The Emptiness of the Realism-Conceptualism debate: its consequences for (bio)linguistics

In this paper we will critically analyze the foundational claims of biolinguistics, situating it in the context of the realist-conceptualist debate on the foundations of linguistics. While summarizing the proposals, we will very briefly introduce alternatives, in order to prove that logically consistent systems are attainable outside the artificial polarization the formal field has suffered, without falling into either of the apparently binary terms. In the second part of the article, we will carefully revise the main claims of conceptualism and realism and look for logical inconsistencies in their own terms in order to demystify the allegedly exclusive "debate" and generate awareness of the need of new, superating alternatives.

Keywords: *biolinguistics; realism; conceptualism; methodology; metalinguistics*

1. The Biolinguistic Enterprise

The term "biolinguistics" was coined, according to Chomsky (2005a), by Massimo Piatelli-Palmarini in 1974, under the influence of Lennenberg's recent (1967) Biological Foundations of Language. At that time, however, it was not clear whether we were in presence of a sub-branch of linguistics or a field on its own right. It was also not clear what its scope, aims and methodology were: one of the founding questions, very much alive today, was whether principles that appear unique to language are in fact shared by other cognitive domains, and if so, in which manner. This also led to ask to which extent we can find a principled explanation for emergent properties of language as a state of the speaker's mind, ultimately, a "property of matter" in Darwin's terms.

This inquiry has led to the so-called "Minimalist Program", in the methodological and substantive effort to simplify not only the theoretical apparatus (methodological minimalism) but also the extra-theoretical entities that are to be accounted for (ontological minimalism). Whether the enterprise has been successful is still to be discussed (and not just assumed), but there is a wider question in hand, which has been systematically wiped under the rug in most of the vast recent literature on the topic (see, for example, Di Sciullo & Boeckx, 2011; Di Sciullo, 2012; Chomsky, 2005a, b, 2007, 2010; Boeckx, 2010), namely: is the principled explanation of linguistic phenomena to be found in biology? The aforementioned authors, with different degrees of convincement, seem to think so. This belief, which is actually an axiom of the theory[1] implies a whole view of the natural world and its foundations (even if the scope of the works under the label "biolinguistics" are usually much narrower) which is, needless to say, not innocent. We will discuss such a view, and then present some alternative proposals which relativize the role of biology in language studies, as well as the plausibility of some biolinguistic

[1] We beg the reader to notice how the notions of "theory" and "program", which should be kept apart in hard science, are permanently overlapping in the literature and are sometimes used as synonyms. Minimalism, as it apparently has "ineliminable elements" (Epstein & Seely, 2002) such as features (thus, substantive elements), is more a theory than a program. Its purely programmatic side, the elimination of superfluous elements, is nothing new, as it is the mere application of the Occam's razor metatheoretical desideratum, which has been applied in science and philosophy for many centuries now.

claims.

1.1 Language from a biological point of view or biology from a linguistic point of view?

The title of this section reflects a growing tendency in current studies on "biolinguistics": that of forcing concepts, not methodology and thus get mixed approaches with very limited scope (see Poeppel & Embick, 2005 for a criticism of such approaches). Consider, for example, the claim that language is a part of cognition (for a recent reference, see Boeckx, 2010: xiii). Such a claim limits the horizon of linguistics to that of cognitive science, and there are only some marginal works which attempt to go beyond (assimilating linguistic structures to mathematical or physical structures, for instance). Some examples of the role of language in cognition and cognition in language (with the curious methodology of not defining any of them in unambiguous terms[2]) often come from the side of structurally or lexically ambiguous sentences, or gestalt-like figures. Such examples surely show that there are more general issues involved in language comprehension and generation, but it does not provide any evidence to restrict the scope: it may very well be the case that instead of a faculty of language interacting with external systems (as in Chomsky's 1995 et. seq. architecture) we have general underspecified (i.e., object-neutral) algorithms that activate whenever necessary, thus eliminating the need for a specific mental-biological organ "Faculty of Language". The second option has only been pursued from "alternative" / "outsider" approaches, like Survive Minimalism (Stroik & Putnam, 2013) and Simpler Syntax (Culicover & Jackendoff, 2005) within (non-mainstream) Generative Grammar. Most often than not, these approaches have been confused with the claim that there is a general learning mechanism, in a Piagetian way, presumably in the hope to make them fail in the same mistake Piaget did, and which was pointed out, among others, by Chomsky.

Perhaps the most salient characteristic of the orthodox approaches is the attempt to force concepts and patterns into the data and, from there, the architecture of the cognitive system. Notice that the reasoning is the mirror image of Carnap's (1966) model: axioms are forced into the data and there, an alleged explanation is claimed to have been found. Let us see a clear example: the notion of antisymmetry. Quite trendy since Kayne's (1994) highly influential Linear Correspondence Axiom (which maps c-command relations in 2-D tree-like X-bar theoretic representations onto phonological precedence relations), the concept has been adopted and adapted in a biolinguistic framework. Let us confront two proposals regarding a(nti)-symmetry, namely, Jenkins' (2011) and Di Sciullo's (2011), to clarify the scenario. Jenkins claims that asymmetry is to be found at many levels in the natural world (though limiting himself to biology on both extremes, we could easily extend his perspective if we accept Uriagereka's 1998, 2002, 2012 findings on mathematical structures far beyond what biology can *empirically* test), and a plausible line of inquiry is to ask whether the asymmetry effects we see actually arise from a principled asymmetry or from an independent source. This is the key concept in Jenkins' methodology: he does not stipulate asymmetry as a principle organizing the Universe (actually, if string theory is correct, it would be quite the opposite: the Universe would be ruled by supersymmetry), but he finds certain effects (like brain lateralization and neural wiring optimization, also following Cherniak's, 2009 non-genomic nativism) and proposes the following, as a plausible line of research:

"*Since many linguistic systems exhibit asymmetric properties one should always ask whether*

[2] For instance, Boeckx (2010: xiv) claims: "*It stands to reason that in selecting material I am offering a rather personal view of what cognitive science is and what cognition may be*". However, there is no *explicit* formulation of that alleged view. Therefore, his views (like those of many others, frequently including Chomsky himself), turn out to be outside the domain of falsation.

these may have arisen from the general principles of symmetry that we find operative in physics and the other sciences" (Jenkins, apud Di Sciullo & Boeckx, 2011: 126)

It is strange to see that this is precisely the line of inquiry that is not only not pursued, but rejected: universality theories of the kind pursued in physics (e.g., M-theory) are systematically rejected in linguistics. Instead of integrating (some aspects of) the generative approach to syntax (which is by no means exclusive of Chomskyan linguistics, consider, for example, Systemic Functional Linguistics' "system networks" as a cognitive, on-line generator) on a more general scientific framework including findings from neurology, physics, and cognitive science; the road taken seems to be that of ostracism. Consider, for example, the frequent leap from linguistic considerations, often involving highly theory-specific stipulations (feature valuation mechanisms, for example) to biological considerations often involving the FOXP2 gene having little support when considering linguistic structure. So, instead of a reasoning of the form "there is asymmetry in language, language is part of nature, let us see whether asymmetry can be subsumed to more general properties of natural systems, like physical systems" (Jenkins' proposal, and our own), Di Sciullo (2011) takes the inverse path: "language is part of nature, then all characteristics we find in language are to be found in biological systems". In that way, we go from uninterpretable/unvalued features [*u*-F] driving the syntactic derivation to an attempt to justify the feature valuation process (stipulated in Chomsky, 1998 as a way of getting rid of the problem of the property of displacement in natural languages) from a biological point of view. Feature valuation is at the very heart of the system, and asymmetry follows from the fact that there is a local relation between two instances of a feature, one valued and one unvalued. That pervasive system is implemented at all levels of morphosyntax. Should this approach be correct, however, the system of variation exposed by Di Sciullo would be very elegant and parsimonious, although its biological plausibility (i.e., how do we understand biologically the nature of an [*u*-V] or [*u*-D] feature? What, if any, are their neurological bases? Is there any difference that can be seen in an fMRI, for example?) would still need further clarification, not to be taken on faith. Merge, Agree, Transfer, etc. are part of the formal vocabulary of linguistics, not of biology. If biolinguistics is conceived of as the techné of shifting fields while maintaining terms, then the whole enterprise is doomed to failure on methodological grounds.

Kayne (2011) follows the same path as Di Sciullo, and they both rely on Chomsky's mysterious "third factor", which has remained undefined since Chomsky (2005b). Non-specific properties of a computational system could follow from deep biological principles, but they can also have arisen as a result of more fundamental physical constraints, or as the optimal resolution of a generation/implementation tension (as in Uriagereka's 2012 CLASH model). The working hypothesis for Kayne is that anti-symmetry is a human-specific characteristic. Let us bear in mind that the concept of anti-symmetric relation as introduced in Kayne (1994) was applied to phrase structure, more specifically, to the concept of c-command with linearization purposes. A general definition of anti-symmetry would be as follows:

1) A relation between A and B is anti-symmetric *iff* it holds for A → B but not for B → A

This formulation rules out the possibility of having "mirror" trees, and gives major importance to the "side" of the tree in which an element appears, in the line of what Di Sciullo (2011) has posited for Merge. The tree diagram is, then, more than a model, it becomes more a(n allegedly) substantively correct representation of mental content and process. This is a claim that underlies Kayne's work, and one that might undermine its biological plausibility, as it is highly unlikely that humans actually have bi-dimensional trees in their minds, mainly because of processing issues (consider first the hundred step rule that applies to linear Turing-like generators, or

Markovian processes, and then the whole amount of operations the human brain must coordinate in less than a second), but also because of mathematical principles determining possible physical instantiations: if mathematics is n-dimensional (and there are even many mathematical models for different dimensions, that is, you do not study a Calabi-Yau figure with the same tools that you use to study an Euclidean triangle), why should all physical systems be ℕ-D (where ℕ is a theory-convenient fixed natural number)? Is there a possibility that the human mind works with more than 2 dimensions (contra Kaynean tree diagrams, which in turn guarantee an unambiguous system of projection for the theory)? If so, would it imply a computational advantage? This does not mean each physical system has a dimensional system of its own, but rather than we should not assume a strong regularity thesis instead of a strong universality thesis: the first claims all systems are the same in all respects, the second, that there are underlying generation patterns which configure the deep structure of the Universe. In our opinion, the mainstream "naïve" biolinguistics (with due notable exceptions, like Jenkins, 2011; Uriagereka, 2012; Stroik & Putnam, 2013; Smolensky & Legendre, 2004; and to some extent Jackendoff, 2002) has taken the first hypothesis as valid without even considering the second alternative.

1.2 Non-mentalist biolinguistics

So far, we have seen that one of the main claims of biolinguistics is that the knowledge of a language is describable as a state of a speaker's mind, in turn "a property of matter", that is, of the neural substrate. This view relates to what is called "mentalism" or "sub-personal approaches" (in a more pragmatic tradition, see Carston, 1998): what linguists are really analyzing are mental objects, internal, individual and intensional, thus getting what has been called "i-linguistics". Everything else is, to this view (expressed at its best in Chomsky, 1986), epiphenomenal, external. "i-linguistics" thus opposes to "e-linguistics", a label that groups functional approaches as well as Saussurean structuralism (recall that Saussure defined langue as something social, as opposed to the parole, which was an individual act of will and intelligence): everything externalized is parasitic on the generative engine, and the interest of Chomskyan biolinguistics, heavily based on psychology, is to study the mind, not the externalization of the computations. The restricted equation "only i-linguistics = biolinguistics" follows quite straightforward. On this line, Mendívil Giró (2006) explicitly delimits biolinguistic approaches to internalist theories, rejecting proposals like Deacon's (1997) that language is like a "virus", existing "outside brains" and evolving in a continuous tendency towards optimality in order to be learnt. In this view, universals would not be a priori as in the Chomskyan view, but a posteriori, the result of an evolutionary process that affects language just as it affects species. Darwin (1871) already pointed out that "(…) *the formation of different languages and of distinct species, and the proofs that both have been developed through a gradual process, are curiously the same* (…)", and Deacon takes the parallel between languages and species to the next level. Deacon's perspective, even if "biologically based", is not considered "biolinguistics". Why is that so? Because he assumes that languages exist outside the minds of the speakers (an anti-psy take on language), a position that is closer to what has been called "realism" or "platonism" by Katz & Postal (1991), in contraposition to Chomsky's "conceptualism" or "cartesianism". The very foundations of the current biolinguistic enterprise are affected by this debate: is language an *external* object or an *internal* object? Does it have any existence *outside* the speakers' minds? And if so, is it worth studying these external manifestations of language? In the next section, we will analyze the realism-conceptualism opposition, in order to try to clarify this issue and see if, and to what extent, a "realist biolinguistics" is possible and desirable. To do so, we will have to review the most important claims of both realism and conceptualism, to have a complete a picture as possible and see if the

problem is fully defined by those two positions or there is logical room for, say, third or even fourth alternative/s.

2. Realism and Conceptualism in Linguistics

It has been recently argued (Postal, 2003; Katz & Postal, 1991) that the object of study of Linguistics is a "real" object (what has been called the "realist view", or "platonistic view", in Postal's terms), in contraposition to the Chomskyan (1965, 1986, 1988, 2002) allegedly more "cartesian" view of language as a purely abstract construction (see Chomsky, 1966). We will argue that neither pure conceptualism nor Katz & Postal's "naïve" realism provide an accurate account of the ontology of linguistic objects. To this end, we will revise the arguments of both sides and then provide our own take on the matter.

2.1. The Conceptualist Enterprise

Conceptualism in Linguistics can be taken as the idea that objects have only a *formal* entity, being observable counterparts only epiphenomenal. This claim is not exclusive to Chomskyan linguistics, but it is essential to other frameworks, like Construction Grammar (Goldberg, 2006), which claims (contrarily to the GB tradition) that constructions exist, like templates, in the mind of speakers, and those templates, and the constraints upon their well-formedness, are the object of language learning. Structure, in this framework, is inherently meaningful, a claim we will share but mainstream Minimalism rejects. Crucially, in the context of generative linguistics, formal (in the sense of exclusively syntactic) entails biological: language is a biological object, which has no entity apart from its mental representation (Chomsky, 1972). The object of study is then an innate "faculty", which provides the substrate for language to develop and display the main characteristics that concern Chomskyan studies: *recursion* and *displacement*. Both are taken to be properties of mental objects, ultimately part of human biology if psychology is considered a proper subset of biology, as Chomsky has explicitly done over the years (e.g., Chomsky, 1987: 6). The framework we have described is in line with Chomsky's (1965) claims on *strong* vs. *weak* generation procedures: strong generation was (and, we will show, is still) taken to be the generation of structural descriptions via phrase structure rules (PSR) of the type $X \rightarrow Y$ (/_ /C). Even though Chomsky claims that his enterprise is to account for the unconscious knowledge of an ideal speaker, it is clear that the object to be explained has been fixed beforehand:

"*We must require of such a linguistic theory that it provide for:*
(i) an enumeration of the class S1' S2', … of possible sentences
(ii) an enumeration of the class SD1, SD2, … of possible structural descriptions
(iii) an enumeration of the class G1, G2, … of possible generative grammars
(iv) specification of a function f such that $SD_{f(i,\ j)}$ *is the structural description assigned to sentence S, by grammar* G_i*, for arbitrary* $_{i,j}$
(v) specification of a function m such that m(z) is an integer associated with the grammar G, as its value (with, let us say, lower value indicated by higher number)" Chomsky (1965: 31)

This is the first formal description of a strongly constructivist system, which we have formulated in a summarized way as follows (Krivochen, 2012a, b):

2) Given a generative system Σ, and a finite set $S = \{\alpha_1 \ldots \alpha_n\}$ of well-formed formulae, Σ generates S and crucially **no** α such that $\alpha \notin S$.

Notice that S is in fact predefined in (i) and (ii), and even the possible class of generative procedures is determined a priori in (iii). Such a constructivist system, in which no "ill-formed" object is generated by Σ, be Σ phrase structure rules or Merge. Let us go deeper in this respect: what exactly is the difference between these two kinds of generative procedures and the objects they generate?

PSR are rewriting rules, of the kind X → Y. They are recursive, in the sense that it might be the case that X ∈ Y. More generally, any system in which (3) holds is recursive in this early Chomskyan sense:

3) $\forall(x), \exists(y) \mid y \in x \wedge x' \in y$

We use the prime (x') notation for different *tokens* of the same structural *type*. For example:

4) a. S → NP, VP
   b. VP → V (S')

Needless to say, the embedded S' is not the same as the matrix S, as (5) clearly shows:

5) $[_S\ [_{NP}$ The boys $[_{VP}$ claim $[_{S'}$ that $[_S\ [_{NP}$ the boys$]$ $[_{VP}$ claim $[_{S'}$ that…$]]]]]]$

If x ≡ x', then we have infinite embedding, which is computationally possible but neurologically impossible, because there must be a halting algorithm for interpretation purposes: a syntactic object featuring infinite embedding is simply not parseable due to limited memory issues. This problem of mathematical structures has been already spotted by Tegmark (2007)[3], among others, and we have proposed a solution in Krivochen (2012a) which lies precisely on interpretation: if interpretative systems have access to the generative engine, then they determine what counts as a legible unit (necessarily finite) and thus the problem of deriving a halting algorithm for mental computations (in which there is always interpretation involved) reduces to the already existent problem of formulating the input conditions of interpretative systems.

Even considering the condition in (3), the reasoning of Chomsky's early conceptualism is circular in the following sense: the formulae imply, from the very first step, the final object. Then, there is no point in developing the rewriting formula as it will not add any information to the first object, namely, S. Rules are not consequent but subjacent: rule A applies if and only if rule B applies (that is, A is subjacent to B), which implies but does not define a derivational diachrony. Notice that (4b) only applies to the output of (4a), only if (4a) applies. There is no explanatory adequacy whatsoever in PSR as the Standard Theory presented them.

Minimalism brought an *aggiornated* generative algorithm, called Merge. The operation Merge,

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[3] Tegmark (2007: 19) graphs the interrelations between Formal Systems, Mathematical Structures and Computations, together with the potential problems for each of those aspects of the unified theory of the Multiverse. With respect to Computations (defined as special cases of mathematical structures which produce theorems of formal systems), the problem Tegmark finds is that there might be no halting algorithm built-in the system. In Krivochen (2012a) we made two proposals:

a) Not all computations need halting: only those that have to be interpreted, because of memory limitations.
b) In an architecture with invasive interfaces, there is no need to formulate independent halting algorithms, apart from a general definition of legibility conditions:

$\forall(x)$, Transfer(x) applies *iff*:
$\exists(IL) \mid x \subset IL$ (in the case of language, IL = Phon / Sem)
$\nexists(p) \mid p \in x \wedge p \notin IL$

as defined in Chomsky (1995) and Kitahara (1997) is precisely based in this derivational diachrony, as Epstein et. al. (1998: 18) point out:

"*It looks as if a category enters into C-command relations with other terms only* ***when*** *it is merged […] X C-commands all and only the terms of the category Y with which X was paired/concatenated by Merge or Move in the course of the derivation*." (highlighted in the original).

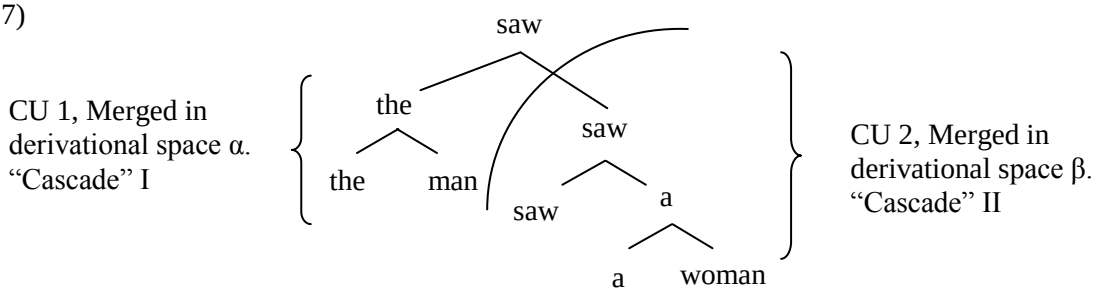
C-command is the fundamental relation in phrase structure with relation to hierarchy, enough proof of this should be Binding Theory and its extensions (the ECP, for example, considering traces are categorized as either anaphors, pronouns or R-expressions). The application of an operation that takes A and B and forms K ⊃ (A, B) is sequential, bottom-up in traditional accounts and manipulates only one element at the time. Kitahara (1997: 5) presents Merge in a way that, to our knowledge, combines both PSR and the *diachrony* of Merge:

6) Input: α, β
Concatenate α and β, forming K
Output: K

This can be seen as an "inverse" PSR: instead of developing a structure, Kitahara's Merge version makes it analytical, so that K is logically equivalent to (α, β) for the purposes of further computations. In any case, the label K (which is also the structural description of a term) is projected because a further instance of Merge-to-the-root will occur, therefore anticipating future operations.

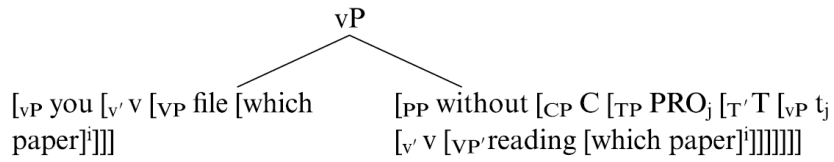
The question to be asked now is: is there any difference as to the ontology of the resultant objects? We will argue there is not. Both Merge and PSR generate (in different senses) symbolic abstract objects, describable in set-theoretical terms. Both formulations, also, neglect the physical dimension of the operation and the resulting objects: which physical configuration of matter licenses the properties that are ascribed to the assumed computational system? This is not trivial, since, for example, the binary-branching character of syntactic trees, adjudicated by Kayne (1984) to unambiguous paths requirements for connectedness and dependencies in Binding Theory (BT), are the result of certain assumptions regarding the mechanical basis of the system, basically a Turing machine (an assumption shared by Katz & Postal's "realism"). Such a computational system works in a bi-directional way within a 2-D dimensional space, which is already a limitation: the possibility (outlined in Penrose, 1997; Stapp, 2009; Vitiello, 2001, among others) that the mind is in fact a Quantum Computer, deriving objects in parallel *n*-dimensional workspaces (a very plausible claim, given recent developments in neuroscience related to the multiplicity of tasks carried out by the human mind-brain, and the possibility of maintaining two simultaneous representations corresponding to a single object, as in the case of ambiguous words) tears such limitations apart. In the theoretical realm, we would need a particular stipulation to determine that generation is constrained by the 2-D possibilities of a Turing-like linear algorithm. Let us see some arguments raised in favor of parallel computations and multidimensional spaces. Uriagereka (1999/2002) has already argued against a system with only monotonic Merge, that is, linear applications of Merge to the root tree: he formalizes the possibility of merging two "command units" CU (i.e., Markovian structures independently formed via monotonic Merge, see Uriagereka, 2012) as in the case of complex specifiers. For instance, in the derivation of [$_{saw}$ the man saw a woman], [$_{the}$ the man] and [$_{saw}$ saw [$_{a}$ a woman]] are generated separately and then assembled externally by substitution. The derivation would proceed as follows:

7)



If the derivation occurred in a single derivational workspace, then the units [the] and [man] would be merged to [saw a woman] sequentially (i.e., monotonically) and not as a single constituent [the man]. That is, if we assume that [saw] would project a label, the final phrase marker would be the semantically ill-formed (Markovian, monotonic) sequence: *[$_{saw}$ the [$_{saw}$ man [$_{saw}$ saw [$_{a}$ a woman]]]]: the constituent structure is not accurately represented, which is unproblematic for a blind generator (e.g., Chomsky, 1995) but not for the interpretative semantic interface, as the determiner, which restricts the reference of the nominal root, does not constitute a unit for the purposes of further computations. This is, we believe, a strong argument against uniform Turing-like generators, which do not accept non-monotonic processes (for instance) to be managed differently from monotonic objects. The derivation of parasitic gaps proposed by Nunes (2004), along the lines of Uriagereka (1999/2002), also support our perspective: the derivational point (7) shows that the two immediate components of the *v*P must have been merged in parallel (Nunes' (27), p. 100):

8)



At this respect, Nunes says that:

"*As the derivation proceeds, other lexical items are pulled out from [a Numeration] N' in (24a) and merge with K and M [terms formed by Merge from N and N'] in (25), yielding the objects P and Q in (26).*
(26) a. P = [PP without [CP C [TP PROj [T' T [vP tj [v' v [VP reading [which paper]i]]]]]]]
b. Q = [vP you [v' v [VP file [which paper]i]]]
(27) [our (8)] represents the next derivational step, where P adjoins to Q […]. In (27), no chain formation between the nondistinct copies of which paper can take place, since they are not in a c-command relation (...)" (2004: 99)

Even if we accepted that the derivation of P and Q in Nunes' quote may have its expression in a linear Turing machine with a serial rewriting algorithm, we need at least two such machines to derive (7) and likewise sentences. Chomskyan minimalism's generator function has to be revisited under the light of these kinds of structures, as well as the findings of multidominance theories of movement (and dependencies in general, see Vicente, 2009). This gap between the formal properties of the generator and its alleged psychological reality is precisely one of Katz & Postal's (1991) arguments against "conceptualism". It is true that the implementation of the Chomskyan machinery within a biological framework has not lead to the results the

"triunfalistic rhetoric" (in Newmayer's terms) of some theoreticians would lead us to expect, particularly if we are to pay attention to methodological aspects: it is indeed hard to attribute *psychological* (let alone, *biological*) reality to, for example, features and their respective values (see Di Sciullo, 2011 for such an attempt and Kosta & Krivochen, 2012 for a comment on the outcomes of the enterprise), which have been considered actual explanatory devices within Chomskyan minimalism. Perhaps the greatest flaw of so-called "conceptualism", particularly in its "biolinguistic" version, is precisely this: *to attribute by force biological entity to formal tools*, to try to establish a univoque relation between what are, in our opinion, really *three* different sets of entities:

a. Objects of the phenomenological world, independent of a perceiving mind
b. Propositions about those objects, presupposing a (modal) subject
c. Propositions about propositions about objects, also presupposing a (modal) subject

The first set contains *Bedeutung*[4], which we will symbolize with square brackets [ ]. The second comprises their corresponding *Sinn*, which we will symbolize with inverted commas " ". The third set contains Sinn which take the objects of the second set as Bedeutung (which we will symbolize with angled brackets < >). We follow Frege's (1892) proposal, then developed by Russell (1905) –among many others-, that a proposition can appear in either primary or secondary occurrence. Let us, then, take an example:

9) [What did you think John bought?] (P for short)
"P is an interrogative sentence"
<"P is an interrogative sentence" is a descriptive proposition>

Notice that we have added no object, crucially. Only predicates, but no arguments in either of the levels. That is:

10) Descriptive(Interrogative(P))

To the third of our sets belong logical/philosophical notions like analytic, synthetic, tautology, among others. Notice that those notions apply to propositions, that is, to Sinn, but not to mind-external objects, that is, Bedeutung. An object of the phenomenological world is not analytical or synthetic, neither true nor false. The same holds for electrons, rocks and linguistic expressions: all three exist as part of set (a).

Which is the place of linguistic theory in this mess? Let us take a classic claim from Chomsky (1987: 6):

"*I will assume that one of the 'mental organs' of the human mind-brain is a language faculty which allows various possible specific realizations, the specific human languages*."

Chomsky & Lasnik (1995: 14) elaborate on this:

"*We are concerned, then, with states of the language faculty, which we understand to be some array of cognitive traits and capacities, a particular component of the human mind-brain. The language faculty has an initial state, genetically determined; in the normal course of*

[4] We use Sinn and Bedeutung in their Fregean sense. We maintain the original terms to avoid confusion and misinterpretations.

development is passes through a series of states in early childhood, reaching a relatively stable steady state that undergoes little subsequent change, apart from the lexicon."

These claims, and many others that have been made since *Aspects* to this day, belong, we think, to set (b). Moreover, the corresponding Bedeutung is simply stated, presupposed even if no theory-independent evidence is presented in its behalf: a so-called *Faculty of Language* (FL)[5]. We could work with FL belonging to set (b), as a purely methodological abstraction, but Chomsky's claims (to take the most representative) seem to have a different aim: there is a theory of this initial state of FL, called Universal Grammar (UG). So, if there is a theory of FL, then either FL belongs to set (a) and UG to set (b) or FL to set (b) and UG to set (c). If the latter case is correct, we have pure Sinn, but no ultimate Bedeutung: a situation that has already been largely studied with the sentence "The present King of France is bald" and related sentences. Can we have a theory of something that is in itself a theory? Yes, we can (e.g., meta-mathematics), but our understanding of the ultimate object will not grow much if the object itself is not addressed at some point. Now, let us assume FL belongs to set (a). How could we prove it? The answer that comes to mind is that we should be able to find independent arguments for FL that do not belong to sets (b) or (c) and, moreover, that cannot be accounted for with a "non-existence" hypothesis (that is, holding the null hypothesis that there is no FL). To our knowledge, two different sets of arguments have been raised in favor of UG, let us briefly examine some examples of those arguments in turn and see if they pass our test.

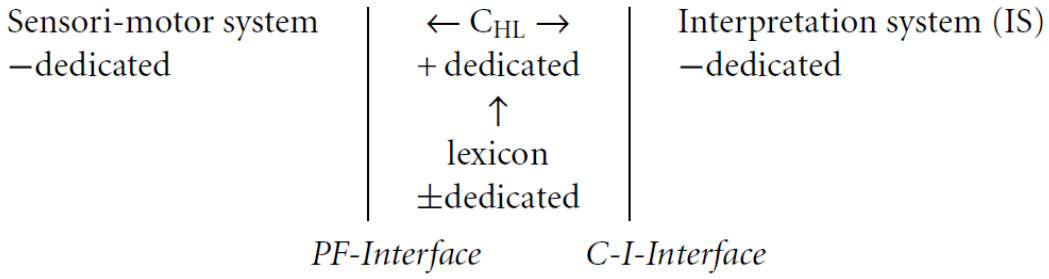
Argument 1: Reuland (2011), Hauser, Chomsky & Fitch (2002)[6]

Reuland (2011: 202), from the core of generative linguistics (attributes the recent interest on language evolution on the advent of the Minimalist Program, and its assumptions. He argues that there are two defining characteristics of UG: unboundness and "desymbolization" (i.e., dissociation of form and function). The reasoning is as follows: if there is UG, there is unboundness and desymbolization. Language displays both characteristics, therefore, there is UG (and, therefore, FL). The logical structure is fallacious insofar as it affirms the consequent in a silogistic structure. It may very well be the case that language, whose nature and ontology is left mysterious in this account, displays the aforementioned characteristics as a result of recombination of non-specific components, what Reuland calls "- dedicated" components. In his account, Merge (that is, C(HL)) is "+ dedicated", as it is language-specific. And, it is language-specific because it is "+ dedicated". Nothing informative here, unless we can prove that there is no element outside language that displays characteristics that could only have been generated via Merge. Reuland's schema is as follows (2009: 206):

[5] The fact that frameworks like Goldberg's Construction Grammar (see 2006: 4-5), and HPSG work without assuming UG should be revealing at this respect. This problem is not polarized either (that is, it is not necessarily a matter of "FL or not FL"), but most likely, *when* we need to resort to a domain-specific workspace. If *all the time*, we have a Chomskyan FL, assuming UG and so on. If only when deriving an object in real time, then alternative models arise. Or, it can be claimed that there is no FL at all, and all processes are shared between all domains. No option should be *a priori* dismissed.

[6] In Krivochen (2012b) and Krivochen & X (2013) we have argued against the existence of a fixed FL on computational grounds following the criteria exposed in Laka (2009), affecting Merge and the form-function binarism. The following argumentation will focus on the logical structure of the argument, while assuming previous discussion.

11)



Unboundedness is guaranteed, in his model, by the fact that $C_{(HL)}$ is marked as + dedicated, comprising *Merge* and *Agree* (a claim shared by Hauser, Chomsky & Fitch, 2002 for Merge). Let us consider each in turn. It has been already suggested (see, for example, Uriagereka, 1998; Jackendoff, 2002) that hierarchy is relevant in the organization of the sensorial information, for example, when distinguishing a figure from a ground in the visual apprehension of the world. Hierarchy in language is created by Merge; assuming that hierarchy in other systems is created by some other operation would be multiplying theoretical elements beyond necessity: the so-called "essential properties" of Merge, combination of two elements and labeling of the resulting structure for the purposes of further computations (Reuland, 2009: 206-207) are actually epiphenomenal. It would take a special stipulation to restrict the power of the generative algorithm to binary structures (stipulations derived from the LCA or Binding Theory, in most cases), should we attempt to impoverish FL to get only the essential, binarity would certainly not be in this set. Two different answers have been proposed: either syntactic structure is not necessarily binary (see, for example, Culicover & Jackendoff's, 2005 *flat structure*) or it is binary, but as a result of interface requirements, particularly semantic conditions on interpretation in a derivational system driven by the need to combine conceptual information (roots) and instructions as to how to manipulate that information, particularly restricting reference (procedural nodes, in Relevance Theoretic terms, Sperber & Wilson, 1995; Krivochen, 2012b). Even if both these alternatives were proven wrong, the mere possibility that alternative formal systems can be built without the *axiom* of binarity, either because *n*-ary structures are allowed (*flat structure*) or because it is not an *axiom* but a *theorem*, generating no inconsistency is itself an argument against its essential character. The proposal of Radical Minimalism (also shared with other approaches like Jackendoff's Conceptual Structures, Survive Minimalism's architecture, and Uriagereka's CLASH model) is that Merge can be better expressed as an *n*-ary *concatenation* function applying in an *n*-dimensional workspace, all constraints (including binarity when existent, see Krivochen, 2011a, 2012a, b) being determined by interface conditions over interpretation. Thus, conceptual structures (of the kind Moss et. al., 2007; Taylor, et. al. 2011 describe) are hypothesized to be built via Merge applying to concepts in (say) a 3-D mental workspace, which represents the phenomenological world that is to be "understood" and somehow projected in the mind, taking Jackendoff's terminology. Phonological structure, in turn, is possibly Markovian (see Isardi & Raimy, in press), the elements manipulated being discrete, something particularly noticeable in music (conceptual structure being arguably absent), where we have a finite set of elements with which we can generate infinite structures. Variation also arises, like in language, not because of the generator function but because of the characteristics of the "lexicon": in this case, the occidental system based on 100 cents difference between two immediately adjacent notes of a chromatic scale and the oriental system, with a threshold of 50 cents. It is not absurd to visualize that the

possibilities of combination that arise in one system and the other are comparable to, say, the possibility of incorporating manner into motion as in Germanic or not, as in Romance. The discussion itself licenses a whole book, but we consider the arguments presented here at least serve as a strong suggestion that Merge (or, in more general terms, a combinatory engine) is not language exclusive. That leaves us just with Agree, which is in turn parasitic on the notion of feature valuation and (un)interpretability. Even if such features and valuation process actually existed and were a necessary condition for language (against which we have strongly argued in Krivochen, 2011a, 2012a, b), it does not follow that Agree itself is necessary for language, additional stipulations have to be made, namely:

- Features come in four variants: valued-interpretable / valued-uninterpretable / unvalued-interpretable / unvalued-uninterpretable (see Pesetsky & Torrego, 2007; cf. Chomsky, 1999)

- A single feature must be present in two locations, namely, a "probe", in which it is unvalued, and a "goal", in which it is valued. This justifies the feature value copy that makes the unvalued feature in the probe an inert element for interface purposes.

Orthodox Chomskyan linguistics is forced to make these assumptions (or equivalent) in order to have a consistent theory, but with a high cost: lots of new elements have to be introduced almost on a daily basis so as to maintain the machine working and providing "accounts" of the data. The disadvantage we find here is mostly methodological, and this is something that has been more obvious since Chomsky (1998). He claims that language has the property of displacement, which means that objects are interpreted in different places from which they phonologically appear. However, displacement is seen as an "imperfection" of the language faculty, which is not to be admitted under so-called "Minimalist" assumptions. Therefore, Chomsky introduces the concept of "uninterpretable features", another "imperfection", and links both by claiming that uninterpretable features lead elements to "move" in order to value / check those features, in short, to eliminate them from the syntactic representation. The problem is kind of solved, but, as we said, at a high cost: there is no independent reason why we should have features in the first place[7]. This is the main argument we will present here against proposals like Reuland's (2011): the fact that a property X can be accounted for via Y (for instance, recursion via FLN) means neither that Y is a necessary or a sufficient condition for X (i.e., we could have recursion without FLN), nor that Y even exists outside a theoretical framework.

Argument 2: Chomsky (1970, 1986); Boeckx (2008, 2010); Hornstein, Nunes & Grohmann (2005)

The arguments these authors present, which are by no means restricted to them (just like Reuland's is just a representative take on the issue of the specificity of the language faculty which many others have followed), is much simpler than Reuland's, but perhaps it has been accepted as a fundamental evidence in favor of the Chomskyan enterprise for many more years, going back to Chomsky (1970). The proposal can be summarized as follows: there is a set of fundamental questions about language, whose answer results in a plausible theory of language origin, structure and evolution. Chomskyan generativism can provide answers for these questions (none of them claims it has indeed provided fully satisfactory answers, though).

[7] For a very recent take on the issue, see Putnam & Fábregas (forthcoming). Their argument can be summarized as follows: there are certain phenomena (categorization, labeling, clitic positioning) which "have to be present in the narrow syntax". However, the article *presupposes* the existence of features, without discussing the possible implications of a feature-less model.

Therefore, Chomskyan generativism is a plausible theory of language. Let us see the questions and then make our remarks on the methodology chosen to support the program (see, for example, Boeckx, 2008: 44; 2010: 12; and Chomsky, 1970 as the *locus classicus*)

12) (1) What is the knowledge of the Faculty of Language or language?
(2) How did this knowledge or faculty develop in the individual?
(3) How is that knowledge put to use?
(4) How is it implemented in the brain?
(5) How did that knowledge emerge in the species?

It is to be noticed that the aforementioned set of propositions do not always take the form of questions of the level of generality and vagueness (which allows the linguist to make presuppositions derived from the very formulation of the problem, we will come back to that below), sometimes it is a set of "big facts" about language that has to be accounted for, and a long argument about how Chomskyan linguistics is the best way to deal with it (as in Hornstein, Nunes & Grohmann, 2005), all other alternatives being rendered as non-credible proposals (Chomsky, 2012b: 3), without further discussion. More than theoretical or empirical, the flaws we find in this line of argumentation are *rhetoric*. Take question (1), for example: the equation of "knowledge" and "faculty of language" is not innocent. It is possible to think of knowledge of language without a faculty of language, moreover, if the goal of (bio)linguistic theory is to account for the existence of UG and FL, hardly could this goal be attained if we are presupposing the object within the inquiry. If all we should know about language is FL (that is, i-linguistics), then (1) is asking:

13) What is the knowledge [of language] or language?

which is, as Postal points out, to confuse an object and the knowledge of that object. In our terms, there is a confusion between an element from set (a) -objects of the phenomenological world, independent of a perceiving mind- and an element from set (b) -propositions about those objects, presupposing a (modal) subject-, assuming that the knowledge of language is a set of propositions referring to an independent object, a basic assumption in our approach. The mere mention of "knowledge" is problematic in itself, since "know" is actually a binary predicate, in conceptual terms, relating a "knower" and a "knowee". There are, we think, certain objects that are only defined by a "knowing" relation, like mathematical relations. Postal (2012) gives the example of "the square root of 169", and he says that square root is 13 "regardless of who knows it or whether anyone does". We think his position oversimplifies the problem on the opposite extreme of what Chomsky and Boeckx do. To be "the square root of" is a *relation*, not an *object*. As any relation, it is at least a dyadic predicate; triadic if one is to assume a Peircean analysis for these kinds of relations: a denotatum, a denotans and an interpreter (the terminology may vary, but the notions are the same). In any case, the very same object of inquiry is presupposed in the question, along with at least two properties:

- It is knowledgeable
- It is in fact the knowledge about itself

For starters, this is not good methodology. Notice that if the first question falls apart, all the rest do as well: what if there is no such a thing like "knowledge of language"? Not that we are actually saying that, but it is a possibility that has to be considered. Take, for example, functional approaches in which language and knowledge of language are two completely different things, and the study of the evolution of language phylogenesis and ontogenesis is

carried out from an externalist perspective, taking into account the social conditions in which language played a role (see DeLancey, 2001). The set of questions do not set an agenda for research, they are actually a manifesto which can be reconstructed from the presuppositions each partial interrogative generates, namely:

14) (1) There is a faculty of language
(2) It develops in individuals
(3) It is put to use
(4) It is implemented in the brain
(5) It emerged in the species in some manner (without it implying necessarily that it is species-specific)

(1) and (4) are actually *conclusions*, not departing points. Therefore, the inquiry that takes these "questions" as guidelines does little more than trying to prove itself. We thus reject this methodology, even if the results turned out to be correct: there must be, we argue, independent ways to get to them without assuming conclusions as starting points.

2.1.1 A note on syntacticocentrism

One of the foundational stones of Chomskyan approaches to language is the claim that there is a Faculty of Language which allows a speaker to creatively understand and produce an infinite number of sentences. More recently, the advent of the Minimalist Program and research on the so-called "performance systems" Sensory-Motor S-M and Conceptual-Intentional C-I have led to a distinction between "faculty of language in the narrow sense" (FLN), comprising only the computational system in charge of recursion; and "faculty of language in the broad sense" (FLB), comprising the interpretative systems S-M and C-I as well (Hauser, Chomsky & Fitch, 2002: 1570-1571). In a word, FLN = Merge. However, it is not as simple as it looks like: recall that Merge is highly constrained in traditional Chomskyan theory, particularly by Agree issues that, while guaranteeing a constructivist system in which only well-formed formulae are generated, add both elements and operations to the theory in an undesirable methodological movement, which is absent in constraint-based (though also mentalist in nature) theories (see Culicover & Jackendoff, 2005 for an overview, including Construction Grammar, HPSG, and other non-transformational theories of syntax). Within the theory, however, the logic is flawless: all operations are feature-driven (Chomsky, 1998), Merge is an operation, then it must be feature-driven (see, particularly, the proposals of Pesetsky & Torrego, 2007 and Di Sciullo & Isac, 2008). If the flow of information is *unidirectional*, from the lexicon, through the syntax, to the external performance systems, this means that all the information that reaches the interface is already encoded syntactically, in the form of features or properties of lexical / functional elements. These instructions come in three varieties: semantic, phonological and formal. The latter are not interpretable by any system; consequently, they must be eliminated through Agree (involving, in early Minimalism, *search*, *copy*, and *deletion*). The question is: can all properties of a sentence be encoded in interpretable features? It is useful to see what Culicover & Jackendoff's (2005) Simpler Syntax program, a fully consistent alternative to orthodox Mainstream Generative Grammar (MGG), as they call it, has to say about the issue:

"Culicover and Jackendoff (2005) argue that the increasing complexity and abstraction of the structures posited by mainstream generative syntax up to and including the Minimalist Program has been motivated above all by the desire to encode all semantic relations in overt or covert syntactic structure, and that ultimately the attempt fails because semantic relations are too rich and multidimensional to be encoded in terms of purely syntactic mechanisms." (Jackendoff,

2011: 7).

Indeed, it has been the case that the inventory of features has grown exponentially over the years, up to a point in which there are some nomenclatures that correspond to more than one feature[8]. We find two particularly important claims in Jackendoff's quote:

15)
a) Semantic relations are multidimensional
b) Semantic relations cannot be encoded by means of purely syntactic mechanisms

Of course, some further clarifications are in order. The term "multidimensional" is left undefined, and this undermines the proposal, since it can be interpreted in either a technical sense or an "everyday" sense: in the former, concepts are located in n-dimensional spaces, quite in the line of Wittgenstein's (1953) logische Raum. This is the hypothesis we have put forth in Krivochen (2012a) in the light of our geometrical model of syntax: roots denote n-dimensional areas within the conceptual space and procedural elements restrict that space in such a way that the restricted reference of an entity can be specified as a finite set of coordinates, whose number depends on the kind of object we are dealing with: an event, for example, is, under the simplest assumptions, an ordered pair (space, time(√)) in the conceptual space. However, there is another sense (the "everyday" sense we mentioned above) in which to interpret "multidimensional", which is simply to have different aspects of the same object. That is, multiple layers of meaning (for instance, explicatures, implicatures, presuppositions) are also referred to as "dimensions" in some works. Since the sense in which the term is taken is not specified we can make no judgment of Jackendoff's position, but it is internally consistent, and belongs to neither Chomsky's nor Postal's "theoretical pole".
The second point is a little more conflictive: if understood in a wide sense, it is (specularly) restating Chomsky's (1957) thesis of the independence of the grammar, insofar as "grammatical" cannot be equated with "meaningful" since they are defined on different grounds. Of course, Jackendoff has modeled a (cognitivist) semantic theory beyond anything that has even been proposed within Chomskyan linguistics, but the spirit is the same. Now, if understood in this wide sense, the claim is obviously false. That is because semantic relations like "be the agent/patient of", for example, can be read from the position of arguments in relation to the position of relational predicates (quite in the line of Hale & Keyser, 1997, 2002; Roberts, 1991, for both generativist and non-generativist proposals), with no need to adhere to strong static theories like Baker's Uniform Theta Assignment Hypothesis, which links univocally syntactic positions to theta-roles (something that would not work in an *n*-dimensional, dynamic model of syntax). For example, anything within the scope of P is interpreted as a Ground, while its periphery is interpreted as Figure, being within a locative domain but under the scope of an eventive node (see Mateu Fontanals, 2002 for discussion):

16) [VP event [NP [P [NP]]]]

Hale & Keyser (1997: 38) put this thesis quite clearly:

[8] Perhaps the best example of such a theoretical complication is the famous "EPP", which is now understood in at least three senses (see, for instance, Gallego, 2010: 62):
a) Move a DP to Spec-TP (the traditional definition of the EPP)
b) Merge α with β containing EPP (sometimes called "edge feature")
c) Move an XP to an outer specifier of a phase head (sometimes called "occurrence feature")

"*While there is surely some truth in this, and while meaning it is a fine heuristic, its use is methodologically incorrect within the framework we are assuming here, not merely because meaning is slippery, a thin reed to lean on, but because we maintain that certain crucial aspects of meaning are dependent on the very structural features whose identification is at issue.* ***If we "knew the meaning", we would know the structure, perforce, because we know the meaning from the structure***." (Our emphasis)

Following this lead, Mateu Fontanals (2000b) proposes a very interesting principle ruling the syntax-semantics interface, in clear contraposition to the wide interpretation of premise (15 b) above:

17) *Meaning is a function of both (non-syntactically transparent) conceptual content and (syntactically transparent) semantic construal.*

Needless to say, the syntax-semantics mapping is not uniform (it would require identity between the systems to have uniform mapping, the operation thus resulting trivial), but this is not to say that there is no mapping at all. In fact, we have proposed as an alternative to the syntactico-centric "feature-driven syntax" that syntax is driven by the need to generate C-I convergent objects (Krivochen, 2012a; Krivochen & Kosta, 2013): in a word, syntax is *semantically driven*. In a non-trivial way, then, semantic relations are encoded in syntactic configurations because it is the only way to represent them all the way through a derivation in consonance with the Conservation Principle (Krivochen, 2011a, b; see also Lasnik, Uriagereka & Boeckx, 2005)

18) *Conservation Principle*: information cannot be eliminated in the course of a derivation, but it must be instantiated in the relevant system in such a way that it can be read and it is fully preserved.

If the road to take is *conceptual structure* → *syntactic structure* → *semantic representation* (LF), a function relation between syntax and semantics is inevitable, which means that semantic functions must be encoded in syntactic relations.
The second sense in which we can interpret "syntactic mechanisms" is "substantive elements present in the syntactic workspace", instead of "relations created by means of syntax". In this second sense, premise (15 b) is a clear plea for Minimalism. Semantic relations like "being the agent/patient of", if encoded via features (as in Hornstein's 2003 model), complicate the system in at least two ways:

19) a. New features must be added
b. Agree mechanisms to value those features become ineliminable

That, without even considering the possibility of argumental alternations, that is, the fact that a predicate can appear in different sentences with different argumental requirements, for example:

20) The ice *melted* (ergative, uncaused)
21) The heat *melted* the ice (causative, non-volitional)
22) The explorers *melted* their way through the ice (Path-of-Motion)

Notice that, if alternances were to be encoded as features, we would need at least two features: one for transitive alternations (which require an internal argument and an external argument) and one for intransitive alternations, with one or two internal arguments (for distinguishing ergatives from unaccusatives respectively). Such a theory has been advocated for by Hornstein

(2003), who considers that theta roles are features, which are checked in a Spec-Head relation between a nominal argument and a V. DPs must move to get theta-features, thus deriving the property of displacement from the need to get semantic interpretation and avoid superfluous elements in a syntactic representation (namely, unvalued features in a DP). This way of encoding semantic notions as syntactic substantive elements is clearly out of the spirit of Minimalism, and undesirable in a system that founds itself on principled reasons. These arguments argue against the very foundations of the feature-driven syntactic system as the locus of semantic relations, following premise (b). We see that even those approaches that present themselves as alternatives to mainstream Minimalism fall short when considering their basic claims in depth.

In the next section, we will deal with the so-called "realist approach", analyzing it as we have critically revised the "conceptualist" approach. In doing so, we will also present our own proposal, in permanent dialogue with Katz & Postal's realism as we have done with Chomsky's conceptualism.

2.2 The Realist Enterprise

The most recent summary of the realist enterprise, and at the same time, a review of the arguments Chomsky has systematically ignored over the decades is to be found in Postal (2012). Postal (2008) points out that the "biolinguistic ontology" is incoherent, and Postal (2012) extends this claim beyond the limits of the label to argue against the ontology of language Chomsky proposes. It would not be possible (although it would certainly be desirable) to talk about "debate" since it has been a one-sided discussion: Chomsky rarely addresses a criticism from outside the orthodoxy. However, even Katz & Postal (apparently in discomfort with the Chomskyan enterprise) present their position as the *only* alternative to conceptualism, which makes it a two-sided problem, independently of the (re)actions of the actors involved. Chomsky's "conceptualism", which claims without any demonstration or argumentation that grammars are "(…) *real objects, part of the physical world* (…)" (Chomsky, 1983: 156) is explicitly rejected, both by Postal -and ourselves-, insofar as there is no definition of "real" or, more importantly, of "the physical world". There is an irresponsible use of the word "physical", as it has led to great misunderstandings and shortcomings, incidentally also affecting Postal's stance. Postal's position, which he has made explicit in a number of works with J. Katz (particularly, their 1991 article) and some recent solo pieces, is that

"(…) *Katz's work not only rejected NCs psychological/biological conception of NL but developed the distinct platonist view that* ***the elements NLs are composed of, sentences, are abstract not biological objects****. Moreover,* ***NLs, taken as certain classes of sentences, are clearly abstract objects and hence not biological entities****. NL sentences share, under Katz's view, the ontology of mathematical objects, e.g. numbers, logical objects, e.g. propositions, musical objects, e.g. songs, etc.*(…)" Postal (2012: 4). Our highlighting.

Notice that the fundamental thesis is that sentences (in this fragment, clearly an intra-theoretical term) are not biological but abstract objects, without making it explicit what it means and implies: what exactly is an "abstract object"? We could picture "biological objects" from common sense assumptions (but with no theoretical foundations, since they are not provided in Chomsky's writings, and there are only vague references in most state-of-the-art books, like Di Sciullo & Boeckx, 2011) like, say, a cell. A species, for example, is not an object of our metatheoretical set (a), but, in any case, an abstraction of intensional characteristics belonging to the set (b): the species of the tigers, for example, can be described as the set of intensional characteristics any entity X must fulfill in order to belong to the set of "tigers". The ontology of

so-called "abstract objects" in platonistic linguistics is far from clear, particularly as mentions to mathematics and physics enter the scene. In the excerpt above, three kids of objects are invoked as examples of "abstract objects": numbers, propositions and songs. They are of little use, since those objects greatly differ from each other. The nature of numbers is still far from clear, see Frege (1884), and as we said in the previous section, it is not clear at all that mathematical relations like "be the square root of" are actually as independent from subjects as Postal claims, without providing any argument for it. Moreover, computer science has provided both upper and lower bounds for computability of certain expressions given, say, different memory capabilities: a simple Push-Down Automaton does not have the same computational properties as a Turing-Machine. Arguably, the differences rely on the software used to generate and process symbolic representations being dependent on the hardware: consider Hameroff & Penrose's OrchOR model for quantum consciousness, based on quantum vibrations in neural microtubules, which are in turn liked to processes in physics and cosmology (see Hameroff & Penrose, 2014 for a very recent presentation of the theory). If the software is a function of the hardware, the alleged *gap* between the two levels might not be such.

Still, there is a more important problem in Postal's criticism of the Chomskyan version of linguistic conceptualism. While it is true that there is a gap in Chomskyan linguistics between formal tools and biological instantiations, in such a way that the biological content in the arguments (e.g., most articles in Di Sciullo & Boeckx, 2011) seems alien to the linguistic argumentation and vice versa, Postal (2012: 5) takes the incompatibility to another level. He claims that:

"*The reason for the incoherence of NC's foundational position is that (...) the nature of NL sentences has always forced NC to describe them in a way incompatible with their being biological (...). Anything biological would* ***exist in time and space****, would* ***have a cause****,* ***could cause things****, would* ***be destructible****, would* ***have mass or energy****, etc.,(...). But NL sentences have no* ***physical*** *properties at all*." (highlighted in the original)

This paragraph shows a very naïve conception of what "physical" is. Notice that Postal is confusing "material" with "physical", which is only acceptable if one interprets "physical" in the everyday sense, but in a technical discussion about the foundations of the biolinguistic enterprise, such a slip must not be overlooked. Let us clarify the position we will take on this issue, following current theoretical physics. While it is true that anything biological exists in time and space; that is also true of anything physical. In fact, to clarify the idea, time is (bended) space (as special relativity has demonstrated, at least beyond the Planck scale), with which all we have is "anything biological exists in space", quite a trivial claim for the ends pursued by Postal. But the informative part comes afterwards: anything biological would also "have a cause". This is completely unclear to us, since "cause" is very different from "origin" (clear evidence can be obtained from lexical semantics cross-linguistically, see Kosta, 2011 for a thorough study). We can say a cell has an origin, has evolved, has changed and undergone several processes, but we can hardly say it has a "cause". On the one hand, because the argument would have a teological flavor we want to avoid (recall Thomas Aquinas' arguments for the existence of God based on the "uncaused causator"); on the other, because the implications of the notion of "causation" go way beyond the scope of biology: causation can entail volition or not, but the possibility is always there. We can hardly say that there is "volition" in biological change under the light of modern evolutionary theory. The third characteristic, "could cause things" falls apart with this very same criticism. Consider the following sentence:

23) The wind opened the door

It is quite clear that we have a caused event, and the external force which caused it is [the wind]. However, it is not a biological "thing" in any relevant sense we can think of. Conversely, let us see what would happen if we tried to make a causative sentence with a universally accepted "biological thing":

24) The gene caused her disease

Most biologists would say the verb choice is at least inaccurate. A consequential relation between two events does not entail causation, as it can be seen in the following example:

25) It is cold because it is winter

While we could say that the cold is a consequence of the winter, it would be at the very least odd to assert that the winter is the cause of the cold in a strict sense. Coming back to (24), while it is possible that a disease or an impairment (e.g., in language) may be related to a mutation in a specific gene, no reference we know of refers to Specific Language Impairment in terms of "causation".

The fourth and fifth characteristics are perhaps the most important for the purposes of the present discussion. Consider "be destructible" and "have mass or energy". This is actually true of biological "things", though it is not of species or relations, which also fall within biological studies. But let us go deeper in the final claim: "(…) *but NL sentences have no physical properties at all*." Either Postal is equating biological to physical, which would be a gross methodological and substantive mistake (physical magnitudes are not biological entities in any sense, and physical objects are not always biological "things": there are particles with no mass), or he tries to make Chomsky claim two –apparently contradictory- things: that language is at the same time biological and physical (perhaps, a straw-man fallacy). In any case, the only condition that would hold for physical objects would be to "have energy", and this would be valid (that we know) only to a certain extent (even mass is a property that physical objects might or might not have). Consider 1-dimensional strings, for example. It is not clear whether they have mass, let alone energy. They do vibrate, but the *source* of the vibration is yet unknown. Physical magnitudes have neither mass nor energy, and vectors, for instance, represent *forces* in *n*-dimensional spaces, but certainly not *mass*. What is more, mass = force / acceleration, and we can express force and acceleration effects (the latter, subsuming gravitational force in Einstein's conception of gravity) independently of mass. Under the light of the preceding discussion, Postal's claims against conceptualism are not better founded, particularly the notion of "abstract" as opposed to "physical". While it is true that Chomsky has obscured the ontology of his linguistic program and the set-theoretical notions on which he bases the syntactic representations he uses, it is not less true that Postal does the same with the vague opposition "physical" / "abstract", grounded on dubious notions as far as "physical" objects are concerned, as we have seen. Postal finds several "incoherences" in Chomsky's program, one of which is his use of set-theoretical notions, particularly regarding Merge and the model outlined in The Logical Structure of Linguistic Theory to describe sentences, if these are actually biological objects. Postal claims:

"(…) *physicists use abstract formal structures to characterize physical things, not abstract ones. The objects of description have temporal, spatial, causal, etc. properties*" (Postal, 2012: 7)

We have already argued that Postal's stance on physical objects does not take into account many powerful counterarguments from theoretical physics (arguments that a piece that mentions "physical things" cannot overlook), apart from the crucial fact that (physical) models and (physical) objects are well-defined within physics (thus, for instance, no one would think Lorenz's attractor *is* a chaotic system, but a *model* of a chaotic system; whereas climate *is* a chaotic system), and we find no such clear distinction in linguistics[9]: notice that Postal takes natural languages as "certain classes of sentences", without further clarification about which are those *classes* (not actually NLs) or which is the nature of those sentences (which would actually *be* NL). Postal's argument against Chomsky falls for its own weight: he talks about description, not explanation. And there is no principled reason why a biological object cannot be *described* or *modeled* set-theoretically, using natural numbers or whatever model the theoretician wants. Take an atomic model, for instance: it is a trace of pencil on a piece of paper used to describe the structure of the atom in an approximate way. Can we object to that? Certainly not, to the extent that the nature of the description is not equated to that of the represented object: X-bar theoretical trees, for instance, are not the *structure* of a syntactic object, or a syntactic object itself, but *representations* of the structure of a syntactic object. Even if we use a physical object to describe a physical object, or a formal structure to describe a formal structure (as in metamathematics), the distinction between our sets (a) and (b) prevents the model from criticisms like Postal's. In Chomskyan linguistics, set-theory has the status of a *metalanguage* (thus, our (b) set), as Chomsky himself acknowledges:

In the work that I've done since The Logical Structure of Linguistic Theory – which just assumes set theory – I would think that in a biolinguistic framework you have to explain what that means. We don't have sets in our heads. (Chomsky, 2012a: 91)

Chomsky continues by assuming that set-theory is not neurologically realizable and thus not a candidate for the actual structure of language within the mind, which is a controversial claim since no further argument is provided: the claim that there is a gap between mathematical structure and physical reality, which both Postal and Chomsky share, is *not* unavoidable, nor is the ambiguity with which the concepts of "physical" and "real" are used. Overlooking for the time being computational models for the human mind like the Turing Machine (a favourite of Chomskyan linguistics, see Watumull, 2013), consider modern models of the Universe within mathematical cosmology, like Tegmark's (2003, 2007):

"*External Reality Hypothesis (ERH): There exists an external physical reality completely independent of us humans.*
Mathematical Universe Hypothesis (MUH): Our external physical reality is a mathematical structure." (Tegmark, 2007: 1)

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[9] Consider, for instance, how Postal exemplifies his point: (2012: 5)
"Take for instance:
(3) Most rabbits have big ears.
Where in space is (3)? At what points in time did it begin and will it end? What is its mass? Is it subject to gravity? How can one destroy it? These questions, entirely appropriate for physical things, biological or not, make no more sense than their parallels for objects like the square root of 169 or Sibelius' 5th Symphony."
We find many misunderstandings here: consider 1-D strings, as physical constructs: the question about where in space they are is, at least, misguided: the relevant question would be about scale. There is no point in asking about their beginning and end, because they might not have such things. And so on. The same happens when we go beyond the Planck scale: is space (considered as a quantum foam) a physical "thing", in Postal's terms? We are afraid it might fail some, if not all of Postal's "tests".

Notice that ERH is independent of MUH, it could very well be that there is an external reality (an anti-solipsistic statement) but that it is not a mathematical structure. But the crucial point here is that, contra both Postal and Chomsky, there is *no internal contradiction* between ERH and MUH, therefore, no "gap" to be accounted for if we claim that biological structures are in fact mathematical structures: the reader should acknowledge that while this claim might be false, it is not inconsistent (and it is inconsistency we are discussing in this piece). It is to be noticed that HPSG, CG, and LFG make no explicit claim with respect to the mental / biological / neurological status of linguistic objects (constructions, sentences, lexical items…), and there is no, say, "HPSG manifesto" regarding the ontological nature of the researched objects.

Another argument in favor of purely mathematical models of linguistic knowledge (to which we will restrict ourselves in the present paper) comes from the relation that exists between biology, physics and mathematics and their respective objects of study[10]. Chomsky's claim that Merge forms sets, but that is "metaphorical" and "the metaphor has to be spelled out someday" (2012a: 91) suggests that the only way to think about Merge is metaphorically and thus the phenomenon of structural complexity in the Universe is to be yet explained. Needless to say, if MUH is considered (again, a fully independent and internally coherent proposal on its own), there is no metaphor involved at all: we have atomic (i.e., indivisible) elements at all levels, be them strings, conceptual roots, numbers, nucleotides or whatever object in whatever level; and some concatenation algorithm, which may or may not be sensitive to the characteristics of the objects it manipulates (e.g., nucleotides cannot be freely-merged). To the best of our knowledge, this enters the kind of propositions that are true because of Chomsky's frequently invoked "virtual conceptual necessity" in the following sense: for the Universe to be as it is, with different layers of complexity, both atomic elements and a combinatory operation are necessary conditions. This does not mean that this is the only possible universe (particularly taking into account recent Multiverse proposals), but that any theory that aims at descriptive and explanatory adequacy in this Universe must address the issue of complexity. If not with a mathematical algorithm like pure *concatenation* (in which there is no metaphor whatsoever if the Universe is itself a mathematical structure), we find it difficult to think of something else, but this does not mean there is in fact nothing else. Let us make explicit what is meant by *concatenation* in our own alternative theory, Radical Minimalism, since it is quite different from what is meant by Merge in orthodox Chomskyan linguistics:

26) *Concatenation* defines a chain of coordinates in n-dimensional generative workspaces W of the form $\{(x, y, z\ldots n) \subset W_X, \ldots (x, y, z\ldots n) \subset W_Y, \ldots (x, y, z\ldots n) \subset W_n\}$.

Our generator engine, it is to be noticed, is not sensitive to the substance of manipulated objects, but to their format: not inner structure, but ontology. The only condition to apply concatenation is that the objects, defined as *n*-plets of coordinates in *n*-dimensional conceptual spaces, following the lines of Krivochen (2012a) and current advances in cognitive linguistics. In this framework, geometrical "figures", sentences, and other "observable" objects which constitute the phenomenological world are epiphenomenal results of concatenation in one or several W read off at an interpretative system, in the event that there is one: arguably, mathematical derivations have no interface conditions, being therefore examples of pure syntax. This definition, which follows clearly from what we have been saying, can be formulated in a stronger way: there is no physical reality beyond the interpretation of the concatenation function applied to an n-number of objects sharing format. This is another way to express Tegmark's

[10] Needless to say, this does not mean that non-mathematically based models of language are not interesting or relevant, for other respects. A model of competence like the one outlined in Sag & Wasow (2011) would be inherently limited if a purely mathematical stance was adopted.

MUH; the so-called physical reality is a mathematical structure. We accept MUH without necessarily accepting ERH (the External Reality Hypothesis) as it is formulated: in any case, the notion of external and reality should be redefined. This must not be interpreted as a plea for solipsism: the concatenation function applies to objects that are external to the human mind and perception plays no role in generation. Moreover, there is no need to resort to a human mind: an automaton with the algorithm incorporated and interpretative routines (based on Relevance principles, see Sperber & Wilson, 1995) could serve as well. In this point, it is crucial to say that, if our view of syntax is that of a generative component that manipulates objects regardless their nature, then it can be applied to any so-called "complex object" insofar as complexity can be decomposed in layers of simple, atomic elements somehow concatenated for interpretation purposes. If this view is correct, all physical systems would have "derivations", in the sense of "successive applications of concatenation and subsequent interpretation".

It must be noticed that many critics to the biolinguistic position often come from both a misunderstanding of the concepts on the part of the one who makes the criticism and a frank lack of clarity and definitions on the part of the biolinguist. Let us see an example:

> *QUESTION: Infinite use of finite means; doesn't it entail an inconsistency? Isn't the model of an infinite potential in, a finite organ inherently inconsistent?*
>
> *CHOMSKY: That was the problem until about a century ago. It did look like an inconsistency. One of the important discoveries of modern mathematics is that it isn't an inconsistency. There is a perfectly coherent sense to notion of infinite use of finite means. That is what ended up being the theory of computability, recursive function theory and so on. It is a big discovery of modern mathematics which clarified traditional ideas. There have been sort of intuitive ideas like this around but they really became clarified quite recently-not really until almost mid-century. So, yes, it looks like an inconsistency but it simply isn't. There's a very simple account of it that is not inconsistent. I can't go into it any further here."* (Chomsky, 2000: 62-63)

The "infinite use of finite media" issue was raised in language by Humboldt, but it was already a topic in mathematics and, decades later, in computer science. Let us see an example and then discuss Chomsky's remark, together with Postal's criticism. Consider the following finite set of natural numbers:

27) (2, 4, 6, 8)

How many combinations can we make with them? A quick response would be "*either n! or* n^n*, depending on whether repetition is allowed or not*". This answer would be, at best, incomplete: there is an implicit assumption that we can formulate in the following way: given a certain amount of numbers, possibilities of combination are restricted to that amount. For example, among the possibilities that the reader may have in mind these are surely included:

28) 2468 – 2648 – 2846 – 2684

But the ones in (29) are most likely out:

29) 24648 – 26242864

And so on and so forth. Are we cheating here? Certainly not, since combination is only

restricted by stipulation, and we have added none. The "infiniteness" is a property of the *operation*, not of the objects that is manipulates. Let us see Postal's reasoning at this respect:

"*A real organ, e.g. a lung, is finite along every dimension including the temporal one and* ***everything it does or produces is finite****. So* ***if NL were an aspect of brains, it too would be finite and the output of sentences*** *(granting counterfactually that it makes sense to take organ outputs as sentences)* ***would be as well***." (Postal, 2012: 14. Out highlighting)

At the very least, this is a *non sequitur* situation; at the very worst, a manipulation of some basic claims of Chomskyan linguistics. Natural languages (NL) are not aspects of brains, but of minds, which have neurological substratum (the equation brain = mind is not innocent here, as the dynamics hardware-software are at the very core of the discussion: the brain is material, therefore, finite, but there is no proof, either formal or empirical, that the computational emergent properties of matter are necessarily finite, see Watumull, 2013: 207 for a recent discussion on the level of Turing-computation). We doubt that the issue of finiteness or infiniteness of the mind even makes sense, at least without a formal definition of finiteness on the one hand, and a strong argument about the nature and ontology of the mind on the other (neither of which is presented by Postal after his criticisms). Even without this fundamental correction, there are aspects of Postal's claim that are unclear -if not plainly wrong-: in which sense do we say that a lung "produces" something? Sure, there is O_2 coming in and CO_2 going out, but can we say that the lung "produces" CO_2, at least in the same sense that language is produced? There is not a single proposal that lungs (or any other "real organ") are in any sense computational, thus, they are not generative in the relevant sense of complexity creators via combination. Language exists in the mind, while it is not the only "place" in which it might exist, it sure has mental entity at some point (there is an intention, which is embodied in a sentence that has entity before it is externalized, all this without even entering the realm of Fodor's "language of thought"). Mental objects have biological substrata, or, in other words, it is the configuration of the biological substrata that licenses computational properties of the "mind", quite a common claim in psycholinguistics. If there was no neurological (therefore, biological) dimension, brain injuries should not affect language. This is quite straightforward. There is a link, even if the specific kind of link (cause, mere concomitation, etc.) we are talking about is yet unclear. Please notice that in the whole of the preceding discussion we have not made use of the concept of UG, which we reject (Krivochen, 2012b; Krivochen & X, 2013), because we simply have not needed it, and there is, as we said, no compelling evidence in its favor. A deep revision of Postal's arguments would have to acknowledge the fact that he sometimes uses NL and UG as synonyms: for example, "NL is both biological and infinite". UG (as the initial state of the Faculty of Language) is apparently biological (in Chomskyan linguistics), NLs are infinite by nature and regardless the theory we consider (neither formalists nor functionalists would say that a natural language is a finite set). This confusion, which is, like others, in the function of a certain rhetoric; is to be eliminated as a methodological *mala fide*.
Considering the Chomskyan response now, it is amazing how a very simple issue is made so complicated with irrelevant historical details and no argumentation, just a finishing line "I can't go into it any further here". Nor, apparently, anywhere else.

2.2.1 The evolution of realism

It is quite surprising for the follower of Postal's arguments that they have been turning from serious linguistic-philosophical objection into ad hominem claims, displaying more rhetoric manipulation than convincing arguments (quite the same could be said of Chomsky's 2012b

paper, which is an impressive set of stipulations under the form of a formal discussion on labeling and projection). Perhaps the most interesting substantial exposition of the realist proposal (summarized in Postal, 2012, as well as the arguments against Chomsky's view from many previous works) is to be found in Katz & Postal (1991). While still very much influenced by the GB tradition, the article is a concise presentation of the so-called "realist" position and the fundamental differences with Chomsky's ontology. The same confusion of the three sets (a), (b) and (c) we claimed exists in "conceptualism" is to be found here, in a stronger version than in Postal (2012). Consider, for example, the following quote:

"(…) *acceptance of an overlap between the senses of NL sentences and logical objects involves linguists in foundational issues at least to the extent of committing them to a common ontological position for linguistics and logic. This overlap assumption confronts one with the following paradox. If senses are parts of the grammatical structure of sentences and if linguistics both deals with the grammatical structure of sentences and is psychological, then senses are psychological. But if senses are psychological, then the laws of logic are also psychological, since the ontological status of a law is determined by the nature of the objects to which it refers. Consequently, logic is psychological, contradicting the accepted view that logic is nonpsychological.*" (Katz & Postal, 1991: 520)

Notice that the argument is only valid if set (a) and logical principles overlap. However, logical laws do not belong to set (a), since they are essentially *meta-statements*, thus belonging to set (b). Logical objects are, in any case, part of the formal apparatus used to analyze NL, but crucially *not NL sentences*. In fact, no claim is made about the status of NL sentences, just a presupposition, generated by the conditional "*if senses are parts of the grammatical structure of sentences*…", which is incompatible with the formal, mentalist approach that has prevailed in Chomskyan linguistics since *Syntactic Structures*. Externalization (i.e., materialization of syntactic structure via phonological matrices) has always been rendered parasitic, an "exaptation" (see, for example, Uriagereka, 1998, 2000): we cannot think of another sense in which there is a "sensorial" aspect of NL sentences. In any case, externalized sentences are part of e-language, explicitly excluded from the scientific study of language (an arguable claim, but it is not fair to disqualify a theory because of the methodological boundaries it has set itself). Katz & Postal (1991: 521) argue that the mistake of conceptualism is the failure to distinguish between knowledge of language and the object of this knowledge, language itself. They further elaborate this claim in three arguments they provide against the conceptualist enterprise. In the next section, we will analyze those arguments.

2.2.2 Three arguments against conceptualism

Katz & Postal (1991: 522 ff.) develop three arguments supporting the realist position, and arguing against what they understand as Chomsky's position. Before entering the arguments themselves, it is useful to address a commentary in a footnote, which seems to point at an inconsistency within the so-called "realist" enterprise. Katz & Postal (1991: 522, fn. 4) claim:

"*Richard Montague advocated a realist approach to universal grammar, claiming that it should be pursued as part of mathematics*."

In this context, it is to be noticed that the notion of "realist" is still left undefined. The confusion is even greater when one considers that one of the objections to the conceptualist approach is the use of set-theoretical apparatus to explain the process of Merge, either in Chomsky's or Kitahara's approach. If UG/FL is to be studied as a part of *mathematics* (which obviously

licenses set-theory, as well as other formalisms), either Montague's approach is not realist or Chomsky's is. In any case, in the absence of an appropriate formalism for biological systems, both proposals are equally invalid as explanatory frameworks, achieving at most descriptive adequacy. It is curious that the biolinguistic approach takes biological systems for granted (with the exception of Jenkins' article, mentioned above) and does not attempt to find a mathematical model for, say, genotype-phenotype dynamics or derivations in the mind-brain. This apparent gap (which is not so if, as we said, we consider that biological systems are particular instantiation of physical structures following limitations over mathematical systems) can be overcame if biology is modeled following, for instance, chaos theory (see, for example, Krivochen, 2013a). Not having a definition of "real", the claim that a realist enterprise can model UG as part of mathematics is an empty claim, just like it is to claim that UG's content is to be adjudicated to "virtual conceptual necessity" (VCN) (see for example Chomsky, 1995: 169; 2005: 10; 2007: 8, 12), particularly Merge, Copy and Move, whose specific biological entity is at best mysterious. This theoretical move has two direct consequences:

30) a. Eliminates the possibility of asking for formal demonstrations of theorems involving any of those operations, or the operations themselves
b. Eliminates the possibility of building alternative theoretical approaches, since whatever is not "virtually conceptually necessary" is to be eliminated in favor of allegedly principled "unavoidable" (sine qua non) elements.

Postal (2003) has argued against the wide use the notion of VCN, but his own methodological proposal is equivalent to (30 b). We will come back to this in the conclusion. Let us now focus on the three arguments against conceptualism.

Argument 1: The Type Argument

This argument derives from an apparent "type-token ambiguity", which is a long-dated problem in philosophy (but not in linguistics: consider, for instance, that Chomsky has clearly stated that the Numeration is a set of *tokens*, see Uriagereka, 2008: 16 for discussion about this point). In Katz & Postal's terms NL grammars and grammatical theory are about *type*-sentences. Apparently, for some unclear reason, this is inconsistent with the view that NL has psychological entity. The argument as we see it is a *non sequitur* (see also Watumull, 2013 for the same conclusion, although obtained via different assumptions): no proof is given that there are not types in the human mind (nor is any reference to *linguistic* work in which the type-token problem is discussed; notice that problematic as it may be for philosophers, the type-token dynamics have been worked on in linguistics in unambiguous terms, consider for example Martin & Uriagereka, forthcoming; Krivochen, 2013b). Moreover, even if individual sentences were tokens, there must be a sense in which NLs are mental objects, since sentences belonging to NLs are "externalized" (technically, Spelled-Out, given a phonological form), and what is not "inside" cannot be "made external". It would be possible to claim that individual sentences are tokens even inside the mind, but this would be equivalent to claiming that NLs are a set of tokens, which implies that any speaker must have some representation of each sentence he uses as a token in his mind. Katz & Postal also seem to assume that sentences have some sort of primitive status, without considering their derivation / formation. In Krivochen (2013b) and Krivochen & X (2013) we have demonstrated that a type-token dynamics enhance the explanatory power of a syntactic theory, particularly when it comes to the property of *displacement*. Consider:

"***Definition 11****: a type is an abstract element in a physical system Φ.*

Corollary: there are two kinds of types in a linguistic derivation: those that convey conceptual meaning (i.e., roots) and those that convey instructions as to how to interpret the relation between conceptual types (procedural types).

***Definition 12**: a token is an occurrence of a type within W_X [an n-dimensional workspace]. There are no a priori limits to the times a type can be instantiated as a token but those required by Interface Conditions IC.*" (Krivochen, 2012a: 8)

In this framework, there is no Merge-Move asymmetry (as in Chomsky, 1995 and much subsequent work, particularly the so-called Merge-over-Move principle): structure building is always token-Merge from the Lexicon, defined as the whole amount of types for NL, to be Spelled-Out in due time. Yes, this approach (which we have developed in Krivochen, 2013b; see also Martin & Uriagereka, forthcoming for a different though related approach to the distinction) requires the introduction of the distinction Type/Token in syntactic-semantic theory, and algorithms to link different tokens of the same type (which we have provided in Krivochen, 2013b) but it has proven useful in both a theoretical and an empirical domain: we can unify phrase structure and displacement in a single theoretical framework, without added notions like features or copy operations, and account for the data without *ad hoc* stipulations. If an element can be inserted in a structure (which would include constructions in the sense of CG as well) as many times as IC require, then we eliminate stipulations regarding intermediate landing sites for movement (see Abels, 2003 on this topic), moreover, being tokens of the same type, the establishment of referential dependencies at the C-I system becomes easier as it requires no added elements like indexes and diacritics (they can be used for expository purposes, but they would be theoretically superfluous and have no biological or physical entity).
In simpler terms, the whole argument against Kats & Postal's objection can be summarized as follows: if types have no mental entity, nor do tokens. If tokens do not have mental entity, we could not derive a sentence (assuming the generative algorithm manipulates tokens following interface conditions and thus creates interpretable structures). If we cannot derive (i.e., produce in real time[11]) sentences, there is no NL. But, mind you, there is NL.

Argument 2: The Necessity Argument

This argument is based on relations of necessary implication between sentences, or so it may seem. Consider the following pair:

31) a. John killed Bill
    b. Bill is dead

According to Katz & Postal (1991: 523), (31 a) could not be true and (31 b) false. In their approach, a "proper account of NL" must explain this necessary entailment, arising from the "semantic structures of the entailing and the entailed sentences". This argument is vulnerable on several flanks. On the one hand, Katz & Postal are assuming a theory of lexical semantics as well as a theory of truth conditions. In their account, we must explain the fact that [kill X] *entails* [X die], against which many voices have arisen (e.g., Fodor, 1970 and much subsequent work in lexical semantics and syntax-semantics interface). Our own argument against the

[11] A note is in order here: the fact that lexical derivations can be coined, thus fossilized, as well as idioms, beyond the word-level, does not mean that those fossilized linguistic elements, of arbitrary complexity, have not been derived at some point in the *diachrony* of a language L. The work on proleptic names done by Trejo (2013), under Radically Minimalist assumptions provides good arguments in favor of this approach.

"Necessity Argument" rests in the idea that language and truth do not belong to the same domain when it is not about analicity (which is based on lexical semantics within a structure, not on a relation between structures or propositions). Modern accounts of entailments make them a matter of pragmatics, that is, language in use, and not a matter of analicity in any way. They base their argument in an inference that arises from "the semantic structure" of the involved sentences, but there is no clue as to what such a structure may look like. What is more, the very concept of entailment, which is the word used by Katz & Postal, includes as one of its main characteristics being cancellable in assertive contexts (sometimes, this is used to differentiate entailments from presuppositions, which are only cancellable under the scope of negation). We will not get into this, but just point out that truth conditions, since they were relativized in formal semantics by Davidson (1967), cannot be determined in isolation (even less so if one subscribes to pragmatic accounts which reject the concept of truth values as primitives of a linguistic theory) but in relation to (minimally) a speaker, a time and a place. This said, following Fodor (1970), there is no contradiction in the following sentence:

32) Mary knew that Bill died, but she didn't know someone/John had killed him.

That is, there is no contradiction in asserting that (31 b) can be true for a speaker while (31 a) may not. The speaker can even deny the truth of one or the other based on his/her knowledge of the state of affairs described in the asserted sentence. The second argument Katz & Postal use is stained with stipulations about the nature of natural language semantics, for which they neither provide a theory nor cite independent references (only Katz's works, which are part of the "realist" enterprise, not independent theories that can be used to reinforce that enterprise).

Argument 3: The Veil of Ignorance Argument

This argument is based on the assumption that language and knowledge of language are two different things. This may sound trivial insofar as "know" is a dyadic predicate, requiring a "knower" and a "knowee", as we pointed out above. However, the conceptualist claim that there is no scientific object of study for linguistics outside the knowledge represented in the mind a speaker has of a language L (so-called i-language, with its alleged mathematical and biological aspects) is *not* an internal contradiction (although it might very well be wrong, as we think). Nor does a contradiction arise when the notion of e-language (i.e., external and extensional linguistic samples, in Postal's terms, *sentence tokens*) is included in the equation. It is true that the notion of a "Faculty of Language" is uncritically accepted as an axiom within orthodox Chomskyan studies rather than problematized and demonstrated (with due, but minority, exceptions), but that is an independent argument, which we have made in the previous section. The conceptualist thesis that there is nothing more to language than a state of a mind-brain in a speaker may very well be false, but it is certainly not inconsistent with the axioms in the conceptualist enterprise.

3. 3. A note on "progress in Linguistics"

The considerations we have been making with respect to the self-appointed exclusive tendencies in formal linguistics cast doubt with respect to the possibility of having scientific progress within the discipline: there are studies of the use of the language faculty ("faculty" in a wide, non-technical sense) from sociology, ethnography, philosophy and cognitive psychology, to mention but a few. However, the most basic problems, the foundational abstractions, the grounding concepts are still far from clear: there is not even consent as to what exactly is language, as we have seen in the previous sections. Is it a mental entity? A formal entity? A social entity? Something else? The concept of "fact" in linguistics is to be challenged if some

progress is to be made. For example, Chomskyan theories base most claims about "language design" on movement, which is interpreted as literal displacement (GB tradition) or copy (MP tradition, see Nunes, 2004 and Corver & Nunes, 2007 for an overview) of constituents following well-established rules: phase impenetrability (Chomsky, 1998), barriers (Chomsky, 1986), Minimality (Rizzi, 1990), Head Movement Constraint (Travis, 1984), Condition on Extraction Domains (Huang, 1982), among many others. However, the alleged "facts" that are accounted for only follow from an architecture that actually claims that elements "move". Take Systemic Functional Grammar, for instance (for a recent review, see Fontaine, 2012 and, for a more technical introduction, Fawcett, 2010). It is unlikely that problems of displacement-as-literal (feature-driven) movement arise in such a theory, where issues of locality are interpreted in a more cognitive-discursive way, in close relation to the metafunctions (ideational, interpersonal, textual) that configure the mellow of the theory. Our point is, simply, that what is taken as a "fact" in a theory is something that may not even arise in another. In order to claim that an element is interpreted in a different place from which it appears phonologically, we would have to resort to some kind of mechanism dissociating interpretation from phonology (already a strong claim) and then make that procedure explicit in logical, cognitive, biological or mathematical terms. What can be expected as "progress", then? Little if the polarization explicitly proposed by Katz & Postal (1991: 1 fn.1) is held as a "fact", dismissing as "non-credible proposals" alternatives which are equally consistent formal systems but depart from different axioms. This historical situation allows us to say that little progress has been done in linguistics, because of its essentially atomized character. Something like "progress" in Lakatos' terms could be said to have been done within this or that theory, in terms of empirical adequacy. However, contrarily to Chomsky's repeated claims, the fact that new questions are being asked does not entail that we know more about "language", simply that there has been a shift in focus. Chomsky (2012: 1) says that

"There has been remarkable progress in understanding language in the post-World War II period, over a very broad range, including the general principles that shape this highly special cognitive faculty, dissociated from others in many ways and unique to humans in essentials. One indication is that it was routine and reasonable for prominent linguists in earlier years to write books entitled Language. No longer. The task would be radically different today; far too much has been learned."

He seems to attribute the absence of books dealing with foundational issues to the fact that "far too much has been learned". However, outside Chomskyan linguistics, many object to that claim, including ourselves. The same can be held of any theory, however: it is not the fact that we know more that "prevents" linguists to undertake the enterprise of writing programmatic pieces, but mainly *dogmatism*. Within mainstream generative grammar (in both its conceptualist and realist versions), the foundational issues (like the existence of a "faculty of language" with its mental and physical aspects, or the very definition of "language", a very delicate issue Chomsky has refused to address beyond stipulative definitions) are considered to be solved and understood, and are thus taken for granted. The same happens in SFL, Cognitive Linguistics, and their respective sub-theories (HPSG, LFG, etc.). To undertake research at "peripheral" levels of the theory (e.g., testing empirical adequacy) does not mean or entail leaving "core" issues aside (e.g., the very definition of "language" we are working with): a permanent revisitation of both is, in our perspective, the only way to avoid self-centeredness and guarantee integration with the last developments in different disciplines (particularly those related to technology, like neurology or molecular genetics).

3.1 What is to be "discovered"?

Conceptualist generative linguistics has a curious concept of "discovery", and the ontology of the objects that qualify as "discoveries". The official vision is to be found, for example, in Pesetsky (2013), who lists alleged "discoveries" of generative syntax. An undeniable merit of Pesetsky's exposition is to provide an answer to a question often asked to Chomsky but that he seldom answers (see Behme, 2012 for discussion and examples), but his methods are at best arguable. The methodological assumptions behind Pesetsky's exposition are the topic of the present section.

To begin with, we consider that a crucial distinction has to be drawn between discovery and invention. To discover something is to notice and provide an account (descriptive, explanatory) of a pre-existing phenomenon, then subjected to further research. For example, electrons existed even before they were first noticed by scientists, in the late XIXth century. The existence of the electron is, and was, independent of scientists' awareness of it. Invention, on the other hand creates an object (either material of formal) which did not exist before the act of invention.

Now, let us take a look at the alleged "discoveries" of (orthodox Chomskyan) generative syntax Pesetsky lists:

- Hierarchical structure
- Case Theory
- Locality of Syntactic Relations
- Support for the central conjecture of generative syntax

Then, Pesetsky proceeds to "discuss" articles that contradict the aforementioned "discoveries". To the best of our knowledge, Pesetsky (and many more) have incurred in two mistakes:

a) A *historical* mistake
b) A *methodological* mistake

The historical mistake is simple enough: the effects that are observed have been studied for quite a long time now, before generativism and independently of its particular assumptions. Panini, for instance, presents a distributional theory of Case, based on morphology and semantic roles (or *karaka*), quite similar to the generativist "Theta theory" but without its added stipulations (government, subcategorization frames, etc.). Varro, in his *De Lingua Latina* also studied nominal and pronominal declension in detail, as well as the basic property of natural language from MGG perspective: recursion (from which phrase structure and hierarchy derive). Langendoen (1966) puts it the following way:

"(…) First he viewed the phenomenon of syntactic derivation in Latin as following a universal feature of human language: the ability to form an unlimited number of expressions (in fact, words) from a limited number of elements in a systematic fashion. Second, he justified this position on the grounds that if it were not true, then language acquisition would be impossible." (1966: 34)

The arguments in favor of locality (i.e., the relations between constituents are limited to certain domains, outside which relations result in ill-formedness: binding theory is a good and well-known example) within recent chomskyan generative grammar have unfortunately reduced to self-reference (something Boeckx & Grohmann, 2007 acknowledge) and forcing the data to fit the theory (e.g., chomskyan phases). There are some notable exceptions, which try to link linguistic phenomena to properties of other systems, thus deriving locality effects from more

general principles (e.g., Uriagereka, 1998, 2012), but these alternative approaches are overwhelmed by orthodox assumptions.

We think the historical mistake is sufficiently illustrated, but we can also mention the Port Royal Grammar and Logic (1660 and 1662 respectively), which argued for the mental entity of grammar and the fundamentally logical structure of natural language, which was to be argued for in Chomsky's early *Logical Structure of Linguistic Theory*[12].

The methodological mistake is somehow more serious: it implies that, if Pesetsky is aware of the references we have cited (and many others which also tackle these issues in the Greek-Latin tradition, as well as medieval studies with Aristotelian bases), he actually believes that generative Chomskyan syntax has provided evidence that proves that:

a) The invoked principles and rules are *necessary* conditions for a certain phenomenon to appear
b) The invoked principles and rules are *sufficient* conditions for a certain phenomenon to appear

Why is this relevant? Because scientific proof requires not only an account of why the relevant portion of the Universe is the way it is, but also *why it could not be otherwise* given certain parameters (mathematics tends to offer fine examples of such proofs). In this particular domain, the linguist (if concerned with methodological issues and willing to make a strong claim) must formally demonstrate that his formal procedure can productively generate the phenomenon in question (not only describe it, as it would be a mere reaffirmation of existence) and that either no other procedure can or, if there is an alternative way, that such a way requires extra assumptions or is in a specific and well-defined way less economical. Needless to say, this is not achieved in Pesetsky's talk, but, more worryingly, it does not seem to be of the concern of first-line linguists: the methodology is more inclined towards *assuming* something (e.g., the existence of FL/UG) and provide "evidence" in favor of that alleged "fact", which in turn results in the conclusion that the assumption is actually the case (by all means a circular reasoning). Counterevidence or alternative but equally consistent proposals are seldom discussed, and the basic properties of formal axiomatic systems (particularly, consistency and incompleteness, both intimately related to overgeneration[13]) are most frequently overlooked. This, we argue, goes against both theoretical and empirical development, since a theory must be restrictive enough to give interesting insight over natural language (or any other object) and it must be explicit enough to be subjected to the strictest formal scrutiny. As the reader may have noticed, the arguments we have been revising in this paper do not fulfill these criteria, or do only to a very limited extent. The "realist" and "conceptualist" positions thus limit themselves by their own rhetoric.

5. Conclusion and Methodological Warnings

In the preceding discussion we have analyzed two approaches to the foundations and ontology of linguistics. In this conclusion, we would like to make some methodological considerations regarding the apparent validity of the conceptualist-realist "discussion" (underlying more than

[12] Not to mention, for example, these kinds of claims: "*Legate's discovery that the left periphery of Warlpiri looks like Rizzi's left periphery for Italian (and Cable's for Tlingit) would have been the topic of an hour on NPR Science Friday*" (slide 72). The Left Periphery and the whole array of functional projections cartographic approaches assume did not pre-exist Rizzi's work (or, at least, it has not been adequately proven otherwise), consequently, it all falls within the realm of the "invention".

[13] If a formal axiomatic system contains every *p* and its negation ~*p*, it will be complete, but necessarily inconsistent. It will also be useless for scientific purposes, for it would have no restrictions.

just Chomsky's and Postal's personal stances, as we have seen), which has deep consequences for the ontology of linguistic theories.
Katz & Postal (1991: 515, fn. 1) claim:

"*We are aware that some philosophers and linguists think there are foundational positions distinct from nominalism, conceptualism, and realism. Although we cannot deal with this issue here, every such putative alternative with which we are familiar reduces to one of the three standard ontological positions*"

Crucially, their discussion is centered on conceptualism and realism, leaving nominalism aside. This polarization has the following logical consequence: if there is no other position (and their insistent ignorance of nominalism leads to this thought), then for every axiom or theorem *p* in theory A, theory B has a *~p* axiom or theorem by necessity, otherwise, the polarization thesis they need for their arguments to be valid (and convince the reader that rejecting their presentation of Chomskyan conceptualism inevitably leads to accepting their realism) would fall apart. If there are only two truth values, let us call them true and false (or 1 and 0, after all, it is the same as long as we have two discrete possibilities), then for every proposition that belongs to sets A or B, we could determine its truth value unambiguously. This is, of course, false: it is unlikely that either A or B would have only true propositions and the other only false propositions: a third alternative (C), composed by only the true propositions of A and B is logically necessary (that is, C is the intersection of A and B only containing true propositions). The definition of a method to determine the truth of the propositions that compose A and B, of course, is not provided by either Chomsky and his advocates or Katz & Postal and their supporters; moreover, the logical way out (i.e., build theory C) is a fallacy, insofar as there are really more than two theories (or stances), some internally consistent, some inconsistent; but all describing / explaining a *different* aspect of the object of inquiry; thus all epistemologically valid (with the demarcation criterion being internal consistency). An excellent summary of the allegedly exclusively binary debate we have been analyzing, and a fundamental flaw in its logical conception is enlightenedly put by Ross (1983: 3):

"*What is the matter with a pluralistic situation in which there are many approaches to the truth?*
My answer to this question would be: nothing is wrong. But that is an answer which seems to go against the mythology of science in which I was trained. I was taught to believe that for any two theories of the same domain [in our case, Realism and Conceptualism]*, A and B, there are only two possible logical situations that can obtain:*

1) *One of these theories is correct and the other incorrect.*
2) *These theories only* ***appear*** *to be different –* ***really*** *they are the same theory, wearing different terminological clothing. They are notational variants.*

I was not prepared to deal with a third situation:

3) *Each of the theories captures a fundamental part of the truth, but they are incompatible with each other, and neither can be reduced to the other.* ***Both*** *are necessary*" (highlighted in the original)

A further note is in order at this point (applicable to inter-theoretical criticism in general): notice that the alleged "incoherences" of the conceptualist position have been pointed out from a "realist" position, but not within conceptualism. In that case, we are not facing inconsistencies, but merely *incompatibility* of two different frameworks (Ross' situation (3)). A criticism to a framework is to be made in the very terms of that framework, as it is the only way of finding internal logical inconsistencies and proving that the theory in question is logically untenable.

There is no point in finding apparent contradictions in a theory from the perspective of another: it is at least a trivial enterprise, if not directly mislead.

What is more, we have provided some examples of propositions that belong to neither A nor B, but to a D alternative which is not made up from propositions of one or the other, but constitute a whole new system: Simpler Syntax; Survive Minimalism, Radical Minimalism, the CLASH model, among many others, are viable alternatives to both the binary system proposed by Katz & Postal and the unary system Chomsky proposes, dismissing all other alternatives as unreasonable or departing from the undefined concept of "virtual conceptual necessity". The proof that there exists at least a third and a fourth alternatives opens the door for more alternatives, all equally valid and consistent (and we are limiting ourselves to theories that make a statement as to the ontology of language, not to mention those theories that present an architecture of the grammar without asking for its place in the natural world, as most non-transformational models do): Katz & Postal's, and Chomsky's position are dangerously close to what Austin (1962) calls "the descriptive fallacy", in this case also involving a polarization of the market in terms of venues for alternative positions apart from a narrow scientific horizon. We hope this work helps de-polarizing the field of formal linguistics and create some awareness of the necessity of creating interdisciplinary bridges with mathematics, physics and biology (or the fields the reader feels closer to) without limiting the collaborations to the adoption of terminology or forcing concepts (like features) on thin ice.

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