★ FORUM ★

On the Feasibility of Biolinguistics: Koster's Word-Based Challenge and Our 'Natural Computation' Alternative

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1.   Introducing the Challenge and Our Motivation

The present paper examines Jan Koster's "skeptical view on Biolinguistics and linguistic internalism" (Koster 2009: 61),[1] and concludes on a far more positive note than he does regarding the prospect of this emerging field. Examining Koster's challenge also gives us the opportunity to point that while it is important to remember, as Jackendoff (2010) stresses, that thinking about the biology of language (e.g., its evolution) 'depends on one's view of language', it is equally important to bear in mind that thinking about the biology of language also depends on one's view of biology. We think that this point is worth emphasizing at a time when both modern linguistics and biology are re-examining their foundations.

Let us begin by stating Koster's argument in a nutshell. In order for languages to be acquired and used, Koster (hereafter, K) agrees, certain uniquely human biological requirements are required. However, so K's argument goes, in as much as human biology is not 'transparent' with respect to its role in language, the idea of translating these biological underpinnings into a distinctive mental faculty (often called 'the faculty of language', FL, or 'the language organ') makes no more sense than positing distinctive faculties for human activities such as trumpet playing or bicycle riding, or (to invoke distinctively human anatomical sites) hat-wearing or glasses-supporting systems. It is K's contention that this 'Panglossian' drift of modern linguistics (we have a language faculty in order to support languages, much like we have a nose to support glasses) is the historical consequence of generative grammarians having uncritically adopted a series of conceptually problematic and empirically unwarranted compromises since Chomsky (1957), the latest outcome of which is the chimerical discipline now

[1]   For a complete presentation of his argument, see Koster (2009). Partial accounts of it are also found in a number of shorter papers from 2005 to 2010, accessible from Koster's webpage (http://odur.let.rug.nl/~koster).

known as Biolinguistics, devoted to uncovering the biological foundations of said language faculty.

We, as advocates of Biolinguistics, think that is important to meet K's challenge. After all, K has had a distinguished career as a theoretical linguist, and is deeply familiar with the issues and practice of theoretical, 'Chomskyan' linguistics. In addition, K's is a sophisticated argument constructed upon several interesting premises that are worth thinking about, and to which we now turn.

2. Language is Words, Words, and Words

K's first premise is that language is not within our brains, but within our words. This is not an in itself invalidating argument against Biolinguistics, as words could still happen to be natural, biological objects (within our brains), but it nevertheless is, according to K, a first necessary step in order to dismantle the whole biolinguistic project. Let's see why.

K argues that Biolinguistics is constructed upon the belief that the human brain contains a system specifically devoted to computing linguistic expressions, which can be abstracted away from other peripheral components of FL and which is furthermore considered the site of one of the most distinguishing features of language: namely, 'recursion' (unbounded nested-embedding) — i.e. as in Hauser *et al.* (2002). But such a belief is, according to K, nothing more than a historical residue of Chomsky's (1957) thesis on the 'autonomy of syntax', an idea that K takes to have been *de facto* rejected in Chomsky's subsequent works with the adoption of the 'lexicalist hypothesis' (Chomsky 1970), and to which we will come back.

K's own contention regarding this issue reads as follows: Linguistic recursion is not a property of an autonomous system of computation, but a property of words, because, according to the lexicalist position that K endorses, it is in the words' content that instructions are encoded as to how they are to be combined — for example, by means of a structure headed by a word of the same type.

It is obvious that in order for the resulting structures to be full-fledged, a rather powerful computational space is required. However, according to K, such a space is just a biological substrate that 'facilitates' the completion of the properties of words (such as linguistic recursion). Moreover, K takes such a space not be 'transparent' with respect to these properties, meaning that the biological substrate would remain 'ignorant' of the facilitated properties were it not for the fact that humans have historically given it such a linguistic functionality. So K's conclusion is that the human brain does not incorporate a system in charge of computing linguistic expressions, but a general-purpose computational space, on which linguistic functionality simply rides, thanks to words. Because K views words themselves as inherently cultural symbols, as opposed to natural, biological units, there cannot be any proper field of study devoted to the biology of language.

3. Decomposing and Deconstructing K's Arguments

Let us note that K's view is an updated version of an idea with a long-standing

pedigree that K traces back to Sapir (1921), but as a matter of fact it is explicitly articulated as well in Whitney (1875), where the contention is already made that language is just a matter of having (culturally) discovered how to put into a derived or secondary use natural resources inherently unrelated to it. It also was Saussure's (1916) idea, who expressed in very similar terms the relation of language to Broca's region, as well as the official position of European functionalism — as witnessed, for example, in Martinet (1960). For K, departing from this venerable tradition would require more compelling arguments than those so far adduced by biologically oriented linguists.

Historical and traditional considerations aside, the substantive part of K's argument actually splits into two different theses, the first one having to do with the centrality of words, and the second one with the non-specificity of the system subserving the computation of linguistic expressions. We will now try to show that none of these arguments is compelling enough as to support K's anti-naturalist stance on language.

3.1. Lexicalism vs. Lexicocentrism

For purposes of K's argument, we can define lexicalism as the position according to which grammars to a great extent have the forms that they do thanks to the instructions encoded in words, contained in their lexicons. As we already stated above, the reason why K believes that lexicalism is such a problematic, indeed lethal aspect for any biolinguistic project is that words are inherently cultural, not natural/biological entities: Words are "man-made, public cultural objects" (Koster 2009: 66). Accordingly, if lexicalism is assumed and the combinatorial properties of language are taken to depend on properties of words, such combinatorial properties will have to be traced back to cultural, not natural/biological attributes.

But how true is lexicalism? And how essential are words?

For K, as we saw, they are pretty much everything. As he writes on his website (see fn. 1), where he summarizes his view: "Invented words rather than syntax are at the essence of language in this view, while recursive syntax is seen as a successful extension of the properties of the cultural objects in question ('words')". In other words, for K, "[s]yntactic structures are not generated by lexicon-independent rules (like phrase structure rules or Merge) but as the spelling out of the contextual properties of lexical items ('valency')".

Other linguists, too, ascribe an essential role to words (though they do not conclude from this that Biolinguistics is a doomed enterprise). Here is a representative quote from Pinker and Jackendoff (2005):

> We now come to an aspect of language that is utterly essential to it: the word. In the minimal case, a word is an arbitrary association of a chunk of phonology and a chunk of conceptual structure, stored in speakers' long-term memory (the lexicon) […]. [W]ords have several properties that appear to be uniquely human […]. Our assessment of the situation is that words, as shared, organized linkages of phonological, conceptual, and grammatical structures, are a distinctive language-specific part of human knowledge […]. [A] good portion of people's knowledge of words (especially verbs and functional morphemes) consists of exactly the kind of information that is

> manipulated by recursive syntax, the component held to make up the
> narrow language faculty.   (Pinker & Jackendoff 2005: 213–215)

But a growing number of linguists are coming to the conclusion that words[2] are
not distinguished building blocks in syntax or morphology or semantics, and that
lexicalism, or as one of us has come to call it, 'lexicocentrism' is not the right
model for FL. Here are a few representative quotes:[3]

> In this work I have claimed that a word is a morpheme sequence that shows
> internal cohesion and has independent contribution relative to other
> morphemes. I have argued that these properties stem from the syntax:
> although each morpheme is inserted separately into syntactic structure,
> syntax may cause some groups of morphemes to show the behavior
> characteristic of words […]. If two morphemes form a distributional unit of
> this kind [one that cannot be interrupted — SB, CB & GL] every time they
> appear together, the two morphemes in question will be perceived as one
> word. This means that 'word' in the non-phonological sense is a
> distributional concept […]. Crucially, there is not one single syntactic
> configuration that underlies all complex words […]. On this approach,
> words are not necessarily syntactic constituents […]. The consequence of
> these claims is that words do not really have a place in grammar at all. From
> the point of view of grammar, 'word' is an epiphenomenon, and not a basic
> concept.   (Julien 2002: 321–322)

> A Word, as conventionally conceived, is a syntactic constituent which
> (happens) to correspond to a phonological unit of a given size (e.g., for the
> assignment of primary stress). While it is likely that there are some universal
> constraints on what syntactic constituents can correspond to such
> phonological units, beyond that, the mapping is language specific, and
> syntactic constituents of equal complexity may or may not be phonological-
> stress units. Crucially, then, Words are not syntactic primitives or atomic in
> any meaningful sense.   (Borer 2005: 1)

Marantz (2000) adds the following relevant observation:

> It's somehow intuitive to think that knowing a language involves knowing
> the words of the language. Linguists that start with this notion quickly get
> into trouble by not being clear about what a 'word' is such that a speaker
> might know it or what 'know' is such that a speaker might 'know' a word.
> Jackendoff (1997) argues that the 'lexicon' should be extended to include
> units larger than phrases. But doesn't the Wheel of Fortune corpus rather
> argue against the correlation between 'memorized' and 'special linguistic
> properties'? We know we've encountered [Any friend of yours is a friend of
> mine] just as we know we've encountered 'nationalization' (with a certain
> measurable degree of certainty). That means, in some sense, we've stored
> these items — in some way or other. But does 'storage' necessarily imply
> 'storage in a special linguistic Lexicon'? Jackendoff's observations call into
> question the notion that we don't store information about structures unless
> the structures have special linguistic properties. None of the examples [he
> provides] have special structure — none involve special connections
> between sound and meaning. Rather than arguing for an extended lexicon,
> Jackendoff is actually arguing that we should abandon the notion of a

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[2]   Or even morphemes; see Starke (2010), Boeckx (2010a).
[3]   We quote the relevant passages in full, as we don't want to give the impression of
       constructing a strawman.

'lexicon' (of items with internal structure) entirely.

> Jackendoff pulls a fast one on us. He suggests that anyone trying to keep 'fixed expressions' out of the lexicon is trying to keep them out of the language. But, since fixed expressions are made of words (phrases, phonology, etc.), they are clearly part of language. What he fails to argue successfully is that fixed expressions have the sorts of meanings that need to be negotiated by the linguistic system. Knowledge about 'any friend of yours is a friend of mine' is clearly knowledge about a linguistic object — but that linguistic object is constructed via the generative system of the language.   (Marantz 2000: 1–2)

True, as Marantz (1997: 201) points out, most contemporary theories of grammar assume a general organization in which elementary constituents are drawn from a place called the 'Lexicon' for composition in the syntax. But when linguistic practice is scrutinized, as Boeckx (2010a) has done, far less than the full array of properties traditionally ascribed to words turns out to be needed. In fact, Boeckx goes so far as to argue that no notion more than the 'edge feature', as defined by Chomsky in the following quote, is needed to reconstruct the essential properties ascribed to the faculty of language in the narrow sense:

> For a L[exical] I[tem] to be able to enter into a computation, merging with some [syntactic object], it must have some property permitting this operation. A property of an LI is called a feature, so an LI has a feature that permits it to be merged. Call this the edge-feature (EF) of the LI.
>   (Chomsky 2008: 139)

If it is indeed true that the edge property is the only lexical property needed to jump start (Narrow) Syntax, words lose the centrality they have in K's argument.

What we have pointed out just now is in fact an old observation, already made by Otero (1976). Consider the following quotes:[4]

> Given the theoretical framework Chomsky had developed in [Chomsky (1965)], it is somewhat surprising that he did not go on to draw what, from a generative perspective, appears to be a very natural, if not inescapable, conclusion, namely that morphemic representations play no role in the (syntagmatic) derivation of a sentence.

Otero goes on to formulate the 'Dual Hypothesis', according to which "a grammatical system consists of two major modules: (i) a syntagmatic grammar; (ii) a paradigmatic grammar". Otero notes that this "yields a much improved theory of generative grammar" — "a form of grammar that is conceptually simpler":

> [O]ne with fully differentiated but internally homogeneous components. The syntagmatic subsystem consists of a central component (the syntax) and two interpretive components (the phonetics and the semantics). The syntactic component consists of a recursive set of context-free phrase-structure rules and a transformational subcomponent with root transform-ations, one nonlocal transformation ('move C') and a set of local trans-formations in the sense of Emonds (to a great extent language particular), which together generate what might be called 'construction forms' (cf. LSLT

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[4]   Otero's important study remains unpublished, and the transcript of the talk hard to gain access to. For this reason, we reproduce significant portion of his argument here.

> [Chomsky 1975 — SB, CB & GL], §33.1), that is, abstract phrase markers including only syntactic category and subcategory feature specifications […] The 'construction forms' will presumably be enough to derive a 'logical form' […]; a full interpretation can only be derived after the insertion of phonological matrices of words (in the extended sense) from the paradigmatic subsystem.

Otero further notes that:

> A syntagmatic grammar is essentially universal (biologically given in essence), while a paradigmatic grammar is, to a considerable extent, a historically evolving subsystem, burdened with the weight of the past, like other cultural systems. Only a paradigmatic grammar can be fossiliferous. This brings to mind the distinction between 'core grammar' and a 'periphery' of 'borrowings, historical residues, inventions, and so on', which we can hardly expect to — and indeed would not want to — incorporate within a principled theory of UG.
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> Every paradigmatic grammar is, to a considerable extent, language particular, and to some extent fossilized, while the syntagmatic grammar can be assumed to be a fairly direct reflection of the language faculty of the mind/brain […]. No student of human language ever dreamed of a universal dictionary.

Otero concludes that at the syntagmatic level "there is only one language, as the evolutionary biologist would expect".

This, we submit, is what makes Biolinguistics possible, a point to which we will return presently when we deal with the issue of transparency. But let us first expand a bit more on the question of lexicalism with an additional observation we believe it may be relevant to complement the ones put forth so far.

To be sure, K could argue that what we have just shown is that lexicalism is a debated issue — something that linguists have been unable to agree upon — and that this doesn't necessarily invalidate the contention that if lexicalism is true, then Biolinguistics is impossible. To which we could retort that K's faith in lexicalism doesn't make it true either and that a closer look at the fate of what we could call 'radical lexicalist frameworks' casts serious doubts, if not over the 'truth' of the lexicalist hypothesis ('truth' being a rather strong word), perhaps over the general viability of the whole program, at least in the form K appears to interpret it.

It is important to note from the outset that K's main reference for lexicalism is the Government & Binding (GB) framework of Chomsky (1981) and subsequent work, a grammatical theory that always kept a substantial bulk of its grammatical principles away from the lexicon and that was often seen from other quarters (most of them based in California) as relying too much on configurational notions to account for certain grammatical phenomena. A case in point is, for example, Binding Theory (by the way, one of K's favorites; see, for example, Koster 1987: Chaps. 3 and 6), which was mostly based on the notion of c-command, a structural relation between nodes in a tree which need not both be within the domain of a head word and its dependants, and which was 'lexicalized' by, for example, Pollard & Sag (1992, 1994) in an illustrative attempt to remove such principles from the syntax and place them directly in the lexicon, in the internal structure of words.

Our reference to HPSG is not casual, as it is perhaps the grammatical framework that most clearly illustrates the point we want to make here, although similar points could be made with respect to LFG or the various versions of Categorial Grammar (CG). The point is that LFG, HPSG, CG never, ever, assumed that phrase structure could or had to be lexicalized in some way or another — witness LFG's c-structures, HPSG's ID rules (or its two types of signs: words and phrases) and CG operations of functional application and functional composition (plus type raising, which is 'syntactic' not lexical). Now, from the fact that all these frameworks have always needed something more than just words to account for the context-free backbone is, in our opinion, the demonstration that it is not enough with the combinatorial properties of words to get some structure. Structure doesn't come for free and, in fact, assuming this strikes us as a fallacy equivalent to that of claiming that genes are 'replicators', which they aren't, since no replication is possible without all the cellular machinery in charge of actually doing that; in a similar vein, words are not 'combinators', but combinable building blocks in (desperate) need of a combinatorial operation. It is perhaps impossible to lexicalize structure and this perhaps explains, for example, HPSG's steady development in the direction of becoming a version of construction grammar (Ginzburg & Sag 2000, Sag, in press), where the traditional distinction between lexicon and grammar is blurred into a continuum consisting of pure lexical constructions at one extreme of the spectrum and multiword (or 'combinatorial', to use Sag's, in press, terminology) constructions at the other end.[5]

Summarizing, then, perhaps words and groupings of words are some kind of cultural objects, but as such *they are transparent with respect to their underlying biological structures* (call them Unification, Merge, Phrase Structure Rules or whatever operation is responsible of building the structures). This takes us to the issue of transparency and domain-specificity.

3.2. *Transparency and Domain-Specificity*

As for the second part of K's argument, it reduces to the idea that the system that computes linguistic expressions is not transparent with respect to the properties of words *because* it is a system with no inherent specialization, subserving and facilitating the tasks of a wide array of domains — language among them. In K's own words: "My argument is not against innateness but against the idea that biological structures are transparent with respect to their cultural functions, including their role in language" (Koster 2009: 66).

K's argument is subtle and deserves careful examination. Its initial premise is that biological structures are not transparent with respect to their functions, indeed that biological structures are all functionally unspecific. Thus, the

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[5] This is not to be taken as a criticism of construction grammar, as, for the purposes of this paper, we would like to remain agnostic as to what is the best approach to grammatical description. We just find it symptomatic that linguists with a historical strong commitment with lexicalism are abandoning it in favor of other clearly non-lexicalist models. Thus, in addition to HPSG, we could cite the case of Ray Jackendoff, also coming closer to construction grammar (e.g., Goldberg & Jackendoff 2004) or Joan Bresnan, now favoring probabilistic approaches to grammar (e.g., Bresnan 2007).

computational system underlying language, being biological, did no evolve 'for language' (remember, words) but only acquired its linguistic functionality once language (i.e. words) was invented; from this we can only conclude that there isn't anything internal specific to language (which is external), since its current functionality was imposed from the outside, and, thus, Biolinguistics, being concerned with the internal biological structures underlying language, is impossible.

The problem here is that Koster has it backwards. It is certainly true that biological structures are never transparent with respect to their functions (cultural or otherwise), expecting the contrary would constitute a natural theological assumption that was untenable for most biologists even before Darwin (e.g., Owen 1849). Indeed, the process of acquiring one (or more) functions is a historical one, a dialectic between the formal properties of the biological structure and several environmental factors. From this, however, it doesn't follow that form and function are entirely decoupled, as K seems to suggest, but rather that functions, behaviors, etc., *are transparent with respect to the biological structures underlying them*, and that from the analysis of behaviors, cultural objects, etc., *independently of their function*, one can infer important properties of the said biological structures. In other words, we contend that from the lack of specificity and the lack of functional transparency of structure — from which K derives the thesis that the system that computes words in combinations is inherently unfamiliar to language — nothing of interest can be said, since transparency works in the other direction, from function to structure. For us then, a system of computation can be unspecific and, an the same time, an inherent component of FL — as well as the other faculties that it subserves, as actually witnessed by the fact that it is transparent to the properties manifested by words — or other symbols in different domains (say, music or arithmetic). This position, we think, deserves to be carefully explained and contrasted with K's opposing views.

K's position is that there does not exist such a thing as a language-specific system of computation, and that in the absence of such a system it makes no sense to postulate the existence of FL — i.e. a naturally evolved cognitive system in charge of linguistic tasks. The rationale underlying K's contention is that organic systems acquire their functional specializations by two different means: a) as a result of 'natural selection', in which case they can be properly deemed 'adaptations for doing X' — as in the case of the lungs and breathing; or b) as a consequence of 'intentional decision', in which case they become 'instruments for doing X' — as in the case of the lungs and playing the trumpet. According to K, language belongs to the second category, as it is the cumulative outcome of particular intentional agents having historically decided to give a secondary use to systems — including a computational system—naturally evolved for other purposes. So K's idea is that in as much as it makes sense to speak of a language-dedicated computational system, it is just as an instrumentally adapted apparatus to a non-natural function. As a consequence, no natural language-dedicated computational system can be said to exist — and, concomitantly, no such a thing as FL actually exists.

It is our impression, however, that there is a flaw in K's rationale that

compromises this chain of deductions. In a few words: the idea that every single organic system has a 'proper function' corresponds to a narrow-minded, old-fashioned, and probably wrong biology of sorts. Let's explain why.

Contrary to common wisdom, organic systems are not inherently adapted to fulfill particular functions. They naturally evolve certain structural properties that endow them with the capacity of performing some activities, while others fall completely outside of their dynamic potential. So the activity that a given structure normally or most prominently runs — the one that we are tempted to attribute it as its 'proper function' — is better to be understood as a contingent effect of that structure's connections to other organic systems and to a particular environment. What it is truly inherent to natural systems is their potential to perform a more or less open array of activities, were their organic or environmental context to change — in Reid's (2007) terminology, their 'adaptability'; see also Balari & Lorenzo (2010a, 2010b).[6] Based on this, our claim is that it makes perfect sense to speak of an organic system as inherently devoid of a specific function, while acquiring different specializations as it naturally evolves certain connections to other systems and starts to be sensitive to certain environmental inputs. For us, this is a very suitable description for the system of computation underlying linguistic brain activity, and one capable of legitimately inspiring the biological study of FL.

Curiously enough, K's reasoning is to a certain extent parallel to our own and he even makes use of a notion of 'recycling' — adopted from Dehaene (2009) — that can be seen as the cultural counterpart of the idea of 'adaptability' referred to above. K is in apparent agreement with us when he contends that "there is no such a thing as an intrinsic function of a physical structure" (Koster 2009: 69). However, while we defend that this is the case even when a structure seems to fix some practical specialization within a certain context — internal, external, or both, for K this is a state of affairs that applies only up to the point at which either natural selection 'adapts' (or 'exapts') it for a natural function — as in the case of breathing — or human invention 'recycles' it for non-natural tasks — as in the case of language.

Before closing the topic of the domain-unspecific character of the cognitive resources dedicated to language, let's observe that K's argument against Biolinguistics contrasts with another current of opinion according to which for linguistics to fulfill the project of becoming a branch of the natural sciences, a relaxation of the degree of specificity of the said resources is a crucial requisite. Otherwise, no true convergence with standard biological disciplines as neuroscience or genetics can reasonably be expected (Boeckx 2010b, Hornstein 2009). The logic underlying the idea is clear: the more specific the mechanisms put into use in a certain domain — as it is routinely assumed by most descriptive approaches in the case of language, the more difficult it becomes to connect them with their putative variants in other organisms — and, consequently, the less plausible any evolutionary explanation for their emergence. Thus, far from

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[6]   An evolutionary corollary of this idea is that highly specialized structures — 'adaptations' — are more a risk than an advantage in the long run, given both the plasticity of organisms and the instability of environments. This kind of considerations is not, however, our main focus of interest here.

putting at risk the biolinguistic enterprise, the task of decomposing previously thought language-specific mechanisms into domain-general ones is for some, including ourselves, an urgent necessity in order to frame linguistic explanations within normal biological practice.

In other words, what K takes to be a lethal path for Biolinguistics — the path that Chomsky (2007) has called "approaching Universal Grammar from below", we take to be a desideratum for a *rapprochement* between linguistics and biology. What makes these two opposite interpretations possible is the fact that what counts as biology is not fixed once and for all. Jackendoff (2010) was certainly right when he said (adapting his statement slightly) that one's view of the biology of language depends on one's view of language, but we wish to stress that it also depends on one's view of biology.[7] It is indeed important to bear in mind that biology is far from a simple field. Many are the biologists who have argued for a pluralist conception of the life sciences (note the plural!) (see, e.g., Gould 2002, Pigliucci & Mueller 2010), and even strong advocates of narrow, pan-adaptationist conceptions of biology such as the late Ernst Mayr ("the Darwin of the 20th century") recognized the need to distinguish between two kinds of science, cutting across traditional disciplines like biology, for instance. Mayr (2004: 13, 24) leans toward attaching what he calls functional/'mechanistic' biology (i.e. molecular biology) to the natural sciences, and what he calls evolutionary biology to the historical sciences, and notes that each science has its own methodology and principles.

The same distinction may be necessary in the context of the language sciences, with one part of the field devoted to more cultural aspects of language, the languages — call this part (theoretical) philology —, and another devoted to the more natural aspects of language — call this Biolinguistics.

Ironically, K himself once pointed out (Koster 2003) that Chomskyan linguistics pursued along minimalist lines was "not philology by other means" (p. 171). We think this is exactly right, and moreover we think that this vindicates Biolinguistics. What is true of largely cultural entities like languages need not be true of the language faculty: whereas few would deny that the morphosyntax or grammar of particular languages is largely determined by their lexicons, we

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7   Ironically enough, a conception of words almost identical to that of K is not seen as an obstacle, according to some authors, in order to approaching language with biologically informed lenses. It is the case, for example, of Millikan's (2005) self-styled 'biological model' on linguistic conventions, according to which words are individually created items that replicate, proliferate and eventually become fixed for their coordinative benefits — relatively to similar units — within a community of users. Thus lexical inventories — with their grammatical, semantic, and pragmatic associated rules — are historical outcomes of ordinary Darwinian processes of differential reproduction and survival of the fittest.

  Despite appearances, Millikan's and K's are not incongruous models. On the contrary, K's anti-biologicist stance on words is fully compatible with Millikan's biological model, as Millikan's is just a variant of Universal Darwinism, which means that the 'model' put forward in order to explain linguistic units is biological, but not the 'object' to which the model applies. Besides, Millikan is explicit in declaring that the objects to which her model applies have nothing to do with individual psychology and, therefore, with Biolinguistics as properly understood. So Millikan belongs to the same anti-biological quarters as K, even if contending that some laws exist that universally hold in both the biological and the cultural realms. So the question remains whether words are inherently foreign to individual psychology — and whether they justify a biological approach to language.

submit that the syntactic principles of Universal Grammar are completely independent of the cultural constructs we call words (or morphemes). Such principles continue to depend on atomic units, but these units only consist of natural instructions.

4. A Note on the Proposition that Words Are Tools and Language Is the Technology Embodied by Them

In this section we would like to briefly examine the contrast K introduces between the conception of 'language-as-an-organ' (FL) and his conception of 'language-as-a-technology' (TL).

The latter conception is not completely new, as it is vaguely suggested in McLuham (1964) and developed in Logan (2007), where the contention is explicitly made that language belongs to a series of human inventions comprising spoken language, writing, mathematics, science, computing, and the Internet. Logan's thesis is that all these practices are technical improvements connected to the human necessity of representing and transmitting knowledge, each one historically emerging at points of informational overload that made insufficient the pre-existing technologies. A shortcoming of the idea — and one of which Logan is not unaware — is that improving an existing technique is a thing very different from creating it from scratch, so the question remains of how something like a TL could be created. This is by the way a question that, in slightly different contexts, also worried Humboldt (1836) and Rousseau (1781), and to which both responded by appealing to an instinctual basis for language.

K's ingenious alternative is a different one — and one that deserves to be carefully scrutinized: language (TL) is a human creation resulting from the prior invention of words. In other terms, TL results from the impact of words on the human brain — a source of extremely powerful cognitive resources, but otherwise a linguistic blank slate. Towards the end of this section we'll return to the issue of transparency in order to argue, among other things, that it is not an easy task — if a feasible one at all — to explain how properties such as compositionality and productivity could be added to the pack of inventions associated to words. But problems with K's conjecture are more serious than that. It is K's opinion that his view can comfortably be framed within the 'extended mind' paradigm (Clark 1997, Clark & Chalmers 1998), as it purports that language-associated mental activity results from the recruitment of external inputs (words) by general purpose and linguistically opaque cognitive systems. It is not clear, however, that such an assumption is so congenial with the extended mind framework as normally envisioned by its advocates. Let's see why.

Proponents of the extended mind model are not committed to any particular cognitive architecture — and this obviously includes the idea of mind as a blank slate of sorts prior to its embedding in the world. They just defend that (some) cognitive systems incorporate elements of the environment, so they comprise both internal and external components, the role of which in the normal execution of the system's activities is seen as functionally equivalent. The question is however orthogonal to that concerning the specificity — or lack thereof — of the relevant systems. So, in principle, the 'extended' thesis is

compatible with cognitive architectures of any degree of modularity. However, there exist strong arguments — put forward in Rupert (2009) with great detail — suggesting that 'extended' systems are only operative if their internal components are highly articulated and robustly constructed modules, in the absence of which it is not to be expected any particularly useful sensitivity to external inputs. As a matter of fact, Rupert's (2009) conclusion is that the extended mind rhetoric can be dispensed with entirely without great harm to our understanding of cognition, saying instead that minds comprise an integrated set of mechanisms and capacities in the functioning of which certain environmental inputs may exert an important causal impact.

It is not particularly important for our argument whether the question is settled in favor o against preserving the 'extended' idea and its vocabulary. The substantive part of the question is that words — understood as 'man-made, public cultural objects' — seem to be of little help to cognition in the absence of an associated set of internal mechanisms and capacities that, as we have argued at length, happens to be 'transparent' with respect to the properties of these external objects — and thus deserve the name of FL.

At this point, we would like to stress what we take to be another important aspect of words, in the context of K's challenge. A particular lexical inventory is obviously a cultural phenomenon — there is no point in discussing this. However, it is far more contentious whether the units composing them belong to a same category with the symbols of other non-linguistic cultural inventories (for the ease of discussion, we will refer to the former as 'words' and to the latter as 'symbols'). In our opinion, there exist good reasons to believe that they do not (Balari *et al*. 2011, for a detailed argument). The crucial point is that the information encoded in symbols is 'opaque' in a sense in which the information encoded in words is not, in that in order to be a competent user of symbols one needs to be familiar with the contexts in which they show up and how they relate with each other in each particular context of use (Eco 1975). This kind of acquaintance is not however a requisite in order to be fully competent as a word-system user, as once one knows the information encoded in given words, she gains access to the information encoded in combinations thereof, even without previous familiarity with the contexts in which these words' use is appropriate and even if no such contexts happen to exist. These are well-known facts, of which philosophers and linguists have been aware for a long time — but of which no clear explanations have been traditionally offered.

Indeed, we think that it is one of the strengths of Biolinguistics — under the guise criticized by K — that it comes with the only reasonable explanation hitherto offered to this recalcitrant problem: Word-systems are inextricably connected to a system of computation that — returning to K's terms — is 'transparent' with respect to the properties of words — namely, words are used compositionally and productively. Such an explanation vanishes as soon as this mind internal connection is severed and words are reduced to the same condition as other non-linguistic symbols — i.e. man-made, public cultural objects. It is worth remembering that Wittgenstein, the most conspicuous and influential defender of this 'words-as-external-symbols' view (Wittgenstein 1953), assumed — coherently with the model — that linguistic meanings were not compositional.

An idea that he defended ingeniously and enthusiastically (Wittgenstein 1958), but that only seems to really work in the case of phrases used (quasi-idiomatically) in ritualistic or other highly stereotyped situations — in which words are actually reduced to the condition of symbols.

Wittgenstein's is not for sure K's strategy to deal with this problem. It is not however completely clear what his strategy is. We think that he is forced to admit that compositionality and productivity are inventions added at a certain point — if not from the very beginning — to the way words behave. But once this is admitted, and given that these are properties unfamiliar to other 'man-made, public cultural' symbols, the burden of the proof is clearly on the side of the defender of the 'words-as-external-symbols' view, as it seems extremely counter-intuitive that the said properties are imprinted via words on an system of computation inherently opaque with respect to them.

Let us conclude this section by emphasizing that nothing thus far said purports to deny the evidence that words have external counterparts (to which we can refer as 'E-words'), or even the admission that parts of the information encoded in words have external origins, meaning that FL is not transparent with respect to this particular pieces of information ('E-features'),[8] but as we have argued none of this actually undermines a natural, biological study of linguistic computations. Note only that, by assuming a position like K's according to which words/language are cultural objects with, of course, a biological basis, but one that is inaccessible/irrelevant for the study of language, we run the risk of falling into the trap of the thesis of the ontological autonomy of culture held by many anthropologists and which Dan Sperber (1996) has cogently criticized as being blatantly contradictory. Sperber's point is that ontological autonomy is untenable because it is a form of cryptodualism: if you are a materialist, your cultural ontology has to be grounded on a physical/material ontology (cultural objects must have a material basis if we are not going to accept an 'irrational' account of causality), otherwise, your cultural ontology is vacuous (see also Jablonka & Lamb 2005 for a congenial criticism). An alternative, perhaps closer to Koster's position, is to just say 'of course there's a material basis, who ever denied that?', and leaving it there. This is empty materialism (Sperber 1996: 11), a position totally incapable of justifying an ontology of cultural objects, which, once its material basis is investigated, may turn out to be false.

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[8]   As a matter of fact, this is a suitable way of making sense of Chomsky's concept of linguistic 'imperfection' (Chomsky 1995 and subsequent works), an idea thought to capture those aspects of grammars (case and agreement features being two conspicuous examples) that seem to lack any motivation from the point of view of the cognitive systems (sensory-motor and conceptual) that the computational machine of FL accesses. A reasonable conjecture, worth being empirically tested, is that these features work as external devices that stimulate the development of the computational system and ease its normal functioning (Lorenzo & Longa 2003). This idea would justify preserving to a certain extent K's instrumental view on words. It does not justify, however, the strong instrumentalist thesis (see our section 4) according to which words are the tools that create language (TL), as in any event they are tools that clearly presuppose the existence of a robust cognitive system (FL) devoted to dealing with them.

5.   Conclusion: What a Shame It Would Be to Abandon Biolinguistics so Soon!

The alternative to the various shortcomings of K's theses is, of course, to stay firmly in the idea of FL as an organ — a part of the nervous system that deserves the dedication of a special branch of Biology. Questions routinely directed to other aspects of the biological realm make also perfect sense when aimed at this particular object: What are its component parts and how they compound a coherent unit of activity? How can this activity be described in the abstract and how is it physically realized? How does it become developmentally assembled and how did it evolve this developmental pattern? Such a research program cannot be seen, however, as the denial that systems of grammatical conventions also exist, the historical creation and transformations of which also deserve a scientific branch of specialization. For us it is nonetheless clear that the existence of historical grammatical systems is only possible against the background of a natural system (FL), the study of which seems mandatory in order to understand how they emerge within speaking communities and how they are acquired and used by individuals, as well as to establish the putative role of these systems in the opposite direction — i.e. as agents with a causal impact in the early development of FL in the individual and even in the evolutionary (or co-evolutionary) process of the faculty in the species.

We can give up doing Biolinguistics. Granted. But is it really worth the price of renouncing to understand questions like these?

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