates a different argument. Languages take time to develop fine-grained grammatical markers such as the ones that facilitate distinguishing between the subject and the object phrases in a clause.

- Complexity: grammaticalization is one of the ways of enhancing grammatical complexity in language. Studies of ABSL report a gradual emergence of complexity in prosodic and syntactic structures (Sandler et al., 2005, 2011). The differences observed with respect to the time it takes for more fine-grained grammatical markers to develop in different communities suggest that the time factors should be viewed not just on their own, but as part of a cluster of factors which trigger adaptation depending on environmental needs.

If we take environmental factors into account, there are some lessons to derive. If it is correct that grammatical markers are of an emergent nature, this amounts to emergent 'parametric' variation, in the Chomskyan sense. If the goal is to reduce the role of genetic endowment (Chomsky, 2007), analogies have to be drawn with the right kind Evo-Devo. Instead of a geno-centric perspective that seems to dominate linguistics, attention should be paid to more permissive and wide-ranging frameworks, such as Developmental System Theory (DST) (Oyama, 1985; Griffiths & Gray, 1994). In the words of Benítez-Burraco & Longa (2010, 318):

> Development does not entail any kind of pre-existing genetic program; genes are not the source of the form. Quite the opposite: genes are just one of many developmental sources. Therefore, DST rejects the idea that genes are endowed with any special

> directive power. The main notion of DST is that of 'developmental system', which is to be understood as the overall collection of heterogenous influences on development.

It's becoming apparent within Evo-Devo that it is not possible to distinguish relevantly between the influence of the genes and the influence of the environment in development, since the end product is the result of the interaction of the information from both levels. In light of Evo-Devo, few dychotomies (e.g. I-Language/E-Language, Nature/Nurture, FLN/FLB, gradualism/saltationism and even adaptation/exaptation) make perfect sense.

4 Biological insights

In this section we shall discuss some of the ways in which one can have a fresh look at language, by learning from and applying some insights from modern evolutionary biology. In doing so, one expects to do away with conceptions of biology and language itself of the kind discussed above that have plagued linguistics and prevented a real, biologically informed study of language. We will take a well-known example of an attempt to bioligize language that presents some of these problems as the start of our discussion, and from there offer examples of work that has taken an extra step and incorporated important lessons from evolutionary biology and allied disciplines.

Anderson & Lightfoot (2000) have a conception of environment as only "linguistic environment", that is, experience from which generalizations about a particular language are extracted, while the biological part of the equation is left to the genes, whose job will be to somehow encode language.

> [...] language emerges through an interaction between our genetic inheritance and the linguistic environment to which we happen to be exposed. English-speaking children learn from their environment that the verb *is* may be pronounced [iz] or [z], and native principles prevent the reduced form from ocurring in the wrong place. Children learn from their environment that *he*, *his*, etc. are pronouns, while native principles entail where pronouns may not refer to a preceding noun. The interaction of the environmental information and the native principles accounts for how the relevant properties emerge in an English-speaking child. (p. 6).

From this passage one also gathers a secondary but related claim, namely that genes are the sole responsible for all that is innate. Thus, we have two problems that characterize the treatment of biology in linguistics: that nativism and geneticism are one and the same, and that genes encode the principles that constrain final linguistic structures.

If, however, we look at the modern biological literature—which we ought to—we will find quite a different picture, one where genes are but one part of the evolution of traits and organisms.

There is no clear bridge between genotypes and phenotypes. Here the notion of phenotypic plasticity is key, whereby organisms adapt to changes in the environment (here understood in a very broad sense), in such a way that their behavior, morphology and physiology effectively change as well (Price et al., 2003; West-Eberhard, 2003). This is a very important factor for the successful adaptation of organisms to variations in the environment, and it's modernly understood as including any type of change induced by it (Kelly et al. 2012). During the heyday of the Modern Synthesis in biology, the study of phenotypic plasticity was seen as more of an obstacle than anything else, which is not something very suprising at a time when genes seemed to have all the answers, and any hint of influence of the environment was considered a problmem (Falconer, 1952). It seems that linguists have kept to this conception of the genes/environment interplay, but now we know better and should abandon it. As documented by Pigliucci (2001), interest in the virtues of phenotypic plasticity has increased greatly, and it seems that it is getting progressively more difficult to look at evolution without taking the environment and its relation with the genotype into account. Taking as examples virtually any species, from plants to mammals, the consensus seems to be that phenotypic plasticity can be seen as both something that can itself evolve and something that can guide evolution. This guidance may even take precedence over genetic change (West-Eberhard, 2003, 2005; Lenski et al., 2006)

Moreover, even assuming this, genes themselves cannot serve as a diagnostic for what kind of lingusitic phenotype a subject ends up with. For example, mutated versions of related genes can result in different disorders or lack thereof in different populations (Slate, 2011; Benítez-Burraco, 2012).

Another way in which linguists often rely on well-established yet rarely scrutinized ideas (in the linguistic literature) is in their treatment of the brain, namely so-called language areas. Witness Anderson & Lightfoot's (2000) claim that "[...] even if it were to become clear that there is no

clear segregation between language-related and non-language-related brain tissue, it would still be useful and important to treat the language capacity as a discrete and specifiable human biological system in functional if not anatomical terms [...]" (p. 19). We find this quite untenable, for there is no reason to believe that language is *located* or is processed in dedicated areas of the brain. As Poeppel (2008) has argued, there is no logical entailment between the localization of any one brain area and explanation of its function. Lenneberg (1967) already had some intuitions of this sort: "as biologists, we cannot discern meaning or purpose of specific anatomical developments" (p.33). Moreover, the mapping of the brain is based on cognitive tasks which are understood on the basis of units that are not always compatible with what is known about the kind of units the brain operates, and this is very much apparent in the case of speech and language, where the cognitive units proposed are of a circumscribed theoretical nature and have no bearing on the kind of computations that, as far as research has shown, the brain performs. In this regard it is also important to take into account the fact that idealized, well-defined brain areas do not reflect reality; instead, brain areas vary across the population, and even within the same subject as growth unfolds (Prat & Just, 2011). This is an instance of Poeppel & Embick's (2005) Granularity Mismatch Problem, a problem which until resolved will render any map of the brain only partially informative (Poeppel, 2012). Thus it seems that any attempt at making progress in relation to Lenneberg's time must include the decomposing of cognitive functions, such as language, into much wider-ranging principles, which can be generalized across cognitive domains (and species). Cross-modularity seems to be what best describes language in the brain, as a result of interacting, non-specific brain structures (Griffiths, 2007).

A further lesson one can take from biology is that, much like brain areas, there is no real indication that the faculty of language, the linguistic phenotype, is uniform. Even though the notion of a fixed, shared faculty of language has allowed linguistics to surpass some conceptual barriers and isolate their object of study, we are now in a position to assess this claim and qualify it. The fact that different modalities may co-exist in the same subject (Emmorey & McCullogh, 2009), and that the timing of the steps that lead to the acquisition of language is not universal (Bates et al., 1988; Dehaene et al., 1997) show that the faculty does not unfold in a deterministic manner, unscathed by several other factors. The conceptual opposite also seems to be true: comparable phenotypes can have arise out of different brain structure (Karmiloff-Smith, 2010), which plausibly means that, the same way a specific

brain architecture cannot be a diagnostic for a specific linguistic phenotype, a specific linguistic phenotype is also not a diagnostic for a specific brain architecture.

This largely cross-modular, reorganizational notion of the linguistic phenotype has recently been explored by Balari & Lorenzo (2013), who, inspired by Alberch's (1989) notion of phenotypic space, describe what they call the computational phenotype. According to these authors, a pre-existing, non-specific computational device was recruited for language, which was then associated with a lexicon (a dictionary of symbolic units) and some means of externalization, all of which are plausibly traceable in evolutionary history. The latter—the externalization component—of language is a specially fruitful object of research these days (see, for example, Fitch 2010a). This reorganization would be a consequence of the increase in brain size of humans.

With a small adaptation, the general idea in Balari & Lorenzo (2013) is in line with Boeckx & Benítez-Burraco (2014), who put forward the hypothesis that not necessarily brain size, but rather the human, globular brain-case allowed for an expanded neuronal workspace, opening way for improved (more so than new) connections, namely those that plausibly take an existing computation device and project it across other modules. Both hypothesis challenge the notion of the language faculty as a case of novelty, which Hauser et al. (2002) defended with their notion of FLN, already discussed above.

5 Conclusion

Biolinguistics as a field is not easy to define. Different people disagree on some very foundational issues and yet they can still consider themselves to be biolinguists. Many do biolinguistics without ever having heard of it. Other people even work on what we would like to call biolinguistics, yet they would rather avoid the term. A textbook on biolinguistics—the same way we have textbooks on physics or linguistics proper—would be impossible to put together. There is no consensual body of work that could be passed along as the canon. Still, we believe we can start looking ahead in search of a biolinguistics that does away with ideas that have in the meantime proven inadequate and bring in new ideas that we feel are on the right track. Thus, what we propose is a re-hauled biolinguistics, with the same spirit of what Chomsky and Lenneberg began, but with related fields as real allies, rather than just neighbors. We believe that actually integrating insights and results

from the biological sciences broadly speaking, of the kind we have reviewed in the previous sections, will not only result in new discoveries about language, but also in the definition as a field that biolinguistics currently lacks.

For this goal to be achieved—and we believe that the change that might lead to it is already, albeit slowly, underway—we must start by building our linguistic theories on top of biology. The lack of biological constraining of linguistics we allude to in the first section is a real problem, and one that is not always recognized. Linguists often take shelter behind logical soundness alone, and find it satisfactory that theories and data fit together. This practice is fine as far as language(s) description is concerned, but if we believe that what we are after are the biological properties of the *language faculty*, then biology must have a very important regulatory role.[3] Thus, the notion of plausibility must first and foremost be understood as biological plausibility, and if this is the case typological plausibility will naturally follow.

This notion must rest on a understanding of genes. Contrary to what has been claimed, a concrete, deterministic linguistic genotype, which for some is the meaning of Universal Grammar (e.g., Anderson & Lightfoot, 2000), is not plausible.

Genes alone do not define a trait or how it is used. Here the notion of phenotypic plasticity is key: the degree to which environmental choices affect the way that genes are expressed depends on the specific genotype-environment interaction in each case, for each trait. Even in fairly "linguistic" studies the idea that the environment is also a big player has been recently exploited, which goes to show the environment need not be a taboo for those concerned with the biological properties of language. For example, Lupyan & Dale (2010) put forward what they call the Linguistic Niche Hypothesis. Having conducted a statistical analysis of over 2000 languages, their results suggest that language structures are influenced by the environment just as biological organisms are shaped by ecological niches. Similarly, Wray & Grace (2007) argue that the nature of the communicative context affects the structure of language. According to their proposal, esoteric communication allows for grammatical and semantic complexity, whereas exoteric communication leads language towards rule-based regularity and semantic transparency.

More often than not, a big concern of linguists is whether a particular

[3]This is not to say that biologists have fully assimilated all the important insights from the linguistics literature and that linguists have not returned the favor; very often, one finds in the biology literature conflations of notions that linguists have learned to tell apart unequivocally since decades ago (say, communication and language).

language will display (or be analyzed in such a way that it seems to display) a property which their theory of choice does not allow or cannot account for. When that happens, the goal then becomes to offer an alternative analysis of that property or reformulate the theory so as to account for it or disprove it. Much of the literature of modern linguists has resulted from this practice, and in most cases allusions to biology do little more than perpetuate either simplistic claims that we now know are false, or the idea that we don't really know how it works, and that as such we should indeed focus on proving and disproving linguistic phenomena. This back-and-forth might seem to bear winners and losers, but we contend that the battle we should pick is a different one: the quest for the biological underpinnings of the language faculty. Linguistics—be it innocently or negligently—has not yet embraced biology, and in some ways it has even hindered real progress in the study of the biological foundations of the language faculty. Shifting away from all-or-nothing, geno-centric, reductionist approaches to biology of language, and instead embracing its multi-dimensional character, which encompasses genetic, developmental and environmental factors, will allow us to purse the core questions that started the biologuistic enterprise more than 50 years ago and that we ought to be addressing if we are to unveil what's behind the uniqueness of our species.

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