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# Which Model of Biological Plausibility for Language?: The Case of “What Darwin Got Wrong”\*

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The goal of this paper is to discuss some of the conceptual consequences of the arguments put forward in *What Darwin Got Wrong*, for a broader theory of the biolinguistic approach. The book offers arguments against “New Synthesis” approaches to Evolutionary Theory, that are particularly germane to biolinguistic matters. One main contention is that only approaches to evolutionary facts that capture the “laws of form” observed across living organisms can be theoretically and empirically adequate. However, the book does not investigate whether this contention applies to linguistic matters as well. This issue is addressed in the paper, and it is argued that organism-internal properties, which can be captured via the formal notion of “conservativity”, must be found in language as well. Therefore, it is argued that only those lin-

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guistic theories that capture these properties, be they about syntactic, semantic or acquisition matters alike can be considered as biolinguistically plausible.

Keywords: biolinguistics, evo-devo, conservativity, Minimalist programme, internalism.

## 1. Introduction: The Problem of Biological Plausibility for Language

Jerry Fodor and Massimo Piattelli-Palmarini's joint effort, *What Darwin Got wrong*, presents a criticism of neo-darwinist approaches to Evolutionary Theory. This criticism is based on the "new synthesis" model, classic and contemporary alike (Mayr 1963; Dawkins 1976, 1986; among others). Fodor and Piattelli-Palmarini's (henceforth: "the authors") two main arguments against the new synthesis and its applications, as proposed in the book, can be informally summed up as follows. First, the new synthesis offers an explanatory model that works like Skinner's theory of *operant conditioning* (Skinner 1957). Since Operant Conditioning is inadequate as an explanatory tool, the new synthesis model is equally inadequate to account the data it purports to account. Second, the new synthesis model is logically flawed: it aims to explain data by resorting to principles that are claimed not to be part of the model (artificial selection). So, no putative "recalibration" of the model is possible, in order to amend these theoretical flaws. A new evolutionary framework is called for, the authors of the book claim.

Fodor and Piattelli-Palmarini (2008) does not sketch a fully defined alternative model. In this regard, the authors suggest that the evo-devo model of evolution offers a much more precise and reliable account of these processes ("Evolutionary-Developmental" model: Carroll 2005; Laubichler and Maienschein 2007; a.o.). At

its core, evo-devo is a field of biology that studies how developmental principles permit to trace a common genealogy of related organisms. This approach places a great emphasis on how organism-internal, developmental principles such as cell growth and morphogenesis can in turn define the “evolutionary space” of morphological traits of living organisms (Hall 2000; Prum and Brush 2003; a.o.). Hence, such an approach lends itself to a view of evolution as the result of how organism-internal processes can shape, even “re-arrange” the genetic material (or genotype) of organisms, to possibly result in novel “forms”, or phenotypes. For this reason, the authors argue, evo-devo offers an empirically more adequate rendition of evolutionary theory than the new synthesis model, at least in its most “radical” versions of this latter model. Conversely, since the new synthesis in its more radical instances attempts to reduce, perhaps explain away organism-internal principles to the result of external forces, it carries no empirical weight.

The book received several negative reviews, some of them using damning tones about these two key arguments and their import for biological research. Many reviewers criticized the book’s arguments as an ill-tempered and poorly informed attempt to “dismantle” one of the most influential paradigms in scientific research, Evolutionary Theory (Dennett 1995). For the most part, these reviews have dismissed the authors’ arguments as a futile philosophical exercise. A general claim is that the book is based on a poor understanding of finer-grained aspects of modern evolutionary theory including, among others, a misinterpretation of evo-devo as mutually exclusive with new synthesis models. Hence, its invitation to drop old models of Evolutionary Theory for new ones is unjustified (Block and Kitcher 2010; Pigliucci 2010; a.o.).

However, several other reviewers treated these arguments with a more neutral stance (Okasha 2010; Papineau 2010; Sober 2010; a.o.), others even with a positive one (Hornstein 2010). These reviewers concede that the authors do not take such an

extreme stance, as the authors explicitly state (Fodor and Piattelli-Palmarini 2008: ch.7). These reviewers also note the the authors highlighted several problems that the new synthesis model faces, *when* applied to fields other than biology. In particular, they note that the authors' arguments do outline actual explanatory flaws in frameworks such as evolutionary psychology, in particular in its applications to language. Evolutionary psychology accounts of cognitive phenomena have a tendency to reduce phenomena as diverse as social taboos, moral choices, decision-making processes, partner selection and several others to genetically-driven processes (Tooby and Cosmides 1992; Tooby, Cosmides and Barrett, 2003; Buss 2007; a.o.). In doing so, they place a great emphasis on the evolutionary advantages that certain psychological and social traits can have, in a given environment (e.g. altruism; Buss, 2007; a.o.). However, in doing so contradictory explanations seem to arise, to the point the harshest critics of the field consider evolutionary psychology accounts as “just-so” stories (Plotkin 2004; Richardson 2007; a.o.).

As these neutral reviewers concede, then, the authors' arguments may be quite appropriate, when, and *only when* turned against these more “heterodox” applications. While Fodor and Piattelli-Palmarini (2008)'s arguments seem to offer a strong case against evolutionary psychology, their relevance to matters of linguistic research<sup>1)</sup> seems less clear. In particular, although the authors seem to offer relatively precise arguments on how a biologically plausible theory of language could be defined, it is not clear how one could use these arguments to express such a theory. Two questions seem particularly stringent. A first open question is which strands of the *biolinguistic*

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1) I follow Di Sciullo and Boeckx (2011), and contend that non-minimalist strands of research on language and language acquisition fall within the field of biolinguistics (e.g. Tomasello 2003; Goldberg 2006). I agree, however, that these strands are quite far from the ones I would like to pursue here. I will discuss in some detail whether these proposals also “fall prey” to the authors' arguments, too.

*approach*, broadly conceived, are consistent with the Fodor and Piattelli-Palmarini (2008)'s main argument. A second, consequent open question is which parts of the *minimalist program*, which I will suggest as being the main strand of biolinguistics to be consistent with Fodor and Piattelli-Palmarini (2008)'s argument, offer an optimal level of “conceptual fitness” with these arguments.

Our goal in this paper is to answer these two questions. Hence, I want to verify whether the authors' arguments can also give us an important insight on which “parts” of the biolinguistic approach and, more specifically, of the minimalist program are consistent with the author's arguments. Therefore, in answering the first question I aim to show that the authors' arguments are a more appropriate account of bio-linguistic matters than the new synthesis ones. At the same time, I also aim to show that their arguments are more appropriate than the evolutionary psychology ones, although certainly not opposed to them. In answering the second question, I aim to show that only certain approaches within the minimalist program are compatible with the authors' arguments.

I aim to answer to these two questions via a two-step plan. First, I wish to discuss which strands of biolinguistic approach are consistent with the arguments presented in Fodor and Piattelli-Palmarini (2008). Second, I focus on the minimalist program, and discuss which strands of this program are better suited to satisfy the arguments at stake. I warn the reader that I take a broad perspective to what counts as part of this program, as I will also discuss semantic and psycholinguistic matters that are not usually included in it. However, I will explicitly spell out what I take to be their connection to minimalist matters. I will mostly argue that, if I take these arguments seriously, then these arguments offer us powerful theoretical tools for evaluation, on these two closely related goals. The remainder of the paper is geared towards reaching these goals, accordingly.

## 2. What Darwin Got Wrong: Relevance for a Biolinguistic Approach

The goal of this section is two-fold. The first goal is to offer a brief summary of the arguments, and their import for the biolinguistic approach (section 2.1.). The second goal is to discuss the import of the argument for three dimensions of linguistic analysis: syntax, semantics and language acquisition. I start with syntax (section 2.2.), proceed with semantics (section 2.3.), and conclude with acquisition (section 2.4.).

### 2.1. What Darwin Got Wrong: A Brief Summary and an Analysis of the Arguments

The goal of this section is to offer a brief summary of the arguments offered in the book.

The first argument offered in the book can be summed up as follows. The new synthesis model (henceforth: NS) is not a valid theory of evolution, because it is inconsistent with recent and not so recent discoveries in biology. The analysis of the evo-devo research program offered by the authors suggests that evolutionary processes are the result of rules of “well-formedness” on the combination of genes, the “internal” laws of form that govern the growth and development of organisms. If the authors’ analysis of these findings is correct, then I should take into account the importance of structure and self-organizing principles of genome, when proposing plausible evolutionary models. According to this view, then, evolution is first and foremost an endogenous or organism-*internal* process, and exogenous, on which organism-*external* processes (e.g. NS) can intervene on the *results* of this internal process. Evolution is also a process in which emergent properties play a key role. Both the *combination* of genetic material into more complex units, and the possible

*permutations* of this material into different structures may result into phenotypical variation. So, I should expect that such operations should have a linguistic counterpart that applies onto linguistic structures (e.g. sentences), as well.

The second argument can be summed up as follows. NS is usually defined as a process that can select life forms for their fitness with respect to the environment, as if it were akin to *artificial* selection. However, if NS can evaluate and select life forms with respect to their fitness in an environment, then it acts much like a rational, agent-based process, which is precisely what NS should *not* be like. NS appears to be inconsistent, since it encompasses two opposing principles working at the same time: random selection and non-random, “select-for” or agentive selection. As the authors discuss at length, if the environment acts as an agentive-like principle of selection, then by definition it cannot also be a random, “mindless” process at the same time. So, NS-based theories of evolution are inconsistent, and cannot explain the data they purport to explain.

The authors’ view of evo-devo is far from uncontroversial, and tends to create a tension between the internal, development properties of an organism and the external influences that can play a role on these processes (Fodor and Piattelli-Palmarini 2008: ch. 7). Another influential approach is that externalist, neo-darwinist oriented perspectives and evo-devo approaches can represent two complementary, but integrated aspects of a broader theory of evolution (Tooby, Cosmides and Barrett 2003)<sup>2</sup>). However, such approaches do stress the importance of putative principles of internal organization for biological and psychological processes, including language.

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2) I note that these approaches propose to reconcile evo-devo and NS as two sides of the same overarching evolutionary process. This reconciliation is possible if and only one drops one assumption of NS. *Only* external principles of organization determine the biological properties of organization. To our understanding, this is a “variant” of NS which drops the select-for principle, with its various consequences. Whether this assumption effectively leads to a distinct model of Synthesis, than the one the authors criticize, is a matter than I leave aside.

In doing so, they do share with Fodor & Piattelli-Palmarini (2008) the view that organism-external processes can operate on internal outputs, and in turn influence indirectly these internal processes (cf. Hall 2000). So, although I can maintain these two approaches to evo-devo as well as their import for linguistic matters are quite distinct, I can observe that they both suggest and support at least one flow of information that primarily goes from internal to external principles. Therefore, such a flow is opposed to that proposed by the NS model, which seems to only go in the opposite direction (external to internal). Based on this discussion, I wish to propose these two arguments of the following consequences, for our bio-linguistic discussion.

The first argument has one important bio-linguistic consequence. If organism-internal factors drive the evolutionary process and language evolution is part of these processes, then these factors should guide language evolution, too. Organism-internal factors should also play a key role in “synchronic” phenomena, both at a micro- (e.g. language processing and production) and macro-scale (language acquisition and impairment). If the driving forces of organism-internal processes are laws of form, controlled by specialized genes, then principles explicitly representing these laws of form should be at work in language, too. So, linguistic theories should assume that linguistic outputs (e.g. sentences) must adhere to principles of well-formedness, taken to be as purely internal principles of organization. External principles of selection, then, should operate on such well-formed outputs, rather than determine which are well-formed outputs. Conversely, if the first argument is false and organism-internal principles do not drive the evolutionary process, hence language evolution, then organism-external principles should drive linguistic phenomena. In such a case, the NS would offer an empirically adequate account.

The second argument also has an important bio-linguistic consequence. If organism-external factors play a role only after organism-internal ones have guided evolutionary or developmental processes, then such organism-external factors should

not be the theoretical kernel of a biologically plausible theory of language. Therefore, the second argument strongly suggests that a biologically plausible theory of language must be an *internalist* theory of language first and foremost, and not only an *externalist* theory (Asoulin 2007; Slezak 2009; Løndal and Narita 2010). Although externalist considerations may still play a role, they would do so insofar as they apply onto the output of internally-driven accounts of language. Conversely, if organism-internal principles do not drive the evolutionary process, hence language evolution, then organism-external principles should drive linguistic phenomena. In other words, if language can be accounted as purely environment-based principle, then the authors' arguments will turn out to be unfounded, and the NS arguments would be accurate.

These two consequences, when combined together, entail that the organizational principles found at a biological level, must also be found at linguistic level. If language is an expression of human biology, then language must have its own “version” of these principles. I consider this assumption an instance of *biological conservativity*, for the following reason. Language, as a “part” of our genetic endowment, must follow the principles governing our “whole” genetic endowment. To make this point formally explicit, I “exapt” the standard notion of *conservativity* for generalized quantifiers, found in model-theoretic semantics, into our broader discussion (Barwise and Cooper 1981; Heim and Kratzer 1998: ch.2; a.o.). Conservativity is formally defined as:

$$(1) D(A,B) \leftrightarrow D(A,A \cap B)$$

In words, if a set-theoretic relation between sets  $A$  and  $B$  holds, then it must hold between the first set and the more restrictive intersection of both sets (i.e.  $A \cap B$ ). A biological application can be formulated as follows. If a biological property  $A$  (having wings) is related to a property  $B$  (being a pig), then it must hold for the combination of these two properties, *if this combination does not define an empty property set*.

Since wings are incompatible with being a pig, the set of pigs with wings (i.e.  $A \cap B$ ) is empty. I am not aware if the authors had in mind this definition, too, but I think that possible conceptual divergences are negligible.

If these arguments are on the right track, then their import for linguistic theories can, and perhaps *must* be evaluated with respect to their ability to be consistent with the authors' arguments, *qua* biological principles applied to the sub-domain of language. The reason is as follows. I assume that linguistic properties are a part of biological properties; I represent the set of linguistic properties as  $L$ , and that of biological properties as  $BI$ . I take that one specific type of relation, the *sub-set* relation " $\subseteq$ ", can represent the relation between these sets (i.e. I have  $L \subseteq BI$ ). A logical consequence of this assumption is all the properties that apply to the set  $BI$ , including conservativity, monotonically apply to each of its sub-sets, taken as possible *restrictions* of this set (Landman, 1991: ch. 2). In other words, I suggest that conservativity is a reflection of more general principles of biological organization<sup>3</sup>).

A less formal and more intuitive approach to this assumption is as follow. I expect that a biologically plausible theory of language must also be an internalist theory, which also implements a language-specific notion of conservativity. The reason is simple: if language is an expression of our internal biology, and our biology has the conservative property, then language must have the conservative property. Language as a mental phenomenon, as the authors indirectly assume, must make sense with our knowledge of the brain, and our biological properties. In this regard, the authors' arguments perhaps surprisingly converge with theories apparently distant from their position, such as *embodied cognition* (Lakoff and Johnson 1999; Evans 2010).

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3) This suggestion, in turn, implies that conservativity could be seen as a "third factor" property, a property that is not specific to language alone, but rather to any biological, dynamic system that involve "computational" processes. I do not explore this topic further, as it is not crucial for our discussion. See Chomsky (2005); Trozke, Bader and Frazier (2013); for discussion, among others.

I do wish to claim that such conceptual convergence would be unproblematic or free from controversy, but merely observe that this conceptual result nevertheless originates from different theoretical assumptions. I do not discuss this aspect more in detail, as it would lead us too far afield. What matters for us, then, is that if this assumption about biological conservativity and its extension is *not* correct, then I would expect that the NS model, instead, can give an accurate account of linguistic phenomena. If linguistic properties do not depend on (i.e. are not part of) our biological properties, they should share any features as well. The next sections discuss this hypothesis, and its validity, in detail.

## 2.2. Biologically Plausible Theories of Language: Syntax

My goal in this section is to discuss which syntactic theories meet the requirements for biological plausibility that the authors present, and that I discussed in the previous section. Since I take a broad perspective to the Biological Approach to language, I will discuss a broad set of theories, including externalist ones.

I start from those theories that could be labeled as having an organism-external perspective to sentence structure. Most, if not all of these theories contend that there are no language-specific principles by which distinct constituents are combined together by internal auto-organizational properties. Some examples include “construction-” or “usage-based grammars” (Goldberg 1995, 2006; Croft 2001; Tomasello 2003; a.o.); or the “simpler syntax” model (e.g. Culicover and Jackendoff 2005). Construction Grammars, for instance, assume that “grammar” is the result of the artificial selection of an external process, the communicative intentions of speakers. Certain proposals explicitly argue that grammars can be inherited as cultural artifacts, via the selection process that “society”, as a certain type of environment, places on them (Sterelny 2003; and references therein). The specific problems with these

theories can be stated as follows.

The strong conceptual isomorphism between NS and the first group of theories suggests that these theories also appear to be logically inconsistent. These accounts focus on external factors such as “pragmatic principles”, very broadly construed and defined. So, they seem to lack mechanisms that account for *why* one can distinguish between well-formed and ill-formed sentences, one case being syntactic islands (Goldberg 2006: ch.7). In other words, these theories cannot account for the ungrammaticality of examples such as (2)-(3):

- (2) \*Who what thinks that Mario ate?
- (3) Who thinks that Mario ate what?

One counter-argument could be that some pragmatic principles could rule out the sentence in (2) but not that in (3), at least on putative grounds. One may assume that some native speakers found problematic, during some phase of evolution and from a communicative point of view, to have two sequenced *wh-* words in sentence-initial position. I leave aside the question on *why* such a principle should be found in English but not Japanese, or other languages in which these structures are perfectly grammatical (Hornstein, Nunes and Grohmann 2006: ch.5; and references therein). One obvious problem with this account is that native speakers would need to *select* (3), rather than (2), *for* an external purpose such as some stylistic, culturally received standard of word order. The acceptable syntactic structures observed in a language would stem from language-external principles of selection. Furthermore, such accounts would not explain, at least not straightforwardly, why speakers would reject ungrammatical sentences. Externalist, NS-based accounts of language seem to fail at accounting basic linguistic patterns that are *predicted* on internalist accounts. Therefore, both the first and the second argument seem to rule out Construction-based accounts, as biologically plausible accounts of language, let alone display a form

of conservativity in their analysis.

I now turn our attention to those theories that do take an internalist perspective to syntactic matters. I start from non-minimalist theories, which to an extent do not place a great emphasis on biological or evolutionary plausibility (if any), at least in our understanding. Examples of such theories that I briefly discuss are HPSG (Head Phrase Grammar: Sag, Wasow and Bender 2003; a.o.); and LFG (Lexical Functional Grammar: Bresnan 2001; a.o.), and similar others. A common thread or *fil rouge* that connects these theories with minimalism is that they both take syntactic structures to be the result of the combination, or *Merge* of basic lexical items (words or *signs* in HPSG). These combinatory processes can, in turn, define or license grammatical and syntactic relations among constituents, as in LFG (“c-structure” and “f-structure”, respectively). Given these basic definitions of how the syntactic structure of sentences can emerge from more basic processes of sentence generation, both approaches seem conceptually consistent with the authors’ arguments, as they take an inherently internalist stance. The well-formedness of sentences is analyzed as a purely linguistic matter, so these theories aim to account for and predict linguistic data without any appeal whatsoever to external factors, in the sense discussed here<sup>4</sup>). Furthermore, syntactic structures are treated as the result of a combinatoric process that merges lexical items together, as if they were strands of “lexical genomic code”.

However, these theories present certain explanatory gaps when certain types of syntactic phenomena are discussed, as they assume that sentence structures are “mono-stratal”. Syntactic structures come in a prepackaged order of functional heads

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4) Both HPSG and LFG, and minimalist theories on the other side suggest that certain phenomena are “external” with respect to *Syntax*, as a distinct module of language. The reason is that these phenomena can only be explained when one looks at the relation between syntax and morphology, as in the case of agreement patterns. From our perspective, these “interface” phenomena are still within an internalist take to language.

and structures, and no “permutation” principles can be found at work in the generation of sentences. One peculiar consequence of this approach is that these theories face non-trivial problems in accounting simple forms of *inversion* or *fronting*, such as locative inversion in (4)-(5):

- (4) Behind the car, the boys were playing cricket
- (5) Into the room a unicorn came

The examples in (4)-(5) involve a prepositional phrase that is moved (or copied) into sentence-initial position, as a topic or as a subject respectively (den Dikken 2006: ch.1; a.o.). Both cases are problematic for these theories, as they do not contemplate any operations that allow syntactic structures to “diverge” from a canonical order. More accurately, these theories do not contemplate that syntactic processes can re-arrange sentence structures, although they must preserve the properties of the re-arranged structures, in doing so. One apparently far-reaching consequence of this assumption is that these approaches seem to lack a permutation principle that is pervasive in other biological domains and structures, as the authors suggest. Biological laws of form do not simply define how structures, biological and linguistic alike, are organized (or “ordered”), but also how they can be re-organized, without losing any of their properties as a result. Sentence structures can be re-arranged and permuted, and new structures can be obtained via this process of permutation, in a manner entirely parallel to genomic “strings”. However, in HPSG and LFG this permutation process is not explicitly represented. So, these approaches seem on the right track, from a biological plausibility point of view, but also seem to be missing some expected key property.

Once we move within the minimalist program, we need to make similar distinc-

tions, given the Lakatosian nature of this program<sup>5</sup>). A certain tension exists between approaches that define themselves as a substantial departure from its predecessor framework, the *Principles and Parameters theory* and those approaches that consider it a natural evolution of Principles and Parameters theory (e.g. Balari and Lorenzo 2009; Bénéitez-Burraco and Longa 2010; Hornstein 2009; respectively). As Bénéitez-Burraco and Longa (2010) observe, one central tenet of Principles and Parameters theory is that the language faculty can be thought as a realization of a rich genetic program. The differences between languages amount to the setting of “switches” or parameters. Variation, specifically cross-linguistic variation, may be seen as the result of fixing these parameters, and thus may be subject to external pressures and contingencies (e.g. Chomsky 1981: ch.1). So, proposals based on this theory take a static, but bi-dimensional view to syntactic structures, with some important departures from a purely “sequential” approach.

One framework that follows this tack is the *cartographic approach* (Cinque 1999; Shlonsky 2010, a.o.). This approach places a quite strong emphasis on the rigidity of syntactic structures, since it assumes that syntactic structure come in a fixed order, the *Functional SEquence* (FSEQ). In some incarnations, this approach is not limited to syntactic matters alone, but probes down to “atomic” morphological structures, hence assuming that linear order is pervasive in the grammar (as in *nano-syntax*, e.g. Starke 2009). Therefore, syntactic structures may involve very long “sequences” of functional heads, down to the smallest morphological bits.

One obvious conceptual problem with this perspective is the following.

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5) Informally, a “scientific program” is a research agenda that includes several approaches to a common domain of inquiry, according to Lakatos’ classical analysis (Lakatos 1978). These approaches may vary in consistent amounts with respect to the principles they adopt, and in some instance they may only share certain core principles. These approaches may otherwise adopt quite different if not downright conflicting principles; the data will adjudicate which approach is on the right track.

Cartographic approaches reduce the role of *Merge* as a structure-building operation to a bare minimum. According to the FSEQ assumption, syntactic units are merged in a pre-defined way (Shlonsky 2010; a.o.). So, the role of *Merge* in the generation of sentences is reduced to its basic combinatoric property. Although most variants of the cartographic approach assume that the related operation *Move* can permute syntactic elements, the range of this operation is subject to rather specific constraints. When movement occurs, syntactic units move to “higher” positions in the clausal spine that attract the features of the moved constituents. Examples (2)-(5) would thus involve the operation *Move*, which would permute the target *wh*- elements and prepositional phrases to a “non-canonical” position. Therefore, Principles and Parameters approaches fall in what I could perhaps call a “bi-dimensional space” of biological complexity. While they assume that operations of permutation are possible, they also assume that these operations range over a fixed sequence of linguistic units. This aspect places these proposals within a gray theoretical space, since they include certain conceptual “remnants” of the Principles and Parameters approach, most notably the “sequencing” of building units. However, versions of cartography and nano-syntax seem to overcome this problem, if only in via rather fine-grained theories of movement (Svenonius 2007; a.o.). So, they seem to converge to a fully biologically plausible model of syntax, if only via a set of complex principles of permutation.

Several other approaches take a different stance, in which combinatorial processes take a central role. The approaches are explicitly centered on *Merge*, the central syntactic process that builds up larger lexical items recursively. Examples include frameworks that are different one another, but that take the centrality as a starting point for syntactic derivations. Examples include the “lexical syntax” of Hale and Keyser (2002); introductory works such as Hornstein, Nunes and Grohmann (2006); but also the “combinatorial categorial grammar” of Steedman (2000, 2012). These approaches attempt to model how sentences “emerge”, via the interaction of basic

linguistic atoms and precise “sentence-folding” operations. So, these approaches contend that the examples in (2)-(5) can be seen as the result of the iterated application of Merge and Move, often considered as a variant of Merge. These operations apply to sets of lexical items (e.g. *into, the, room*, etc.), of which the output is a sentence such as (5). These approaches do not postulate any other organizing principles than the rules of sentence formation applied to lexical items as “building blocks” of sentences, and derive the well-formedness of these sentences as a result of these combinatoric principles. In doing so, such approaches indirectly meet the goals of biological adequacy I discussed so far, as they place emphasis on (sentence) structure-building as a simple, “blind” process that generates regular (syntactic) forms.

As matters stand, then, formal theories both within and outside the minimalist program offer an internalist approach to syntactic matters. Hence, they can be seen as biologically plausible theories that are consistent with the authors’ arguments, unlike construction/usage-based grammars. However, these internalist theories form a conceptual cline, in which only a certain sub-set of minimalist proposals seem to fully meet criteria of biological plausibility. While HPSG and LFG seem to only include a combinatoric principle (Merge) in their theoretical apparatus, the cartographic approach only a permuting principle (Move), other approaches seem to include both (e.g. categorial grammars). I leave aside a more fine-grained analysis of these differences, and turn to semantic matters.

### **2.3. Biologically Plausible Theories of Language: Semantics**

Our goal in this section is to discuss biologically plausible theories of Semantics that are consistent with the syntactic considerations I have offered in the previous section. Again, given our broad perspective on the Biological Approach, I will cover several such theories.

Any considerations about the psychological and biological plausibility of semantic theories touch a rather thorny issue. Recent analyses in the literature observe that standard “philosophical” approaches take an externalist position to meaning (Asoulin 2007; Pietroski 2005, 2008; Hinzen 2006, 2007; Løhndal and Narita 2010; Slezak 2009; a.o.). Externalist approaches, in this specific context of discussion, are those approaches that focus on how a semantics for natural language should define truth-conditions that pertain to facts in the world. Context- or society- bound conceptions of meaning and the interpretive process are the norm (e.g. Montague 1973; Dummett 1986; Cresswell 1985: ch.2; Stalnaker 1999; a.o.). These works assume that meanings are social constructs that arise from the interaction among individuals. According to this general approach, then, the “environment” as a broadly construed concept plays a crucial role, in the interpretation of sentences, and the identification of their meanings.

Other externalist approaches to meaning take an even more explicitly “evolutionary” stance to the emergence of meaning. Within the philosophical tradition, certain proposals clearly connect meanings, hence semantics, to evolutionary processes of cultural inheritance. Examples include Dawkins’ “memes” (Dawkins 1976, 1986; Blackmore 1999); Millikan’s “biosemantics” (Millikan 1989, 2004); and other varieties (Dennett 1995: ch. 18; Sterelny 2003; a.o.). The accounts of the processes by which these meanings are acquired and used are usually imported from evolutionary psychology, with several shades and nuances. For these reasons, these approaches to semantics face several philosophical and, most importantly, empirical problems (Løhndal and Narita 2010; a.o.). Several basic patterns of natural language interpretation appear mysterious, if one considers them as driven by the need to be evaluated with respect to external context. Basic semantic properties of lexical items and sentences (e.g. antonymy, synonymy, polarity, implicatures), appear as too hard a challenge, for stimulus-driven accounts. Some very basic examples that fall outside

the explanatory power of such theories are our examples in (2)-(5), but also the following examples:

- (6) Mario has sold the Vespa to Luigi
- (7) Luigi has bought the Vespa from Mario
- (8) Mario went to the pub
- (9) Mario was at the pub

Examples (6)-(7) display an antonymous relation that holds between the Verbs *buy* and *sell*. These sentences can be true in the same external context, as the same individuals (Mario and Luigi) may be involved in the same commercial exchange event. However, these sentences describe the same external event under two different “internal” points of view. Roughly, Mario’s point of view is expressed in (6), while Luigi’s point of view is expressed in (7). Note, furthermore, that the prepositions *to* and *from* also contribute to capture this distinction in points of view, in a basically compositional way. So, externalist approaches would face a problematic challenge in modeling the notion of “perspective” that underpins antonym relations when defined over verbs and prepositions. Possibly, they would also face this challenge with other parts of speech as well.

A similar challenge would also emerge from examples (8)-(9), amply discussed in the literature on events at least since Parsons (1990). Intuitively, once the event of going to the pub described by the sentence in (7) is over, Mario ends up being at the pub, as a consequence. The relation that holds between the two sentences is one of *entailment*, since the interpretation of one sentence (i.e. (9)) can be seen as a logical consequence of the interpretation of another sentence (i.e. (8)). One possible externalist analysis is that speakers could infer that the two sentences describe related sets of events, taken as describing extra-linguistic scenarios (Millikan 2004; a.o.). Speakers could accept these sentences as being “true” with respect to extra-linguistic factors.

If I observe that Mario has gone to the pub in some earlier moment, I can conclude that Mario is at the pub as a consequence of this event of motion. Therefore, speakers can access their logical relations as part of their ability to reason with past and current (observed) events.

One obvious problem with this inferential approach is that speakers can accept such entailments even if no extra-linguistic context is offered. The prepositions *to* and *at* denote certain possible spatial relations that hold among individuals, irrespective of their actual realization in an extra-linguistic context. That is, they represent logical/lexical relations as relations of interpreted terms with respect to a *model of Discourse*. A model of Discourse, by definition, is distinct from the object(s) it purports to model, whether it is a given context or a fully spelled out model of the environment (Landman 1991: ch.2-3; Hinzen 2007; a.o.). Therefore, if one takes an internalist stance to semantic relations, then one should not necessarily assume that these logical/lexical relations can but need not to represent external factors. A similar reasoning can be applied to several, if not all (spatial) prepositions in English, again as discussed in the literature (Parsons 1990; Fong 1997; Zwarts 2005; a.o.).

So, their lexical relation guides the entailment relation that holds between the sentences they partake in, as well as speakers' intuitions about these sentences. More in general, these approaches would probably encounter a challenge from *any* lexical relation, as a relation representing the *internal* properties of a model of Discourse, and the lexicon it purports to represent<sup>6)</sup> (Pietroski 2008; Løhndal and Narita 2010; on this topic). But before focusing on whether I can actually find internalist approaches to this notion of a model of Discourse, I make some *interim* conclusions on the biological plausibility of these aforementioned approaches.

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6) This problem emerges regardless of what perspective I take on the precise nature of the Lexicon, so I will be silent on this specific theoretical matter.

From a philosophical point of view, the authors' arguments offer a severe challenge to these externalist approaches to semantics. Descriptive, pre-theoretical proposals such as Dawkins' memes are ultimately extensions of OT, evolutionary psychology or a combination of both. For these reasons, they both fail to meet the requirements for biological plausibility that the authors' argument outline. As extensions of OT, they will never be able to offer correct explanations of semantic data, but rather false or, at most, empty ones. Philosophical perspectives on semantics are so close to standard OT/evolutionary accounts, that they may as well as be considered the authors' ideal targets of criticism. In other words, the systematic reliance of external factors and a NS-like account that these theories employ, provides the chief obstacle to a correct analysis of data, for these theories.

While the import for these externalist approaches to semantics should be clear, a different picture emerges once I move within the domain of linguistic approaches to semantics. In this domain, model-theoretic semantics have been employed by scholars with quite different perspectives. Classical and modern model-theoretic perspectives place a strong emphasis on semantic derivations and processes. However, these perspectives tacitly consider model-theoretic semantics as attempting to model facts in the world, rather than mental representations thereof. Some modern variants known as "dynamic semantics" explicitly consider the process of interpretation as a way to modify what information I share, in the extra-linguistic (external) context (Groenendijk and Stockhof 1991; Stalnaker 1999; a.o.). Consequently, the model of Discourse that emerges from these theories is an external one, in that it models our use of meanings of utterances in context. So, these theories also fall a within the range of both arguments, as being inherently externalist.

Not all semantic theories, however, take this perspective on models, so some distinctions are necessary. Model-theoretic semantics can be approached as the result of evaluating syntactic structures and their interpretation against a model of Discourse.

This intermediate syntactic step can be skipped: words and sentences are interpreted directly, against a model. The first approach usually has the Fregean property of *compositionality*, while the second usually lacks it. Since I am assuming that a biologically grounded faculty of language coincides with derivational processes, I also suggest that non-compositional theories miss an important generalization. If sentences are the end result of syntactic processes merging together basic lexical units, then the interpretation of these sentences against a mental model of discourse can only be the result of combining together the single interpretations in a coherent structure. Furthermore, if semantic relations among sentences can be defined, then these relations must reflect the semantic relations among the units they include, in their structures. The interpretation of sentences such as (6)-(9), and their relations of entailment and antonymy must emerge as the compositional result of combining Items such as *to* and *from*, or *buy* and *sell* together. Extra-linguistic factors should play no relevant role, in an analysis of these processes.

Several theories, being non-compositional in nature, fail to meet this requirement. Examples of such theories are the “mental models” theory (Johnson-Laird 1983, 1992); “conceptual semantics” (Jackendoff 1983, 1990); “cognitive semantics” (Talmy 2000). Other non-compositional theories can also be included, such as certain incarnations of discourse representation theory (Geurts 1999; a.o.), or approaches to syntax and semantics centered on optimality theory (e.g. Kager 1999). Without an explicit level of syntactic generation, I cannot have a level of semantic evaluation, as no account of how I obtain structures is offered. I am not sure whether these theories fall prey to the authors’ arguments or sit in a theoretical gray zone. However, I think that their perspective is not entirely germane to the authors’ arguments, for the reasons I listed so far. Without a principled account of how words’ meanings interact, their semantic relations can only be postulated, rather than explained in an accurate way.

Several other theories, however, attempt to spell out compositional and model-theoretic approaches (Heim and Kratzer 1998; Chierchia and McConnell-Ginnet 2000; a.o.). A distinct case must be made for semantic theories which may be implemented with minimalist approaches, other than the cartographic ones. For instance, Pietroski (2005, 2008) appears to offer such a theory, since Pietroski's proposal explicitly aims to integrate a (variant of) event semantics with minimalist-oriented syntactic assumptions. However, one might also integrate the minimalist program with theories that allow flexible syntactic assumptions. I think that canonical versions of discourse representation theory can be such theories, given their syntactic neutrality (see e.g. Kamp, van Genabith and Reyle 2005). Other avenues of research that share this perspective are also possible. Categorical approaches such as Steedman (2000, 2012) derive semantic interpretations from syntactic structures in an isomorphic (one-to-one) fashion, a principle known as the *Curry-Howard* isomorphism. So, they propose a compositional, transparent relation between syntax and semantics, one which is maximally explicit in how one computes meanings of sentences from the meanings of words. A similar proposal is made in Andrews (2004), which discusses the possibility of explicitly defining a model-theoretic semantics approach with a strictly mentalist, internalist perspective. The core difference with other proposals is that LFG is taken as the (implicit) syntactic framework of choice. I leave open on whether this latter choice crucially determines the biological feasibility of Andrews' proposal. However, I think that Andrews' proposal also captures an important generalization, that insofar as a theory aims to compositionally model the meaning and relations among words as part of a mental lexicon, then a semantic theory can be consistent with the authors' arguments. In doing so, as I discussed so far, a semantic theory is also able to explain basic semantic data, since they offer evidence that internal properties of language play a key role.

An important point that I must discuss as a distinct topic is the following. As all

of these model-theoretic theories include language-specific (one would say: meaning-specific) definitions of conservativity, they appear to explicitly capture this consistency in their apparatus. It is true that standard model-theoretic approaches to semantics implement a notion of conservativity that was originally devised to capture the semantic properties of generalized quantifiers (Barwise and Cooper 1981; onwards). Our exaptation of this notion may be a useful conceptual tool, but not necessarily a correct one. In this regard, it seems a bit of a stretch to suggest a *direct* logical connection between the properties of the quantified noun phrase *every man*, and the wing-less nature of pigs. However, semantic conservativity is defined as a property that restricts the properties of generalized quantifiers in their *interaction* with other parts of speech. Furthermore, the generalized quantifier approach can be extended, with minor *provisos*, to adverbs and other parts of speech that can denote relations among events (Bach, Jelinek, Kratzer and Partee 1995, a.o.). Therefore, its conceptual import for our discussion turns out to be quite vast, empirically speaking. The examples in (10)-(11) allow us to clarify this point in detail:

- (10) Every man walks in the park
- (11) Mario always walks in the park

Conservativity predicts that the interpretation of (10) can be roughly paraphrased as: every man in the domain of discourse is a man who also walks in the park. It also predicts for (11), although a slightly different level of representation, that every event in which Mario walks is also an event in which Mario walks in the park. Both determiner- and adverbial-quantification obey to the same underlying principle, conservativity. In other words, conservativity predicts that quantifiers and other lexical items must combine according to certain properties and principles of consistency. It also predicts that lexical items that fail to enjoy this property will render sentences ungrammatical, or structurally ill-formed. Perhaps such Lexical Items can be freely

conceived and used in different types of external contexts, but their existence is based on the “design” of speakers, rather than on the natural properties of language (Crain and Pietroski 2001; a.o.). Once I take into consideration the fact that, at a certain level of understanding, quantification is a pervasive phenomenon in natural languages, one fact should become obvious. The internal organization of a mental model of Discourse plays a much more vital role in possible meanings, than any environmental constraints, as it offers a more immediate constraint on what can be interpretable sentences.

So, if I can spell out a semantics for natural language sentences, which is transparently mapped from its syntax, and that also enjoys the conservativity property, then I can make rather explicit claims about the biological consistency of our theories. Our grammars must produce well-formed *and* interpretable sentences, *before* these sentences can be even evaluated with respect to an extra-linguistic context. Approaches that do not take into consideration this basic fact about semantic interpretation would simply fail to be consistent with the authors’ main argument, as they would lack a language-specific form of conservativity. Consequently, they would fail in being able to account for a pervasive phenomenon of natural language semantics, a rather unwelcome result<sup>7)</sup>.

An open question that emerges from this discussion is whether I could define an integrated and biologically plausible theory of syntax, semantics and their interface. However, I think that conservativity gives us an important clue on how this interface should be defined. If one combines a biologically plausible syntax with a biologi-

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7) In several minimalist approaches, a systematic distinction is made between interpretable and uninterpretable features, a distinction which is used to account for agreement phenomena in a variety of syntactic context (Hornstein *et al.* 2006: ch. 7-9; a.o.). I leave aside a discussion on how these notions are related to the model- theoretic ones, as I think that solving such a complex theoretical problem would lead us too far afield from our topic of discussion.

cally plausible semantics, then the result should indeed yield a biologically plausible syntax-semantics interface. Given the considerations I offered in the previous section, this biologically plausible, integrated approach should take the form of an internalist grammar. I take an internalist grammar to be one that builds sentences as mental objects that, in turn, denote (structured) mental representations, or “meanings”, which stand in certain internal (lexical) relations (e.g. entailment). Perhaps, in such a scenario these semantic relations could be interpreted as further dimension of linguistic organization, to an extent ancillary to the dimensions of syntax. I leave aside a more thorough discussion of these complex matters, and focus on more empirically-oriented topics. For this reason, I turn our attention to acquisition matters.

## 2.4. Biologically Plausible Theories of Language: Language Acquisition

The goal of this section is to discuss theories of language acquisition that meet the authors’ arguments for biological plausibility. There is an ample consensus that acquisition problems, *qua* psychological problems, represent a clear case in which psycho- and bio-logical factors must play a vital role in linguistic explanations. So, I think that our choice is quite germane for our discussion on the biological plausibility of linguistic theories.

If the syntactic and semantic considerations offered in the previous sections are correct, then the arguments put forward by the authors appear to be particularly relevant for experimental approaches to linguistic data. This entails that the validity of language acquisition theories can be also evaluated with respect to their ability to appropriately match biologically plausible theories of syntax. Therefore, acquisition matters can be discussed from a similar point of view to that I applied to syntactic theories. Within this field, I can make a broad distinction between theories that can

be more or less reduced to evolutionary psychology theories, and those that are closer (or within) the minimalist program. For instance, construction-based models of language acquisition contend that the acquisition process consists, to a large extent, in the child selecting which syntactic constructions are present in a language. According to these theories, then, a child acquires a language via the directed, guided input of adults surrounding him. Such models often make explicit their reference to neo-darwinist positions (Tomasello 2003: ch.1; Goldberg 2006: ch.2; a.o.) and, in doing so, they define their hypotheses about language acquisition processes as being conceptually isomorphic to NS. Consequently, the logical and empirical fallacies appearing in NS are a problem for these theories of language acquisition, too.

These conceptual problems can be found, although in different forms, in acquisition theories that share the same dearth of “dimensional complexity”. For instance, in generative approaches to language acquisition, the Principles and Parameters view of syntactic structures is considered the norm. Consequently, the child’s task of acquiring a language roughly coincides with the task of discovering the “correct” parameter settings in her language, on a “Head by Head” basis (Guasti 2002; a.o.). This approach is indirectly consistent with the authors’ arguments, since these works offer a view of language acquisition which may be said to “bi-dimensional”. If one takes in consideration that the “Head by Head” model of acquisition acts as a counterpart of the Cartographic approach, then one can easily realize that his model falls within the same conceptual “gray space” of cartography. Part of the structural complexity that a child should “discover” about a target language is, in a sense, under-represented in this model. In particular, it is not clear whether a child would actually acquire Merge as a syntactic principle, since the acquisition task would in part reduce to two more basic processes. One would be acquiring a fixed word order for sentences, the other would be mapping such order to the one found in the target language, via movement operations (cf. Longa and Lorenzo 2008).

The general problem that seems to emerge from this discussion is the following. If I take a biological perspective to language acquisition in children, then the authors' arguments can apply to the acquisition problem as well. This means that a biologically plausible theory of language acquisition cannot be a theory that hinges on external factors driving acquisition. Of course, children require the external input of other speakers, so that their mental grammar can (and must) store this information, to "grow" over developmental time. However, this process of "internalization" of these external inputs is likely to be guided by the specific properties that also govern adult-like language production and comprehension.

Theories of language acquisition such as Crain and Thornton (1998) or Yang (2002) seem to provide an approach which is very close to these assumptions, hence germane to the authors' arguments. The key intuition is that in these theories of language acquisition, a child acts as a "conservative" learner. The child starts with the ability to access basic properties of grammar, which allow the child to understand the underlying sentence structure of utterances, and their possible interpretation in a (mental) model of Discourse. New sentences and possible interpretations, according to these models of acquisition, must be consistent with the structural properties that a child can access from grammar. So, a child can only acquire new sentences, e.g. (2)-(11), as long as they can be parsed as the result of precise syntactic processes, which in turn result in corresponding semantic interpretations. While this process can be subject to constant updates and "recalibrations", such dynamic processes result in expanding the child's grammar and model of Discourse in a structured, internally principled way. Other approaches, as the authors suggest, would simply lead to models of acquisition that cannot account for basic acquisition data across languages.

One important aspect of this discussion is the following. In making this conjecture about plausible models of language acquisition, I assume that conservativity plays a role in this "dimension" of language as well. Our argument is as follows. If the *result*

of the acquisition process is a biologically plausible and internal grammar, as the one I assume for adults, then this result must come from a process which is also biologically plausible. Given that I take the authors' arguments as our guide, a biologically plausible acquisition process is a process that preserves the internal organization of grammar, while allowing grammar to build up a "bigger" set of lexical items. This process should also build up grammatical relations among these items (e.g. whether they are syntactic or semantic) that a child learns when he learns a language. If one takes seriously the authors' argument for acquisition matters as well, then one would expect that the structural properties of grammar (*qua* a biological structure) remain the same, although the "size" of the grammar grows over time.

Such principles are deeply embedded in all the theories based on minimalist assumptions that I reviewed so far, although they seem to be the main guiding principle in Crain and Thornton (1998) and Yang (2002). Other theories such as usage-based models of acquisition, instead, fall squarely within the conceptual pitfall of being "empty" explanatory accounts, given their reliance on external principles of acquisition.

Before moving to the conclusions, I briefly sum up and discuss our analysis of the authors' arguments, and their import for the biolinguistic approach. It may come as no surprise that the authors' arguments offer a criticism that can be extended to several linguistic theories which are indirectly incompatible with the biolinguistic approach. The authors' arguments can also offer a relatively precise account on why these theories are likely to be "false" or, at most, "empty". It should also come as no surprise that these arguments do not bear relevance only to syntactic aspects but to semantic and acquisition aspects as well, since these arguments outline the problematic nature of any theory (linguistic or not) that fails to account for the dynamic, derivational nature of linguistic processes. However, it is an open but not so clear question whether their arguments support the whole spectrum of minimalist

approaches, or are perhaps consistent only with a certain sub-set of proposals that are part of the minimalist logical space. I shall briefly discuss this topic in the remainder of this section.

Again, as the authors observe, the view they offer in the book about the evo-devo program does not necessarily represent the received wisdom regarding the theoretical import of this theory. Rather different accounts exist, and may be considered the majority view in the field (Tooby, Cosmides and Barrett 2003; for discussion). However, if the authors' argument and, consequently, our analysis are on the right track, then only a certain type of minimalist approaches to syntax-semantics and their psycho- and bio-logical assumptions are on the right track. Approaches that are not entirely consistent with the authors' arguments encounter empirical problems that are easily accounted for, in accounts that are consistent with these arguments. At least for language, the majority view seems mistaken.

Although I leave open on which specific theoretical proposals this internalist approach is made of, its core properties should be now clear. An integrated minimalist approach that aims at being consistent with the authors' arguments *qua* accurate assumptions about biological data, must model language as a very precise internal system. So, it must capture in a precise way the general "laws of form", as they are instantiated in language. Other models, in particular externalist ones, won't be equally empirically adequate, both at a linguistic and biological level. I now turn to the more general conclusions of this paper.

### 3. Conclusions

Jerry Fodor commented after the book's publication that he would have been placed on the FBI's witness protection program, since *What Darwin Got Wrong*

would have likely caused deeply negative reactions. After all, Darwin's "dangerous idea", although in a rather extreme interpretation, is considered as *the* solution to possibly one big problem of Science, how life evolved (Dennett 1995; ch.1). However, when this criticism is applied to bio-linguistic inquiries, it turns out to be relatively uncontroversial and motivated. As I discussed in this paper, it may actually offer quite powerful insights on why neo-darwinist theories of language and other mental processes, such as evolutionary psychology, but also offer precise constraints as to which theoretical approach, within Biological Approach theorizing, appears to have a sounder biological plausibility.

Overall, *What Darwin Got Wrong* presents two arguments which seem to be very important for the Biolinguistic Approach and one of its specific incarnations, the Minimalist Program. These two arguments enrich the debate on what constitutes *desiderata* for a biologically sound approach to language. In doing so, *What Darwin Got Wrong* appears useful in stressing out the importance of formulating bio-linguistic theories that are at the same time empirically adequate and theoretically sound, since such theories need to be entirely naturalistic. In doing so, the book spells out a very precise logical and empirical space in which linguistic, and more precisely minimalist theories must move, in order to be biologically plausible. It is of course an open question on whether a specific form of Minimalism can be easily defined, within this conceptual space. However, if the authors are correct in their assessment, a theory that is defined within this space will be a theory that meets a much sought, and yet elusive bio- and -logical validity. Hence, it will be a theory worth pursuing in detail.

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